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RESEARCH ON THE CENTRAL NERVOUS SYSTEM

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OUR knowledge of the external world is built up from the messages which are sent to the brain by the sense organs. Our action on the external world, our behaviour, depends on messages which pass down the motor nerves to the muscles. Our thoughts may or may not be completely determined by the activities which take place in the brain, but at least they are very greatly altered if the brain is damaged. In fact, our conscious life is so closely bound up with the central nervous system that we as persons must die when it dies although much of our bodies might easily be kept alive for hours or days longer. What is its structure, what happens in it and how are these happenings related to what we feel and think ? These are the ultimate problems of research on the central nervous system. There is no great danger that they, or the ultimate problems of any branch of Biology, will be fully answered in our time, but even in the next decade the problems of neurology may have yielded far enough to appear in an entirely different aspect. There are at least three lines of research for which one would prophesy continuous and fruitful advance. It is true that the greatest advances are usually unexpected, but it is a healthy sign that there should be no need to wait for the unexpected to happen.

A great deal is already known about the anatomy of the nervous system and the function of its various parts. For this the clinicians have been chiefly responsible. Disease or injury of the brain or spinal cord throws some part of the mechanism out of gear and this brings about a failure of certain of the normal activities of the organism : for example, a clot of blood next the central canal of the spinal cord may make one hand insensitive to pain or heat, a tumour pressing on the brain stem may cause attacks of giddiness, etc. If such accidents happen often enough and in great enough

variety it is not difficult to work out a plan of the central nervous system showing the main lines of communication for incoming and outgoing messages and the main controlling stations for different kinds of behaviour. Vascular disease, infection, tumour formation, and the accidents of civil life have supplied a vast mass of data and it needs only an occasional European war to fill in the gaps by planting shell fragments in parts of the brain not usually accessible to disease.

Evidence of this kind reveals the gross mechanism of the central nervous system, showing, for example, that there are ascending pathways in the spinal cord conveying sensory messages to the brain, or that there are regions in the brain stem concerned with balancing the body. It has been supplemented by experiments on animals, but the human subject can give far more information, for he can say what he feels and what he can and cannot do. It has been supplemented also by purely anatomical studies, of the adult central nervous system, of the development of the parts in foetal life, of the differences in structure in animals with different modes of behaviour. The ground plan is now fairly clear, though there are still many pathways to trace and much to be learnt by comparing one animal's brain with another's. The results are, naturally, of the first importance to clinical neurology and to any conception of the way in which the nervous system carries out its functions. But they relate mainly to the differences between the various parts, not to the properties common to them all. Thus they tell us a great deal about the arrangements in the brain stem, where so many diverse activities of the body are controlled, but not much about the cerebral hemispheres where the localisation of function is far less obvious.

Studies of its microscopic anatomy have given more information about the general properties of the central nervous system and the nature of the reactions which are likely to take place in it. The main outlines were drawn by the histologists in the latter half of the last century. The structure of the peripheral nerves, bundles of long, unbroken fibres, was known already but their relation to the structures in the grey matter was not. The new stains revealed this as a collection of cells woven into networks by interlacing filaments, or dendrites, and connected for long-range signalling by the axon or nerve fibre. Each nerve cell with its branching dendrites and its single axon is a structural and functional unit, or neurone, and there is no continuity between one such unit and another. That is the neurone theory, enunciated by Waldeyer but made evident by the work of the great Spanish histologist, lately

dead, Ramon y Cajal. The theory is taken for granted nowadays and the bitter controversies it aroused are forgotten, as is the stout defence put up by those who believed in continuity rather than contiguity—in fibrils continuous throughout the nervous system rather than in chains of discrete neurones. The dispute was not barren however. It led to an exhaustive survey of the minute structures in the central nervous system and so apart from any theoretical results it has had the practical one of enabling the neuro-pathologist to distinguish the many varieties of tumour which may invade the brain or spinal cord.

The neurone theory supplies the essential mechanism for a working model of the nervous system. The messages from the sense organs must be handed from one neurone to another by way of the branching dendrites and across the discontinuity which exists between the two cell units. In this region, where the dendrites are in contact, there is often a characteristic structure called the synapse, and there is reason to believe that it is this region which endows the central nervous system with its co-ordinating powers. It can be shown that a message suffers little or no distortion in passing along a nerve fibre, but that after passing several of these synaptic barriers it may be greatly altered : it may be combined with messages coming from elsewhere to the same destination and may suppress them or be suppressed by them. In fact, the opportunity for nervous integration seems to arise in the junctional regions between the neurones.

One method of learning what is likely to happen in the region of dendrites and synapses is to find out what happens in the nerve fibres. These have only to transmit messages rapidly and without distortion, but the reactions which enable them to do so are likely to be found in some degree in all parts of the neurone. This line of attack is of great interest at the moment because of recent successes and because its final aim is to describe the behaviour of the nerve fibres in terms of physics and chemistry. The general nature of the messages which they transmit is already known. All are made of a series of brief impulses, momentary outbursts of activity which travel down the fibres at speeds as high as 50 and as low as 0·5 metres a second ; the impulses in any one fibre are all alike, they are conducted at a fixed rate and are accompanied by characteristic electrical, thermal and chemical changes. These have been studied with all the refinements of modern technique and can be so studied because they appear with mechanical regularity.

A further series of clues has come from measurements of the electric stimulus needed to set up an impulse in a nerve fibre. By

varying the form of the stimulus, its intensity, duration, etc., it is possible to specify the essential features which it must possess if it is to be effective ; and from these it is possible to infer the nature of the changes which the current must bring about in the fibre in order to start an impulse in motion. The method is as old as du Bois Reymond and began to show quantitative precision thirty years ago, but in the past year a new order of accuracy has been reached by A. V. Hill and his collaborators. The equations which determine the stimulating current seem to need no further adjustment and are almost ready to be given a definite physical interpretation. For this, however, we must wait until the data obtained by this and by the other methods can be linked together to give a coherent picture of the impulse. The picture, when it comes, will describe a complex cell reaction in terms of molecular change and this will be its main achievement, but if it shows how an impulse passes down a nerve fibre it can scarcely fail to suggest what may happen in the regions where fibres begin or end.

Searching the peripheral nerves for clues to the central nervous system is a method which should perhaps have been mentioned last of all, since it is the most indirect. In any case it can only be considered as an adjunct to the direct method, the study of conduction in the reflex arc. This is the special field of Sir Charles Sherrington, in whose hands it has become the chief source of knowledge on all that relates to nervous co-ordination. The method is, briefly, to correlate ingoing and outgoing messages by sending a known series of signals into the spinal cord and observing the muscular contractions which result. The method is not as simple as it appears at first sight, for the whole tendency of the central nervous system is to defeat such analysis. Sherrington has stated the fact by naming his book *The Integrative Action of the Nervous System.* In normal life countless messages pass by every nerve root into the spinal cord, even though the body is at rest in a constant environment, and messages are constantly emerging to the different muscles, maintaining a posture of the body or altering it to meet some new factor in the environment or some change arising within the body or the limbs. It is not difficult to trace a connection between stimulus and response, but every time the stimulus occurs the response, in a normal animal, is likely to be different because it depends on the total pattern of incoming signals and not on a single element of that pattern. There are a few simple reactions, like blinking when the eye is touched, which occur almost invariably, but most of our behaviour is so organised by the central nervous system that every movement is affected

by and reacts upon the total response. It is this synthetic function which stands in the way of the analysis of behaviour into reflexes.

Sherrington overcomes this difficulty by simplifying both the incoming messages and the central apparatus which deals with them. The latter can be accomplished by isolating a portion of the spinal cord from the rest of the central nervous system. As the chief synthesis takes place in the brain, the reactions of the cord or of a few segments of it are much more easily followed when the connections with the brain are destroyed. The incoming messages are brought under control by cutting all but a few of the nerves and by the use of electric stimulation to set up the impulses. In this way it has been possible to isolate a number of simple reflexes, mostly movements of flexion and extension of the leg, and to study the way in which they interact.

The conditions are admittedly artificial, the simple reflex is admittedly an abstraction and the reflex arc, which one cannot help depicting as a single chain of neurones with synapses between, is even more so. Yet the reflex is an abstraction which has been of immense value to the progress of neurology, and there is no reason to suppose that the rules which govern conduction through the reflex arc are not valid for conduction through the intact nervous system. As compared with a nerve fibre a pathway through the spinal cord shows two main differences in behaviour. These are (a) that there is a less exact correspondence between stimulus and end effect or between incoming and outgoing signals, and (b) that an incoming signal may inhibit activity besides arousing it.

The different reflexes vary greatly in the degree of correspondence between what goes in and what comes out of the spinal cord. In some each impulse in the sensory nerve fibre, or rather each volley of impulses in the sensory nerve evokes a corresponding volley in the motor nerve. The synaptic regions impose a slight delay in conduction, but apart from this they might be replaced by so much nerve fibre. In other reflexes the connection is less straightforward. The passage of impulses into the cord may have no obvious effect for the first few seconds : then gradually more and more of the motor neurones begin to send out impulses to the muscle and this discharge continues for some time after the sensory inflow has ceased. In Sherrington's phrase such reflexes exhibit " inertia and momentum," a long latency and an after discharge.

From detailed studies along these lines it seems clear that an impulse passing out along the dendrites of one neurone to the synapses between it and another may set up a corresponding

impulse in the latter or may cause no more than a temporary rise in excitability. This rise can be increased by the arrival of more and more impulses at the synapses until eventually the neurone becomes active and sends out impulses on its own. As might be expected the time taken to build up the excitability to the overflow point depends on the concentration, temporal and spatial, of impulses in the synaptic region, and some neurones become active very soon whilst in others the excitability never rises above the threshold value.

The process by which impulses in one neurone can raise the level of excitability of another is evidently an important factor in nervous co-ordination. There is a similar process, inhibitory instead of excitatory, which can convert an active into a quiescent neurone. Here too the effect may appear in all degrees of intensity ; a motor discharge may be suppressed or diminished, or a motor neurone which is inactive but near the point of discharge may be rendered less excitable. How does a neurone produce those effects on its neighbour ? Both inhibition and excitation may be cumulative, outlasting the period of stimulation and arising and subsiding gradually. What kind of cumulative change is likely to occur in the synapses, or in the neurone beyond, when a series of impulses passes into the spinal cord ? An answer to these questions would give a new impetus to the physiology of the central nervous system. The whole pattern of response seems to depend on the balance of excitations and inhibitions developed in the synaptic regions by the arrival of impulses there, and we cannot go much further with the analysis until we know how these opposing effects are produced.

There have been two suggestions to account for the inertia and momentum of reflex conduction. One attributes central inhibition and excitation to substances produced in the synaptic regions and destroyed or dispersed after a variable period. Various drugs are known with specific excitatory or inhibitory effects, and if these were produced in small quantities whenever an impulse arrived at a synapse they might account for most of the phenomena. The other suggestion is more definite and on that account more easily assailable. It is known as the " delay-path " hypothesis and was first stated by Forbes in 1922. In its original form it attempted merely to account for the long after discharges which may follow a sensory stimulus, and it did so by pointing to the fact that there are likely to be very many routes through the spinal cord from a sensory to a motor neurone. Some of these will be longer than others, at any rate the time spent in conduction through them

may vary. Now whenever a nerve fibre branches, an impulse reaching the branch will become two impulses, one in either fork and both of undiminished intensity. Thus if between A and B there are a number of pathways of different length or different conduction time the arrival of a single impulse at A will lead to the subsequent arrival of a long series of impulses at B. The delay path hypothesis is clearly adequate to explain how a single very brief pulse of activity in one neurone can produce effects of long duration in another. It does not demand an impossibly extended central nervous system, for a delay of any magnitude might arise from the trapping of an impulse in a circular pathway. It has been advocated recently by Sherrington's collaborators at Oxford on the basis of experiments which seem hard to reconcile with the hypothesis of evanescent substances with a drug-like action.

The view that central excitation and inhibition result from substances with excitatory and inhibitory properties may seem not much more illuminating than the view of Molière's physician on the dormative powers of opium. But in the last few years it has become more and more certain that some of the excitations and inhibitions which occur outside the central nervous system are actually due to substances liberated by the neurones. The gradual accumulation of evidence has made one of the most interesting chapters of contemporary physiology. The prologue opens with the discovery of adrenalin by Schaefer and Oliver in 1894, the substance produced by the suprarenal glands and giving rise to all the effects which follow stimulation of sympathetic nerves. Much work followed on the general resemblances between the action of drugs and the action of the visceral nerves. Then in 1921 comes Loewi's discovery of a substance with inhibiting properties in the fluid from a heart inhibited by stimulating the vagus nerve. This " vagus substance " is identified with Dale's acetyl-choline and is isolated by Dale from various tissues. It becomes increasingly clear that the whole system of visceral nerves makes use of this " humoral transmission," for the evidence for acetyl-choline as the intermediary for the parasympathetic nerves is seen to be in close agreement with the parallel work of Cannon on the sympathetic system. Finally there is the publication and the confirmation of Kibjakow's experiment, showing that acetyl-choline is produced in the cervical sympathetic ganglion when the nerve fibres leading to it are stimulated.

This experiment opens a new chapter, for it suggests that acetyl-choline is not only responsible for the action of a nerve fibre on a muscle or gland cell, but also for the action of one neurone on

another. In the sympathetic ganglion the nerve fibres from the spinal cord end in synapses round nerve cells which send fibres to glands and muscle. It has been found that these nerve cells are excited by an injection of acetyl-choline into the ganglion, and there is evidence to show that acetyl-choline is actually produced when impulses reach the synaptic area. Its concentration must fall off rapidly after each period of secretion, for it is destroyed by an enzyme present in the tissue fluid. In fact, it seems that the transmission across the synapse could certainly be accomplished through the mediation of the acetyl-choline liberated there.

It is quite true that the same arguments could be used in favour of other kinds of transmission. For example, the nerve cells may be excited by an electric current as well as by an injection of acetyl-choline, and an impulse reaching a synapse produces electric currents as well as acetyl-choline. In the sympathetic ganglion there is little opportunity to decide whether the electric changes give a better or a worse explanation of the phenomena of trans-mission, but the transmission by acetyl-choline is certainly the more interesting hypothesis. Possibly both electric changes and the liberation of an exciting substance are concerned, for there is some evidence for a dual mechanism in transmission from nerve to muscle.

The chapter dealing with " humoral " transmission through the peripheral nerve ganglia is still unfinished, though fresh data accumulate every month. A chapter dealing with humoral trans-mission in the central nervous system has scarcely begun. There have been various attempts to isolate active substances, but it is not surprising that so far very little has come of it. If central transmission takes place in this way it is clear that the exciting or inhibiting substances must be rapidly destroyed, and cannot diffuse far away from the region where they are produced. The production of acetyl-choline at the parasympathetic nerve termina-tions can only be studied because the enzyme which normally destroys it can be put out of action. In the same way until we can prevent their destruction we are not likely to find the corre-sponding substances which may be produced in the brain or spinal cord.

In default of further evidence, therefore, the nature of the transmission from one neurone to another must be left in uncer-tainty. The two possibilities of conduction by delay paths and by chemical mediators are not mutually exclusive, but at the moment the former does not offer much prospect of advance, whereas with the latter there is always the chance that active substances may be detected. Possibly more information will come from simpler

types of nervous system. These can supply valuable data about the general mechanism of co-ordination, witness Pantin's analysis of the reactions of the sea-anemone, Bethe's studies of locomotion in insects and Prosser's of the passage of impulses through the central nerve cord of the crayfish. But our need is for a synapse so large and accessible that we could measure the changes occurring in it at different points, and this has yet to be found.

The problems of excitation and inhibition in the reflex arc are of the first importance, for Sherrington's analysis remains the starting-point for any treatment of nervous co-ordination. An advance may come from many lines, but meanwhile the attack on the central nervous system has been taken up from an entirely different quarter and data are accumulating which at least have the merit of novelty though their meaning may escape us. They refer not to the reactions of the spinal cord in the controlled conditions of reflex analysis, but to the normal activities of the brain and of the human brain at that. It has already been mentioned that an impulse passing down a nerve fibre is accompanied by an electrical change. Similar changes are found in the muscle fibres and it seems highly probable that the activity of muscle and nerve cannot take place, or at least cannot be transmitted throughout each fibre without the accompanying electrical disturbance. Such disturbances are often very small and very brief, but nowadays they can be amplified to almost any extent and the most rapid fluctuations can be recorded without much difficulty. For example, our information as to the signals entering or leaving the spinal cord has been mainly derived from records of the "action potentials" in the sensory and motor nerve fibres. Electric changes are constantly taking place in the central nervous system and there, presumably, as elsewhere they are an index of the activities involved in the excitation of cells and the passage of impulses to and fro. We might hope, therefore, that by making electrical records we should be able to find out what is happening in any part of the central nervous system at any moment.

In practice there are still many difficulties. One trouble arises from the fact that the electrode system is likely to be affected by the potential changes taking place over a fairly large area. Again, a single record can show very little about the process of nervous co-ordination : for this, simultaneous records must be made from several parts of the brain. But any record of potential changes at the surface of the cerebrum gives a remarkable picture of the amount of activity which goes on even under deep surgical anæsthesia. There is a continual rapid fluctuation of potential, measured,

it is true, in $\frac{1}{10}$ths or $\frac{1}{100}$ths of a millivolt, but definitely caused by the fluctuating activity of nerve cells in the cortex. This activity is presumably a spontaneous discharge from cells which never attain an equilibrium but are always either building up or discharging their stores of free energy. As might be expected, in the anæsthetised brain the activity shows little evidence of organisation and the only thing remarkable is that it should occur at all. What happens to it, then, if the cloud of anæsthesia is lifted?

Our information as to this is derived mainly from the human brain since it is far easier to direct the thoughts and feelings, and so the brain activities, of man along a desired channel than to direct those of an animal. There is, however, the difficulty of coming close enough to the brain surface. Electrodes can be placed on the cerebral cortex when this is exposed during an operation, and they can be brought very near it when an opening exists in the skull. But an intact skull interposes a layer of high electrical resistance between the brain and the recording electrodes and this makes it possible to detect only the larger fluctuations of potential. The discovery that something could be detected through the un-opened skull was made by Hans Berger eight years ago. He found that in human subjects of all ages it was possible to record a characteristic oscillation of potential between two points on the surface of the scalp, and he was able to prove that these oscillations were derived from the brain and not from an extra-cranial source. They are sinusoidal waves, occurring in groups with a frequency of about 10 a second and an amplitude varying from moment to moment and reaching as much as 0·1 millivolt. The chief condition for their appearance is that the eyes should be closed.

It was several years before Berger's observations came to be generally known and accepted by electro-physiologists. An initial scepticism was pardonable. It is by no means easy to make certain of the cerebral origin of potential changes appearing in a region where there are many possible sources outside the skull. Moreover the conclusion that Berger's waves came from the brain seemed very hard to reconcile with their lack of variety. It appeared from his evidence that in every part of the brain surface there must be a rhythmic activity with a frequency of ten a second. Various procedures which demand the most concentrated attention, or startle the subject by their unexpectedness, put a stop to the rhythm and so does the mere opening of the eyes to a normal scene; but if the eyes are closed all kinds of complicated mental activities can take place without affecting the waves. The oscillation, though never completely regular, is not made less regular

by such activities as speaking, performing skilled hand movements, listening to or remembering poetry, etc. It seemed most unlikely that there should be a uniform type of activity and a uniform frequency of discharge in all parts of the cortex during such operations.

We are still very far from a complete explanation of the Berger rhythm. It is now generally agreed that it comes from the brain, but there is reason to believe that the greater part of the effect is derived from the occipital lobes, the regions concerned with vision. The hypothesis advanced by Adrian and Matthews regards the waves as due to a rhythmic spontaneous discharge from nerve cells working at a low level of activity. Since the potential changes are appreciable through the skull a large number of the nerve cells must be working in unison. This can happen when a large area (*e.g.* that concerned with vision) is freed from the disturbing effect of messages from the sense organs (*e.g.* by shutting the eyes); it is unlikely to happen when the area is engaged in the normal activities of the cerebrum, for these involve different intensities of excitation and therefore different frequencies of discharge in the different neurones. An explanation on these lines differs from that of Berger and may try to give too simple an account of events which take place in a complex system. If it is correct the appearance of the Berger rhythm merely tells us that some part of the brain is undisturbed and so able to develop the synchronous beat.

It is improbable that the recording of electric changes through the skull can ever provide a survey of brain activity detailed enough to show the changing patterns which accompany such operations as reading or writing. This must wait for the fortunate occurrence of a patient with a large hole in the skull exposing an uninjured cerebral cortex. The physiology of digestion owes much to Alexis St. Martin for allowing Dr. Beaumont to observe the interior of his stomach through the opening left by a bullet wound; this branch of the physiology of the brain will soon be in need of a similar service, although it is scarcely ready for it at the moment. There is, however, the prospect of immediate advance in our knowledge of various pathological states of the brain, particularly those which lead to loss of consciousness or to convulsions.

The recording of electric changes is a relatively new method of research on the general nature of brain activities and its prospects are still uncertain. An older method of more certain value is the study of learning or habit formation reduced to its simplest terms, the study of Pavlov's " Conditioned Reflexes." Pavlov's analysis

of all behaviour into reflexes, unconditioned or conditioned, is too well known to need much description. It is a working hypothesis accounting for a certain range of facts and suggesting methods of investigation, and a great deal of its value lies in the original way in which the results are presented. Pavlov was bold enough to discard the customary ways of thought and to consider the process of learning without recourse to the conceptions of memory, desire, satisfaction, etc., which would normally be used. For him an animal did not learn to expect food when a bell was rung because it remembered what had happened during its training ; instead it salivated and wagged its tail because the training had established a conditioned reflex arc connecting the particular sensory message with the particular patterns for motor activity in the brain. The difference may seem to contrast scientific jargon with common sense, but it is the scientific conception which can be tested by experiment and modified and elaborated to become part or the whole scheme of the nervous system.

Since mankind has developed the technique of learning through so many generations it is not surprising that most of the rules were already known before Pavlov's work began : for instance, that we learn the better when there is some incentive in the shape of a reward or the avoidance of a punishment. But if we consider the process in Pavlov's terms and investigate it by his technique, it becomes possible to go much further, e.g. to make quantitative studies of the effects of the reward in cementing neural connection and of the rate of dissolution of these in its absence. There are many laboratories in Russia where Pavlov's ideas are being worked out and in America there is perhaps more criticism of some of the conclusions but no neglect of the method. A problem which is constantly studied is that of the nervous structures essential for learning. Is the cerebrum necessary ? If so, is any particular part of it necessary and are different parts necessary for learning different things ? Lashley's work on the rat has answered some of these questions and has redefined many others which arise from them. A parallel enquiry asks what nervous operations are essential for learning ? Is it necessary for the action to be carried through completely during each trial, or may certain parts of it be left out, e.g. as when we learn to recite a poem without ever speaking it aloud ? Some interesting work on this problem has been published recently by Gautt and his collaborators at Baltimore.

Throughout this review the problems and the methods have been described, but the results have been called interesting and not much more has been said about them. The fact is that the

most definite achievements of neurology have been in fields concerned with structure, pathology and the localisation of function. There is far less certainty in researches dealing with the general nature of conduction in the reflex arc or of the activities in the cerebral cortex, *i.e.* with the general principles which govern the nervous mechanism. Because of this the results seem to be less worth summarising than are the different methods of attack which have produced them. It may be that none of the present lines will lead very far, that a new outlook may be needed to solve the difficulty of analysing the working of a system in which every part co-operates so closely with every other. This, however, is not likely to be an insuperable difficulty. The components in a physico-chemical system may be inter-related in such a way that the smallest disturbance modifies every part of the system, but we can still derive a reasonable satisfaction from explanations which deal with one component at a time. In the same way it is still profitable, and will be for many years, to regard the central nervous system as made up of neurones, of reflex arcs, and of regions with different controlling and executive functions, although the reflex is an abstraction and the unit is not the neurone but the entire nervous system. Difficulties of this kind, in which the fact of vital organisation must be dealt with in terms of mechanics, are common enough in biology, and are no worse for the neurologist than for his colleagues. But his colleagues can avoid the greatest difficulty of all : they need take no official cognisance of the mind, whereas to the neurologist it remains in the background as something which falls within his province and yet is totally unmanageable by the conceptions which he has to employ.

PASSAGE OF ELECTRIC PARTICLES THROUGH MATTER

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THE extent to which α and β particles can penetrate matter, the ionisation they produce, and other accompanying effects such as the emission of radiation, are problems which have been extensively investigated since these particles were first discovered. An understanding of these effects requires in the first place a knowledge of the structure of matter. We must know the nature and motion of the ultimate constituent particles with which the moving α or β particle collides. The second requisite is a knowledge of the dynamics obeyed in such collisions. In the early work in this field classical mechanics was generally assumed to be applicable, and the main object of the investigations was to find out from the observed effects something about atomic structure. This stage came to an end with the establishment of Rutherford's nuclear theory of the atom, to which, of course, the study of collisions had essentially contributed. Since then the problem of the passage of electric particles through matter has virtually been a problem of mechanics, and it is the development and present position of this aspect of the subject that we shall concern ourselves with in this article.

We shall first discuss collisions in which the velocity of the incident particle is small compared with the velocity of light so that the problem is not complicated by " relativity " considerations. In this non-relativistic region the requirements of quantum-mechanics have been substantially borne out by experiment. We shall give an outline of the general nature of the quantum-mechanical treatment and also consider the status of the classical theory. For collisions involving velocities close to that of light the applicability of the present scheme of atomic mechanics is far less certain. Indeed, in the extreme relativistic region, represented by the high energy electrons of cosmic rays, whose velocities are within 0·01 per cent. or so of that of light, the failure of the theory is clearly

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established. This failure concerns the fundamentals of the theory and the solution of the problem may mean a development in mechanics comparable with the past developments represented by relativity theory and quantum-mechanics. We shall briefly review these points in the second part of this article.

1. NON-RELATIVISTIC REGION

Let us first consider the limitation of the classical theory. This may be done, as in other problems, by considering the classical treatment from the standpoint of the Uncertainty Principle.[1] This principle sets a natural limit to the definition with which the motion of a particle can be described in terms of space and time. In order, for example, that we may define the momentum of a particle with an accuracy δp we must relax the definition of the position of the particle to an extent of the order of $\hbar/\delta p$, where $\hbar$ is Planck's quantum of action, h, divided by 2π. An infinitely sharp definition of the quantities which appear in a classical calculation is thus not possible. We must recognise a certain " blurring " of the classical picture and the validity of classical mechanics is determined by the extent of this blurring. If it is small the classical theory is applicable, and constitutes a satisfactory approximation to the exact quantum-mechanical theory. On the other hand the blurring may completely destroy the classical picture and in that case the classical treatment is invalid and meaningless.

Let us apply these considerations to the central problem of collisions theory—the collision of two free electric particles. Let one (B) be initially stationary and the other (A) move with velocity v, and let their charges be Ze and ze respectively where e is the electronic charge. In actual atomic collisions the impact parameters, ρ, i.e. the least distances between the undisturbed paths of A and B, are randomly distributed, but in order to proceed with a classical calculation we specialise the conditions and consider first the effect of a collision with a given parameter. The classical procedure is then to integrate over all impact parameters. A small latitude $\delta\rho$ in ρ can of course be tolerated in the classical picture, the condition being that the classical effect should not vary appreciably for the variation $\delta\rho$ in ρ. This evidently requires $\delta\rho \ll \rho$. This means however that the momentum of B can only be defined with an accuracy $\delta p \sim \hbar/\delta\rho \gg \hbar/\rho$. Let us now consider the extent to which A is deflected by B (scattering). This deflec-

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[1] I should like to express here my indebtedness to Professor N. Bohr for the opportunities of discussions with him on this and other collision problems, during a recent stay at his Institute.

tion [1] will practically all take place when A is at a distance of the order of ρ from B. The time of effective interaction—which we may call the " time of collision "—is thus of the order ρ/v. The force acting on A during this time is of the order of Zze^2/ρ^2, so that the classical momentum transfer—which determines the deflection—is of the order of $(Zze^2/\rho^2) \times (\rho/v) = Zze^2/\rho v$. The considerations set forth above now mean that if a classical treatment of this kind is to be valid the effect concerned must only be a little " blurred " by the finiteness of the quantum of action, *i.e.*

$$\hbar/\rho \ll Zze^2/\rho v$$

or $\qquad Zze^2/\hbar v = (Zz/\beta)(e^2/\hbar c) \sim Zz/137\beta, = \gamma \text{ (say)}, \gg 1 \ . \quad (1)$

where β denotes v/c, c is the velocity of light and 137 is the numerical value of $\hbar c/e^2$. The scattering of α-particles ($\beta \sim 0.05$, $z = 2$) by heavy nuclei ($Z \sim 80$) gives $\gamma \sim 30$. The fact that this greatly exceeds unity means that the treatment of such scattering by the classical theory, as was done by Rutherford, is perfectly justified, and the classical formula necessarily represents the requirements of quantum-mechanics. Its excellent agreement with experiment is thus quite satisfactory.

Let us next consider the collision of two electrons, or of an electron and a proton, so that $z = Z = 1$. Let the incident particle be an electron of 20,000 volts energy—such as has been used in many experiments. The corresponding value of β is about 0·25 so that $\gamma \sim 1/0.25 \times 137 \sim 0.03$. This is much less than unity, and the classical treatment is therefore not at all an approximation to quantum-mechanics. This applies *a fortiori* to faster electrons. The picture of orbits in such cases is completely false.

The incident particle A, with its random impact parameter (but definite velocity), is represented in quantum-mechanics by plane de Broglie waves. The field of B scatters these waves in much the same way as a region of varying refractive index scatters light waves. This method of treating collisions is due to Born. The classical treatment is applicable under the condition $\gamma \gg 1$, because under this condition the scattering field is so strong that the incident wave train can be divided up into small wave-packets whose refraction along a classical path greatly exceeds the diffraction corresponding to their finite size. However if $\gamma \ll 1$ this is not so and the wave-packets become completely mixed owing to diffraction, and phase relations are then of paramount importance. Under these conditions it is imperative to maintain the representation of the incident particle by an infinite plane wave train,

[1] Assuming for simplicity that the deflection is small.

and the resultant scattering is essentially controlled by the nature of the interference between the secondary waves from widely different parts of the field. The problem is mathematically a wave problem, and it is only an analytical accident that the final solution is still represented by Rutherford's classical formula.

We now come to a very important matter, *viz.* that the formal correctness of the classical results as a representation of the requirements of quantum-mechanics for the collision of two electric particles with $\gamma \ll 1$ is *provisional*—it is true only if the two particles are different, and if they are both free. If the colliding particles are the same, or if one is bound as in the case of an atomic electron, classical and quantum mechanics lead to results which to a greater or less degree are different. It is by virtue of this difference that the falsity of the classical treatment under the conditions $\gamma \ll 1$ has been demonstrated experimentally, and the quantum-mechanical treatment shown to be correct.

Let us first consider the case of two like particles, say two electrons, one of which we shall suppose to be at rest before the collision. How can we say which electron after the collision was the one at rest before and which the moving one ? Because of the similarity of the two electrons it is impossible to do this without watching the progress of the collision and keeping an eye as it were on one or other of the electrons. Such observations would however disturb the electrons. If $\gamma \ll 1$ this disturbance exceeds the effect of their mutual interaction and therefore the only way of determining which electron is which after the collision is ruled out. The underlying principle of quantum-mechanics is that no more must be said about a phenomenon than could be experimentally ascertained if desired. In accordance with this the wave-function representing two colliding electrons must not imply a distinction between them if $\gamma \ll 1$. A wave-function which is symmetrical or anti-symmetrical in the co-ordinates of the two electrons satisfies this condition —but not the wave-function corresponding to Rutherford's scattering formula which is *unsymmetrical*.[1] The statistics of bound electrons

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[1] If c_1 and c_2 denote the co-ordinates of two electrons then a wave function $\psi(c_1, c_2)$ is symmetrical if its value is unchanged when the value of the co-ordinates c_1 and c_2 are interchanged, *i.e.* $\psi(c_1 = a, c_2 = b) = \psi(c_1 = b, c_2 = a)$. It is antisymmetrical if $\psi(c_1 = a, c_2 = b) = -\psi(c_1 = b, c_2 = a)$. In both cases $|\psi|$, which is the quantity with direct physical significance, is unchanged so that no observable distinction between the two electrons is implied. On the other hand an unsymmetrical function means that $|\psi(c_1 = a, c_2 = b)| \neq |\psi(c_1 = b, c_2 = a)|$ implying such a distinction. It might be mentioned that the co-ordinates c_1 and c_2 include the " spin " co-ordinates of the electrons as well as their space co-ordinates.

indicates that for two electrons we should use the antisymmetrical wave-function. This was first pointed out by Oppenheimer [1] and the considerations were subsequently developed by Mott [2]. Mott showed that the effect—usually referred to as the "Exchange" effect—reduces the probability of a collision in which the incident electron is scattered through 45° (for instance) to one-half the value previously given neglecting Exchange, *i.e.* to one-half the classical value. For two α-particles the symmetrical rather than the anti-symmetrical wave-function must be used, and in this case Mott has shown that if $\gamma \ll 1$, the scattering through 45° is raised to twice the classical value. The collision of two like particles is thus a phenomenon which provides a test of the supposed falsity of the classical treatment under the condition $\gamma \ll 1$. Experiments have been carried out for both the cases referred to. For two electrons the necessary results have been obtained by making observations on the "forking" of electron tracks in a Wilson cloud chamber [3]. The forking represents a collision of the incident electron with an atomic electron (which in such collisions is effectively free). One of the branches of the fork represents the incident electron, the other the "knocked" electron. The results obtained show that the number of these forked tracks (of a given energy) is appreciably less than the classical value, and agrees satisfactorily with the quantum-mechanical estimates of Mott. The collision of two α-particles has been investigated by observing the scattering of α-particles in helium [4, 5]. Here the observed scattering through large angles is considerably greater than the classical value, and again agrees with the quantum-mechanical value. These results provide convincing evidence of the inapplicability of the classical treatment when the quantity γ is small.

Let us next consider the other effect which we have mentioned as one in which, if $\gamma \ll 1$, classical theory and quantum-mechanics give different results, *viz.* the collision of an electron or α-particle with a bound atomic electron. This, of course, is the important problem of stopping-power, for in general it is in such collisions that a moving electric particle loses most of its energy as it traverses matter and through which it is eventually "stopped." Until the advent of quantum-mechanics the classical treatment of the problem by Bohr [6] was the only consistent way of dealing with it, the old quantum theory giving no indication of how to allow for the effect of the quantum of action in such non-periodic phenomena as collisions.

Though Bohr made use of classical mechanics his interpretation of the effect of the atomic binding forces is quite general. As in

the theory of the dispersion of light the atomic electron is considered as having a position of equilibrium in the atom to which it is attracted by a force proportional to the distance, thus giving it a certain natural period T, frequency $\nu = 1/T$. Just in the same way that such an electron will react to light of frequency much greater than ν as if it were free, so it will also behave as if it were free in collisions whose duration τ, $\sim \rho/v$, is small compared with T. If, however, ρ is so large that τ is appreciably greater than the natural period (adiabatic conditions), the binding forces reduce the energy loss to a very small fraction of the energy lost to a free electron, and the contribution of such collisions to the stopping power may be shown to be negligible. The whole energy loss thus takes place in the " free " collisions with

$$\rho < \rho' \sim v/\nu \quad . \quad . \quad . \quad . \quad (2)$$

This result is subject to the condition that ρ' be large compared with the size of the undisturbed orbit of the electron in the atom. Since the latter is of the order of u/ν, where u represents the orbital velocity of the electron, the condition may be written

$$v \gg u \quad . \quad . \quad . \quad . \quad . \quad (3)$$

This condition is generally satisfied by β-particles, and also by α-particles if the matter traversed does not contain heavy elements. The condition ensures that when $\rho \gg \rho'$ the perturbing force is uniform over the atom and is very weak compared with the atomic binding forces.

The important fact regarding the cutting off at $\rho \sim \rho'$ is that if $\gamma \ll 1$ it reduces the quantum-mechanical energy loss to a greater extent than the classical energy loss. The classical energy loss per cm. due to the collisions with $\rho < \rho'$ is given by Bohr's well-known formula, viz.

$$dT/dx = \frac{4\pi z^2 e^4 n}{m v^2} \log \frac{1 \cdot 123 m v^3}{2\pi e^2 \nu} \quad . \quad . \quad (4)$$

the argument of the log term being approximately ρ'/ρ'', where ρ'' is the classical distance of approach to give an energy transfer of the order of mv^2.[1] In this formula n represents the number of atomic electrons per unit volume and ν their natural frequency (or more precisely the geometric mean of their natural frequencies since

[1] We have actually substituted for ρ'/ρ'' its exact value as derived by Bohr. There is of course no sharp transition at $\rho = \rho'$ from free collisions to adiabatic collisions, but ρ' has an exact *effective* value equal to $1 \cdot 123 v/2\pi\nu$. The exact value of ρ'' is $(ze^2/mv^2)(1 + m/M)$ where m = electronic mass, M = mass of incident particle. For electrons and α-particles $\rho'' = 2e^2/mv^2$ and this is the value used in (4).

even a single atomic electron has a multitude of natural frequencies).
The quantum-mechanical formula for the energy loss per cm. under
the condition $\gamma \ll 1$ is

$$dT/dx = \frac{4\pi z^2 e^4 n}{mv^2}\log g\,\frac{mv^2}{h\nu} \qquad . \qquad . \qquad (5)$$

the argument of the log term being approximately $\rho' \div \hbar/mv$.
This formula was first derived by Bethe [7] using Born's method,
to which we have already referred. The numerical factor, g, is
2 for α-particles and $\sqrt{\varepsilon/8}$ for electrons.

A comparison of these two formulæ with experiment is given in
the table below, for α and β-particles traversing hydrogen [8]—
which provides the best test case since the evaluation of the require-
ments of theory for hydrogen is practically free from computational
uncertainty. The comparison is made in terms of the ranges of
the particles.

Moving Particle.	Velocity (cm./sec.)	Observed Range at Normal Pressure and 15° C (cm.).	Range according to		γ
			Classical Mechanics.	Quantum Mechanics.	
Electron . . .	$4\cdot0 \times 10^9$	$0\cdot37$	$0\cdot22$	$0\cdot34$	$\Big\}\sim 0\cdot05$
,, . . .	$5\cdot1 \times 10^9$	$0\cdot76$	$0\cdot49$	$0\cdot77$	
α-particle . . .	$(2\cdot54 \rightarrow 1\cdot082) \times 10^9$	$35\cdot1$	30	$35\cdot2$	$\sim 0\cdot3$

For α-particles it will be noticed that there is not much differ-
ence between the two theoretical values, but even in this case the
results are fairly decisive in favour of the quantum-mechanical
formula. For electrons the observed range is, however, more than
50 per cent. greater than the classical range and it was in fact these
results for electrons traversing hydrogen—obtained by the Wilson
cloud method about 15 years after the derivation of the classical
formula—that first showed clearly the limitations of the classical
theory of stopping power. We see, however, that the quantum-
mechanical result is again in good agreement with the observed
values, the difference of a few per cent. being within the error of
experiment. This reproduction of the observed rate of loss of
energy when the quantity γ is less than unity is a signal success for
quantum-mechanics in collision phenomena. The actual values of
γ in the cases considered are given in the last column of the table,
being much less than unity for the electrons, though not so much
so for α-particles. This is why the theoretical values are much
closer for α-particles, the formulæ (4) and (5) actually merging
together as γ approaches unity.

In order to consider what happens to the atomic electron in distant collisions in which the moving electron or α-particle passes *outside* the atom, the concept of impact parameter can still be used even if $\gamma \ll 1$. Moreover, the average quantum-mechanical energy loss in a distant collision is equal to the classical energy transfer, Q_{cl} [9, 10]. Under the condition $\gamma \ll 1$, Q_{cl} is however much less than the ionisation potential of the atom, and while the distant collisions contribute half the total energy loss represented by (5) they do not, in a purely classical theory, give rise to any ionisation at all. On the other hand in quantum-mechanics all the energy loss takes place in excitation or ionisation. As a result there is a much greater difference between the frequency of ionisation on the two theories than there is between the average energy loss per cm., the number of ions produced by the moving particle per cm. of its path being considerably greater according to quantum-mechanical theory than in a purely classical theory.

For fast electrons the actual frequency of ionisation is so small that the droplets which the ions give rise to in a Wilson cloud experiment are well resolved and the number of ions produced by the direct action of the electron (primary ions) may be determined by counting the droplets. A check on the theoretical distributions of the energy loss between different collisions may thus be obtained. For electrons of velocity 1.5×10^{10} cm./sec. traversing hydrogen (N.T.P.) the observed primary ionisation is 15 per/cm. [11]. The theoretical value according to classical mechanics (Thomson's formula) is 3.5/cm. According to quantum-mechanics (Bethe's formula [7]) it is 13/cm., which agrees well with the observed value. This result supplements those for the energy loss in showing that quantum-mechanics accounts satisfactorily for the distribution as well as the absolute value of the energy loss by a moving particle. Indeed, these results and those for the collisions of like particles substantiate all the basic principles involved in the non-relativistic theory of collisions.

It might be pointed out that the possibility of a quantitative test of the theory in the collision phenomena we have considered is essentially dependent on the incident particle being fast enough to satisfy the condition (3), *viz.* $v \gg u$. Considerable work has of course been done in the region $v \ll u$, and this has provided very interesting qualitative confirmation of the " wave " characteristics of quantum-mechanics. A discussion of this region is however outside the scope of the present article. We shall now proceed to consider collision effects for still faster particles than those already

dealt with, with velocities not only satisfying (3) but comparable with that of light, so that relativity effect comes in.

2. RELATIVISTIC REGION

In certain collision effects it is justifiable to regard one of the colliding particles as the "perturbing" particle, the other as the "perturbed" particle. When such a distinction is possible the treatment of the relativity effect is considerably simplified. The essential condition which characterises such collisions is that the reaction on the particle which we choose to call the perturbing particle is small. We shall consider two collision effects of this type, both of which have already been investigated in experiments on the high energy electrons of cosmic rays. These are the frequency of production of ions (primary ionisation) and the emission of radiation in nuclear collisions (X-ray effect). In the ionisation problem the perturbed particle is the electron in the atom to be ionised, the perturbing particle is the high energy ionising electron. The condition for the distinction is satisfied because the energy transfer in practically all ionising collisions is only of the order of the ionisation potential of the outer atomic electrons (10–50 volts). This is negligible compared with the energy of the ionising electron which, since $v \sim c$, is at least of the order of 10^6 volts. The reaction on the latter is therefore negligible, and it may be assumed to move with uniform velocity during the collision. In the radiative effect the high energy incident electron emits radiation because of the acceleration it suffers in the field of an atomic nucleus. The perturbed particle is accordingly the moving electron, the perturbing particle the atomic nucleus, and because of the large mass of the latter it is practically unaffected by the collision. In a system S' in which the electron is initially at rest, and the nucleus the "moving" particle, the nucleus may be assumed to move with uniform velocity during the collision.

Let us now consider the ionisation problem in a little greater detail. Owing to the small energy transfer in ionising collisions the velocity of the perturbed atomic electron after the collision as well as before is negligible compared with that of light. Its behaviour during the collision can therefore be dealt with by non-relativistic quantum-mechanics—the calculation being just the same as in the ionisation by slow electrons. Relativity theory enters only in the expression for the perturbing field due to the high-energy electron. The latter as we have shown can be assumed to move with uniform velocity during the collision, and the effect of relativity is just the usual Lorentz-Fitzgerald contraction of a

moving system. The scale of variation of the field parallel to the direction of motion is contracted by a factor, ξ, $\sim (1 - v^2/c^2)^{-\frac{1}{2}}$. This contraction of the field means that the time of collision for a *given* impact parameter ρ is reduced by the factor ξ. The impact parameter ρ' (eqn. 2, see also eqn. 4) for which the time of collision is of the order of the natural period of the atomic electron is therefore *increased* by the same factor. This increases the energy loss and the ionisation, and the relativistic formula for the primary ionisation is obtained in a simple way by adding the effect of this increase in ρ' to the ionisation given by the non-relativistic formula [9]. This gives

$$I = G(2\pi e^4 n/mv^2 J)[\log (gmc^2/J) + \log (v^2/c^2)$$
$$+ \log (1 - v^2/c^2)^{-1} - v^2/c^2] \quad . \quad . \quad . \quad (6)$$

(J = ionisation potential. For hydrogen atoms, $G = 0.28$, $g = 42$.) The relativity correction terms are in the second line of this equation. The expression inside the square brackets increases with v.
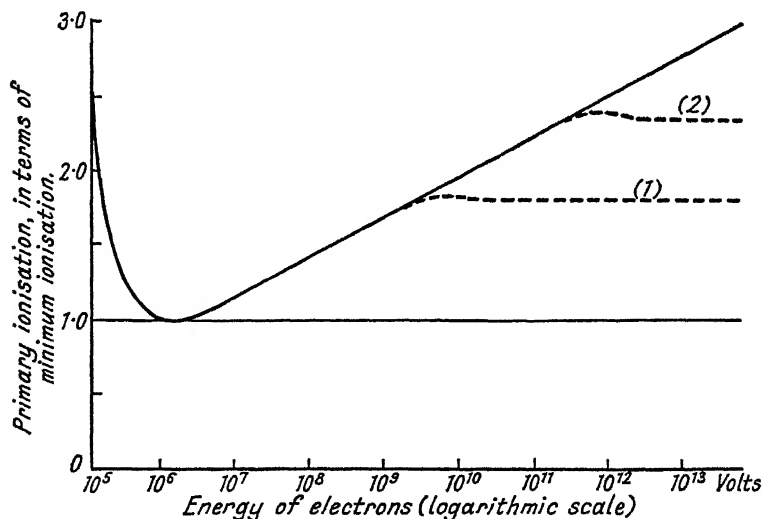
Fig. 1.—Theoretical primary ionisation (by electrons). Broken curves represent modified ionisation corresponding to hypothesis mentioned on page 26.

At $v/c \sim 0.97$ this increase just balances the decrease due to v^{-2} outside, so that the theoretical ionisation passes through a minimum at this velocity (see Fig. 1).

Observations have been made by means of the Wilson cloud method on the primary ionisation produced in various gases by electrons from radioactive sources, representing energies up to about 1 million volts ($v/c \sim 0.96$) [11]. The results are in general

conformity with formula (6) and indicate in particular the necessity for the relativity terms. The energies available from radioactive sources are, however, insufficient to test the rise in the ionisation on the high velocity side of the minimum. The discovery by Skobelzyn [12] that there occasionally traverses the atmosphere electrons with much greater energy than those emitted in radio-active processes has, however, made such a test possible. These high energy electrons belong to " cosmic " radiation, and the investigations of Anderson [14], Kunze [15], and Blackett and Brode [13], have shown that the spectrum of their energies extends up to at least 10^9 volts and possibly beyond 10^{10} volts. For electrons of this energy the theoretical ionisation required by (6) is appreciably greater than the minimum. The observations which have so far been made on the ionisation by cosmic-ray particles *reveal no increase above the minimum*. Before considering the implications of the failure of the theory thus indicated—a thorough experimental investigation of the effect has not yet been made—let us first consider the position regarding the radiative effect.

We require the radiation emitted by an electron of given initial velocity, v, due to its acceleration in a collision with an atomic nucleus. Consider the collision in the system S' in which the electron is initially at rest. In this system the nucleus moves with velocity v, and the perturbing field is of the same type as in the ionisation problem. The emission of radiation by the electron due to its exposure to this field may be compared to the emission of secondary radiation by an electron traversed by radiation (scattering). In fact, the emitted radiation in S' may be calculated by resolving the field of the nucleus into harmonic components and applying to each component the relativistic scattering formula derived by Klein and Nishina for homogeneous radiation [16, 17]. A simple transformation then gives the emitted radiation in the actual system in which the nucleus is at rest. For very high energy electrons the final formula for the rate of loss of energy (allowing for the shielding of the nuclear field by the atomic electrons) is

$$(dT/dx)_{Rad.} = (8\pi e^6 Z^2 n \xi / \hbar m c^3) \log (g \hbar c / e^2 Z^{\frac{1}{3}}), \quad g \sim 1 \quad . \quad (7)$$

This formula was first given by Bethe and Heitler [18], and was derived by them by considering the emission of radiation as due to transitions between stationary states. The striking feature of the theoretical result is that for the energies under consideration the loss of energy through the emission of radiation exceeds that due to the ordinary " mechanical " collisions with the extra-nuclear electrons—a state of affairs first pointed out by Heitler [19]. For

electrons of energy up to a million volts or so the situation is of course just the reverse, as is evidenced by the low efficiency of X-ray tubes, only a small fraction of the energy of the electrons incident on the anticathode reappearing as X-rays, even for anti-cathodes consisting of heavy elements.

By experiments on cosmic-ray electrons Anderson [21] has been able to determine the actual energy lost by electrons of high energy. His results clearly show that there is a process of energy loss in operation in addition to the ordinary processes of excitation and ionisation. There is little doubt that the " excess " effect is due to the emission of radiation. This is indicated in particular by the very large fluctuations in the observed energy loss.

A quantitative study of the actual radiative energy loss however fails to bear out the theory. From Anderson's results it appears that for electrons of energy of the order of 1–2×10^8 volts the coefficient of the theoretical radiative formula is about twice too large. Moreover, the failure would appear to be much more pronounced for higher energies. For 10^9 volt electrons the theoretical emission of radiation is so intense that appreciable tertiary effects from the secondary radiation would be expected. The almost complete absence of these effects indicates that at 10^9 volts the actual emission of radiation is of the order of only 1 per cent. of the theoretical value.

Let us now consider the theoretical implications of these results. In the first place it is certain that if the discrepancies between existing formulæ and experiment are of the order at present indicated they cannot be due to any approximations made in the analysis. They must be attributed to a breakdown somewhere in the basis of the theory. In the two effects we have discussed the calculations are based on [16a] :—

(a) the relativistic expression for the electromagnetic field of an electric particle moving with uniform velocity (at distances from the particle very large compared with its classical radius) ;

(b) the Lorentz expression, $F = (E + [H.v])e$, for the pondero-motive force on an electron in a given field ;

(c) the quantum-mechanics of an electron in a given field of force.

There is no known reason for doubting the validity of (a). It follows directly from the coulombian expression for the field of a stationary charged particle and Lorentz's equations of transformation. The error must therefore lie in (b) or (c). A closer examination of the phenomena concerned, based on a Fourier analysis of the perturbing field, has led to the result that the error must be associated with the fact that (b) and (c) allow the Fourier com-

ponents in such an analysis to be treated independently, and this in turn is to be associated with the linearity of the Lorentz expression for the force.[1] Before considering the possible conditions under which a failure of the Lorentz expression may take place we might point out that (in contradiction to what is sometimes supposed) the breakdown of the theoretical formulæ for electrons with energy of the order of 10^8 volts or greater has nothing to do with the fact that the de Broglie wavelength, $\lambda \sim h/mc\xi$, of such electrons is of the order of or less than the classical electron radius $\sigma \sim e^2/mc^2$. The analysis of the conditions which we have outlined here shows that the magnitude of the ration λ/σ for the moving electron is quite irrelevant. This effect would only come in if the *perturbed* electron, in a system in which it is initially at rest, acquired a velocity which gave it a de Broglie wavelength of the order of σ. This is far from being the case in both the phenomena under consideration.

Two hypotheses have been put forward regarding the limitation of the Lorentz expression, the first that it fails if the field acting on the electron (in the system S' in which it is initially at rest) varies appreciably over distances of the order of the electron radius (e^2/mc^2), the second that it fails if the field strength in S' exceeds e^2/b^2 where $b^2 = (e^2/mc^2)(\hbar/mc)$ [20]. If we suppose that collisions in which these conditions exist give no effect at all, then we find, for the radiative effect, a departure from existing theory of roughly the type observed. Neither hypothesis however can be made to account for the absence of the rise in the theoretical ionisation on the high-energy side of the minimum in Fig. 1, the conditions postulated being absent in practically all ionising collisions until very high energies are reached. The modified theoretical curves are represented in Fig. 1 by the broken curves (1) and (2). Indeed, the absence of a rise if it were proved would compel us to suppose that a pulse travelling with nearly the velocity of light produces no effect if it varies appreciably even over *atomic* dimensions.

The situation regarding the behaviour of high energy electrons is thus not at all favourable to existing views. We have considered only two phenomena—the emission of radiation and ionisation. The results which have been obtained for other phenomena throws hardly any light on the situation, and at present would indeed only seem to make a solution more difficult. For instance, the scattering of high energy electrons, observed by Anderson [21], does *not*

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[1] The linearity of the quantum equations of motion is also involved, but for the present purpose it is safe to regard the error as residing effectively in the Lorentz expression.

appear to fall short of the theoretical requirements. This scattering, like the emission of radiation, takes place in nuclear collisions, and no hypothesis has yet been put forward which would reduce the latter (as is required by experiment) without also reducing the former. Another effect observed by Anderson [22] is the production of fast secondary electrons ($\sim 10^7$ volts) by the very high energy electrons of cosmic radiation ($\sim 10^9$ volts). The theoretical treatment of the effect, due to Möller [23], involves more tentative theory than that which enters into the calculation of the ionisation and emission of radiation.[1] Even so Anderson finds that in *this* case the observed effect is in good agreement with theory ! There is also the phenomenon, peculiar to cosmic rays, of the production of showers—or more accurately of shower-producing radiation. So far no explanation has been given of this effect on the basis of the existing scheme of quantum-mechanics. A feature of the phenomenon which would appear to the writer to go against conservation laws is the large angular spread of the shower electrons. For unless a slow-moving heavy particle plays an active part in the phenomenon the shower electrons (apart from the small scattering they suffer) should all travel in practically the same direction since they have velocities very close to that of light and therefore practically the same ratio of energy to momentum. Finally, we would mention the effects recently reported by Skobelzyn and Mme. Stepanowa [24] for electrons of energy round two million volts—much less energetic than cosmic ray electrons but still well in the relativistic region. They find that such electrons produce pairs of positive and negative electrons with a prolificacy which cannot possibly be accounted for by existing theory. The single scattering of such electrons through large angles and the sudden loss by the electron of nearly all its energy are also found to occur much oftener than we should expect. It would indeed appear, if these results are correct, that collisions in the purely non-relativistic region discussed in the first part of this article are the only ones to which existing theory can be safely applied ! Further experiments on collision effects for high energy electrons are in progress in a number of laboratories. The results will be awaited with interest, for if they confirm the general trend of those already obtained and which have been quoted in this article, then the necessary modifications in theory may affect features which are at present hard to think of as doubtful.

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[1] The distance of approach of the colliding electrons is also relatively small so that the agreement with theory, unless accidental, introduces serious difficulties for any such hypotheses as the ones we have mentioned as possible causes of the failure of the radiative formula.

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STEREOCHEMISTRY OF CARBON COMPOUNDS

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As van't Hoff stated, in 1875 (*La Chimie dans l'Espace*, p. 89), one of the consequences of his tetrahedral carbon atom was that allene derivatives of type (I)

(I)
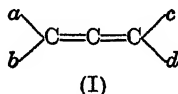
should be capable of exhibiting optical activity. Actually, the simpler type (II)

(II)
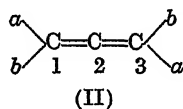
falls into the same category. Representation (III) of this molecule using tetrahedra

(III)
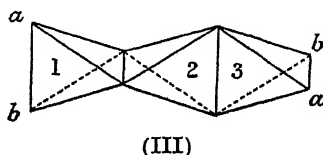
shows that if the edge of C_1 corresponding to groups a and b is in the plane of the paper, the edge at which C_1 and C_2 are combined must be at right angles to the paper. That between C_2 and C_3 is in the plane of the paper and therefore the edge of C_3 corresponding to the second pair of groups a and b is at right angles to the paper. Hence, although each pair of carbon atoms (C_1 and C_2; C_2 and C_3) possesses, as it were, a plane of symmetry, the three-carbon molecule has none, since, for example, the plane of the paper includes a and b attached to C_1 but not a and b attached to C_3. Were these groups identical ($a = b$), the plane of the paper

29

would be a plane of symmetry, because it would fall intermediately between two equidistant and identical groups. In other words, that type of substitution which leads to *cis-trans* isomerism in derivatives of ethylene should lead to mirror-image isomerism in those of allene.

The realisation of the consequences of a piece of stereochemical reasoning is, however, seldom a simple matter, and the establishing of the accuracy of van't Hoff's conception of the configuration of the allene type of molecule has proved particularly difficult.

Dimroth and Feuchter (*Ber.*, 1903, **36**, 2238) made the first attempt at solving the problem, but they, having shown that previous work on allene syntheses was not very trustworthy, themselves failed even to prepare the allene acid they regarded as suitable material for investigation. Lapworth and Wechsler (*J. Chem. Soc.*, 1910, **97**, 38) prepared the diphenyl-α-naphthylallenecarboxylic acid (IV)

$$\underset{C_6H_5}{\overset{C_{10}H_7}{\diagdown}}C=C=C\underset{CO_2H}{\overset{C_6H_5}{\diagup}},$$
$$\text{(IV)}$$
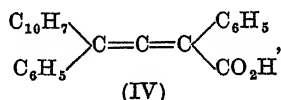
but failed to effect an optical resolution after trial of eight alkaloids, of aminocamphor and of menthylamine as resolving agents. They also tried two less usual methods of resolution with similar results. Hurd and Webb (*J. Amer. Chem. Soc.*, 1927, **49**, 546) made certain experiments on synthetic methods and similar investigations were carried out by Faltis and Pirsch (*Ber.*, 1927, **60**, 1621), Faltis, Pirsch and Bermann (*Ber.*, 1930, **63**, 691) and Ziegler and Sauermilch (*Ber.*, 1930, **63**, 1851). No evidence was obtained that allene compounds could be made to exhibit optical activity, but much information was gained as to the best way of synthesising allene derivatives. It was established that one of the most reliable methods of preparing allenes was by dehydrating the corresponding allyl alcohols :

$$R_2C{=}CH{-}CR_2OH \rightarrow R_2C{=}C{=}CR_2.$$
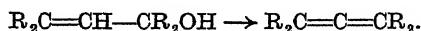
Maitland and Mills carried out the dehydration of diphenyldi-α-naphthylallyl alcohol using the asymmetric dehydrating agents, *d*- or *l*- camphorsulphonic acid :

$$\underset{C_{10}H_7}{\overset{C_6H_5}{\diagdown}}C=CH-C\underset{C_6H_5}{\overset{C_{10}H_7}{\diagup}}\ \rightarrow\ \underset{C_{10}H_7}{\overset{C_6H_5}{\diagdown}}C=C=C\underset{C_6H_5}{\overset{C_{10}H_7}{\diagup}}$$
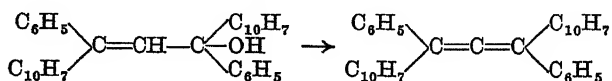
and last year reported (*Nature*, 1935, **135**, 994) that, although most of the diphenyldinaphthylallene formed was inactive, a small

excess of the *d*- or the *l*- allene was formed, according as the *d*- or *l*- dehydrating agent was used. In due course this enabled these authors to isolate the two pure antimers, which were not only optically stable but possessed the unusually high specific rotations, $[\alpha]_{5461} + 437°$ and $- 438°$, in benzene solution.

This result establishes conclusively the correctness of the tetrahedral hypothesis as applied to allene, and provides yet another example of the finality which is impressed on the mind by a sound piece of stereochemistry. It is a great pity that the decisive methods of the stereochemist cannot be used to settle other difficulties, for example, the real meaning of the term " triple bond."

Maitland and Mills (*Chemical Society Discussion*, January 16, 1936 ; see *Nature*, 1936, **137**, 542) showed that van't Hoff's allene configuration fits in with wave-mechanics. The central carbon atom in the three-carbon allene molecule is joined to each of the two outer carbon atoms by a Heitler-London single bond. The remaining two electrons (electron-clouds) of the central carbon atom function in two planes at right angles to each other, each being in resonance with an electron of one of the outer carbon atoms, this giving allene the propeller-blade configuration we know it has. In this connexion it is interesting to note that Wierl, by his electron-diffraction method, showed that both the $C=C$ bonds in allene itself are of the same length $(1\cdot32 \pm 0\cdot05\text{Å})$ as the $C=C$ bond in ethylene.

It very often happens that when the solution of a problem has been outstanding for a long time, more than one school of research reaches it at almost the same instant. Shortly after Maitland and Mills had published their note to *Nature*, the figures given in which showed that they had completed their work, Kohler, Walker and Tishler (*J. Amer. Chem. Soc.*, 1935, **57**, 1743) announced the optical resolution of the carboxymethyl ester (V)

$$\begin{array}{c} C_6H_5 \\ \diagdown \\ \diagup \\ C_{10}H_7 \end{array} C{=}C{=}C \begin{array}{c} \diagup C_6H_5 \\ \\ \diagdown CO\cdot O\cdot CH_2\cdot CO_2H \end{array}$$

(V)
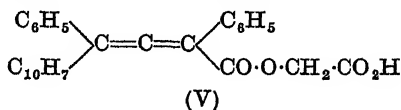
of Lapworth and Wechsler's original acid. The ester gave two brucine salts, separable owing to their different solubilities, and from the salts were obtained the two antipodal forms with $[\alpha]_D + 29\cdot5°$ and $- 28\cdot4°$.

The solving of the allene question will without doubt attract attention to allied problems, most of which remain to be stated, but many of which are self-evident when one considers the frequency with which the valencies of elements adopt a tetrahedral disposition,

in keeping with the octet-rule, or with its stereochemical counter-part, the 4-electron-pair rule.

When a carbon atom is of the type C *abcd*, that is, combined with four different atoms or groups, it is an " asymmetric " carbon atom (VI), and it is clear that van't Hoff's tetrahedron is not a regular one but is irregular. Irregularity can be of two kinds. Firstly, the distances C—*a*, C—*b*, C—*c* and C—*d* (the distances between the atomic centres of the various joined pairs) may be unequal, and, secondly, the angles *aCb*, *bCc*, *cCd*, *dCa* may be all different, although averaging 109·5°, the angle at the centre of the regular tetrahedron. Before the advent of the Lewis-Langmuir theory, Ingold and Thorpe had recognised that, apart from questions of optical activity, inequality of the above angles was a possibility

(VI)
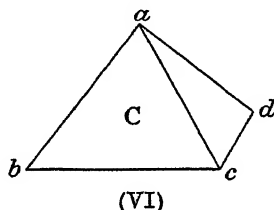
which could be brought about by suitable manipulation of group-property. In illustration, one may cite the great stability of the 4-carbon ring in pinic acid

$$CH_2\text{————}CH\cdot CO_2H$$
$$CO_2H\cdot CH_2\cdot CH\text{————}CMe_2$$

(VII)
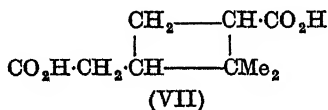
which appears to be due to the fact that the two methyl groups attached by two valencies to a carbon atom are able to make the other two valencies adopt an angle considerably less than 109·5°, so that the 90° angle of the 4-ring is made with less strain than would be required had CH_2 been present in the place of CMe_2.

Now *a*, *b*, *c* and *d* are each joined to the central carbon atom by a pair of electrons. As we pass from Group IV of the Periodic classification to Groups V and VI, we pass, for example, from C*abcd* to N*abc* and O*ab*, but the outer shell of the central atom remains an octet : the central atoms will therefore probably have their 4 electron-pairs, e_1, e_2, e_3 and e_4, tetrahedrally distributed, and an Ingold-Thorpe effect as between one angle e_1Ce_2 and another, e_3Ce_4 is conceivable. In fact, it is now known that the 4 electron-pairs associated with the oxygen atom adopt approximately a tetrahedral arrangement, and that the angle at the oxygen atom

varies from one compound to another, *e.g.* from 105° in water to 111° in dimethyl ether and 125° or more in diaryl ethers. In any event it is clear that atoms capable only of restricted combination will have a preference for certain characteristic valency angles and will be able to oppose attempts to alter the latter in the same way that carbon is known to oppose attempts to strain its valency angles very far away from 109·5°. One of the most interesting consequences of the persistence of valency direction has come to light in the last few years.

Bergmann and Tschudnowsky (*Ber.*, 1932, **65**, 457) found that thianthren (VIII) had a dipole moment of 1·68 *D*, and were therefore

(VIII)
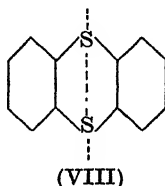
bound to conclude that the molecule was not flat, since only by some folding process could the necessary moment be set up, and it appeared that the only conceivable folding was about the line joining the centres of the two sulphur atoms. This view was substantiated by Bennett and Glasstone (*J. Chem. Soc.*, 1934, 128), who obtained the value 1·5 *D* for the moment. Clearly, the folded configuration of thianthren is a consequence of the tendency of the sulphur atoms to retain their characteristic valency angles. It does not follow, however, that the molecule cannot as it were " flap its wings," and this it actually appears to be able to do, for Baw, Bennett and Dearns (*J. Chem. Soc.*, 1934, 680) could obtain no evidence of the existence of more than two of the three disulphoxides which should exist if the folded configuration of thianthren persists in the oxidation-products (IX, X and XI) :

(IX)   (X)   (XI)
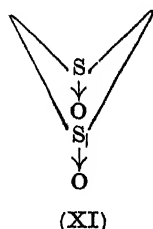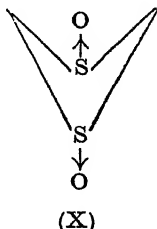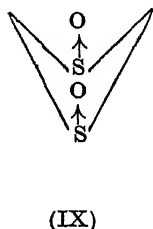
The failure of these authors to detect the third oxide demanded by the folded configuration may possibly be connected with the

fact that when the sulphur atom is oxidised to SO it becomes positive and therefore becomes smaller, as the charge tightens up the control of the outer electrons.

Since the molecule of thianthren is folded, it is evident that a considerable number of similar molecules will also be folded. A general expression for such molecules is XII (XIIa and XIIb)

(XII)   (XIIa)   (XIIb)
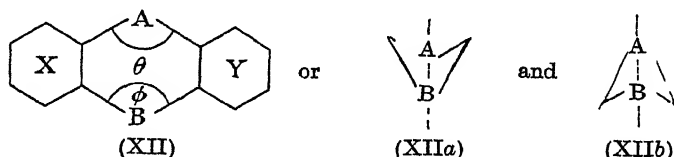
The only cases about which information as to dipole moment is available are those in which A=B=S (thianthren) and A=B=O (diphenylene dioxide). Diphenylene dioxide has no measurable moment (Bennett, Earp and Glasstone, *J. Chem. Soc.*, 1934, 1179), and therefore the molecule is not folded. The oxygen atoms in the flat molecule can have a valency angle, 120°, which approximates to the normal one. It will be interesting to learn whether selanthren (A=B=Se) is folded or flat.

When A and B are not identical, it is still possible to have flat or folded molecules, but it must be remembered that the centres of both A and B must both lie in the same plane as rings X and Y. Lesslie and Turner (*J. Chem. Soc.*, 1934, 1170) took the view that when A and B differed considerably in size, and the molecule was folded, there would be less tendency for the " wings " to flap, and they therefore prepared 10-methylphenoxarsine-2-carboxylic acid (XIII)

(XIII)
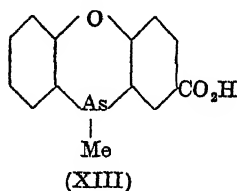
In this compound, the oxygen atom has a radius of 0·66 Å and the arsenic atom one of 1·21 Å. The following table shows the relation between the angles θ and ϕ and the angle (ψ) at the hinge, that is, at the O—As axis, between the planes XAB and YAB.

θ	90°	110°	128°	137 5°
ϕ	72 5°	86 5°	97 5°	103°
ψ	99°	123°	149 5°	180°

In order that the molecule should be flat ($\psi = 180°$), the oxygen angle must be 137·5° and the arsenic angle 103°. If, therefore, the oxygen valency angle is not amenable to much strain, we might expect the molecule to fold. The above authors actually found that the phenoxarsine derivative could be resolved and, moreover, that the optical activity was of a very stable kind. This at first sight is almost conclusive evidence that the molecule is folded, but it is just possible that the valencies of the arsenic atom, as a result of the particular way in which it is combined, are direction-stabilised, so that the activity is due to an " asymmetric tervalent arsenic atom." From the good deal which is known about tervalent arsenic derivatives, this alternative explanation seems unlikely to be true, but clearly the point can only be settled experimentally. Lesslie and Turner have consolidated their results by resolving two analogous phenoxarsines in which the methyl group is replaced by ethyl or by phenyl (*J. Chem. Soc.*, 1935, 1268 ; 1936, in the press). These compounds were also possessed of a high degree of optical stability.

Assuming the correctness of folding as the cause of molecular dissymmetry, it becomes necessary to attempt the synthesis and resolution of a large number of compounds of the above general formula (XII). Should some of these resolve and some not, and should those which resolve differ among themselves in optical stability, a great deal will be learned about the real meaning of valency angle.

The effects of atomic size and valency angles are also brought out in a study of optical activity dependent upon restricted rotation within the molecule. This kind of stereoisomerism is in a different position from that occupied by other types of stereoisomerism, in that it was discovered, not as a result of theoretical considerations, but by what can be called nothing less than an accident. The facts can be recalled by mentioning that in 1922 Kenner obtained by synthesis a 6 : 6'-dinitrodiphenic acid different from that already described in the literature. Kenner accepted the constitution of the older acid as proved, and although four years later he himself showed this acid to be not a space but a structural isomeride of the newer acid, his original assumption of stereoisomerism led him to attempt the resolution of his own acid, and this resolution, of course, was successful. Kenner came to no definite conclusions as to the cause of the molecular dissymmetry, but he was greatly handicapped firstly by the Kauffler view of the diphenyl molecule and secondly by the absence of trustworthy information as regards the shape and dimensions of aromatic systems in general. The

overthrow of the Kaufller formula and the steady sifting of X-ray evidence enabled Mills and to a lesser extent others to explain the molecular dissymmetry of Kenner's and other optically active substances. The explanation was at once accepted (1926) and is a simple one.

Benzene is an approximately flat hexagon. If one group is attached to it, the bond joining it to the nucleus makes angles of 120° with the two adjacent sides of the hexagon. Therefore in phenylbenzene, or diphenyl, the two nuclei will be co-axial, with free rotation about the common axis, and the system will oppose any tendency to destroy the co-axial arrangement. When atoms or groups, A, B, C and D of sufficiently large volume are attached to the four central positions (see XIV), the molecule cannot become wholly planar and therefore becomes dissymmetric, provided neither nucleus is itself symmetrical as regards substitution.

A   C

B   D

(XIV)
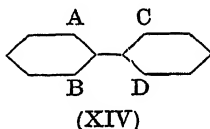
This conception once attained, it became possible to apply it generally, and Mills predicted and obtained optical activity in the naphthalene series and later in the quinolinium series. The exploitation of the idea is far from complete, but sufficient has been done already, by Bell and Kenyon, Kuhn, Meisenheimer, Adams and others to show that no essential modification of Mills' theory will be necessary.

From X-ray and other data, it is known that the radii of atoms such as can take up the positions occupied by A, B, C and D range from about 0·4 Å (hydrogen) to about 1·4 Å (tellurium). The two benzene nuclei in diphenyl are made up of six carbon atoms of radius 0·7 Å (see Pauling, Brockway and Beach, *J. Amer. Chem. Soc.*, 1935, **57,** 2705), disposed hexagonally in a plane, the regularity of the disposition being caused by resonance, which also determines the relatively small radius of the aromatic carbon atom.

In discussing the stereochemistry of diphenyl and related substances, it is convenient to use the term " atomic radius " for that measurement which corresponds to the distance between the centres of 2 atoms when they are combined. For example, a large number of measurements show that the distance between the centres of two joined saturated carbon atoms, *e.g.* those in ethane, $CH_3 \cdot CH_3$, is 1·54 Å. One can therefore speak of 0·77 Å as the effective radius of each carbon atom. Measurement of methyl chloride and similar

compounds shows that the distance between the centres of the carbon and chlorine atoms is 1·85 Å, so that taking the radius of carbon as 0·77, that of chlorine becomes 1·08 Å. Usually, the smaller figure 0·99 Å is taken, after Pauling.

It must be emphasised that atomic radii calculated in this way do not show the farthest distance at which two uncombined atoms can begin to come into each other's fields, and so repel each other, but they do show the distances within which such atoms *cannot* approach each other, and this, from the point of view of devising experiments, is the better state of affairs. The experiments themselves will provide us with an indication of the larger figures, and in a qualitative way this end has already been achieved by the experimental work on the optical stability of dissymmetric diphenyl derivatives.

This point made clear, it is evident from a consideration of the geometry of the diphenyl molecule that if A=B=C=H and D is an atom of radius about 1·3 Å, neither benzene nucleus will be able to undergo a complete rotation about the common axis relative to the other nucleus, since D will be unable to pass the hydrogen atoms A and B.

The case of the halogens as substituents is particularly instructive. Calculation shows that if A=B=C=D=F (fluorine radius, 0·64 Å), free rotation should be possible. In fact, Kleiderer and Adams (*J. Amer. Chem. Soc.*, 1933, **55**, 4219) were unable to resolve

$$\text{(XV)} \qquad \text{(XVI)}$$
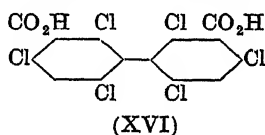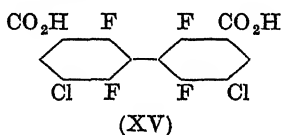
the acid (XV) or even to observe mutarotation of its alkaloidal salts. When, however, A=B=C=D=Cl (chlorine, radius 0·99 Å), it was found possible to effect resolution (White and Adams, *ibid.*, 1932, **54**, 2104) and the optically active forms of acid (XVI) were difficult to racemise. With bromine (radius, 1·14 Å) it has been found possible to effect a resolution in two cases with only 2 bromine atoms present. In the first (XVII) A=C=Br and B=D=H.

$$\text{(XVII)} \qquad \text{(XVIII)}$$
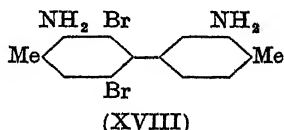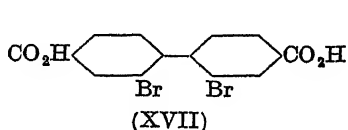
In the second (XVIII) A=B=Br and C=D=H. The active forms of the first compound were very easily racemised, but those of

the second were appreciably more stable (Searle and Adams, *ibid.*, 1934, **56**, 2112 ; Patterson and Adams, *ibid.*, 1935, **56**, 762). Previously it had been shown that the di-iodo acid (XIX) was

$$CO_2H \langle \bigcirc \rangle - \langle \bigcirc \rangle CO_2H$$
$$\overset{|}{I} \quad \overset{|}{I}$$
$$\text{(XIX)}$$
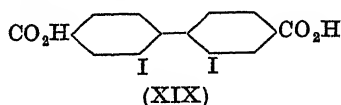
capable of optical activity, although even here, with the larger iodine atoms (radius 1·33 Å) racemisation was easily effected (Searle and Adams, *ibid.*, 1933, **55**, 1649).

When A, B, etc., are composite groups, the problem is even more interesting. Three types are possible, and they can be represented by AX, AX_2 and AX_3 ; BX, BX_2 and BX_3 and so on (for example, OH, NH_2 and CH_3). The rotation of a single atom about its point of attachment in one of the A, B, C, D positions in diphenyl clearly has no stereochemical consequences, but when the atom A carries one, two, or three other atoms, as in the types AX, AX_2 and AX_3, rotation of A implies rotation of the X groups, which describe what may be called an annulus. This is diagrammatically indicated below :

$$\text{AX} \qquad AX_2 \qquad AX_3$$
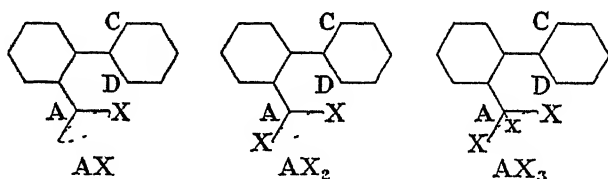
In the case of the rotation of a group AX, X only occasionally revolves into the position represented by the full line, corresponding to the nearest point of approach to D (or C). With a group AX_2, the frequency with which X approaches nearest to D is twice as great and with AX_3 it is three times as great. The comparative volume effects of these three groups should be experimentally demonstrable and should be in the order $AX_3 > AX_2 > AX$, for similarly constituted groups.

The type AX is represented by methoxyl, OMe, and van Arendonk, Cupery and Adams (*ibid.*, 1933, **54**, 4225) found that the acid (XX) could not be resolved, although the salts of the base (XXI) exhibited mutarotation at low temperatures,

$$\overset{CO_2H\ \ OMe}{\underset{OMe}{\langle\bigcirc\rangle}} \overset{OMe\ \ CO_2H}{\underset{OMe}{\langle\bigcirc\rangle}} \qquad \overset{NH_2\ \ OMe}{\underset{OMe}{\langle\bigcirc\rangle}} \overset{OMe\ \ NH_2}{\underset{OMe}{\langle\bigcirc\rangle}}$$
$$\text{(XX)} \qquad\qquad\qquad \text{(XXI)}$$
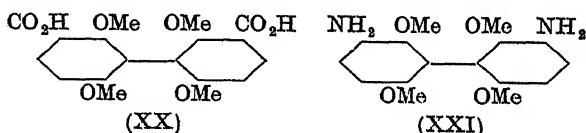
so that OMe has a volume effect larger than F, but not a great deal larger. By replacing OCH_3 by $OCH_2CH_2CH_3$, the volume effect is increased considerably, for (Li and Adams, *ibid.*, 1935, **57,** 1565) the acid (XXII) with $R = OC_3H_7$ is 7 times as stable as that in which $R = OCH_3$
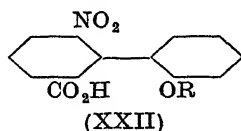
(XXII)

In the AX_2 class there are three interesting groups. Kleiderer and Adams (*ibid.*, 1931, **53,** 1575) were able to resolve the base (XXIII), although the N atom is not appreciably larger than fluorine.

(XXIII)
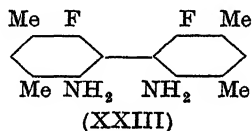
The " dynamic effect " of the NH_2 group is thus clearly demonstrated. The nitro (NO_2) and carboxyl (CO_2H) groups are the other examples. Of these, the nitro group has the larger volume effect, and this may be attributed to the " stiffer " build of NO_2 as compared with CO_2H, this making the group less susceptible to deflexion than the other.

In the AX_3 class, we come to groups which are very efficient " obstacles," since the dynamic effect is so large. The methyl group (CH_3) is a remarkably effective group in this respect, and is superior to the chlorine atom. The sulphonic acid group, SO_3H, has so large an effect as an obstacle that, as Lesslie and Turner have shown (*J. Chem. Soc.*, 1932, 2394), the sulphonic acid (XXIV)

(XXIV)
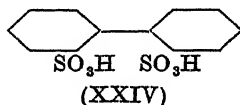
which contains only two of these groups, can exhibit optical activity.

Perhaps the most interesting case of all is that of the trimethyl-ammonium salt group, for the group $\overset{+}{N}Me_3$ is the smallest one that can be imagined of type AX_3, because (1) nitrogen is the smallest atom which can carry three other atoms, (2) methyl groups are the smallest possible stable attachments and (3) the nitrogen atom is positively charged. Yet Shaw and Turner (*J. Chem. Soc.*, 1933,

135) found that the compound (XXV) could be obtained in optically active forms, which were by no means easily racemised.

$$NMe_3I \quad NMe_2$$

(XXV)
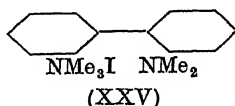
The grouping $\overset{+}{A}sMe_3$ heads the list at the moment for volume effect. Here we have a large A atom (radius 1·21 Å) and a large dynamic effect. The compound (XXVI)

Br

$$AsMe_3X$$

(XXVI)
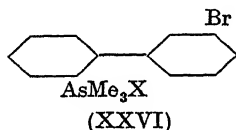
is the only one known containing only one group in the A, B, C, D positions which is capable of exhibiting optical activity (Lesslie and Turner, *J. Chem. Soc.*, 1933, 1588).

It is clear, therefore, that knowing the atomic radii of elements in their compounds, one can predict with reasonable certainty the kind of effect atoms and groups will have in preventing free rotation in molecules of the diphenyl type. From the knowledge so obtained one can form an excellent idea of the steric effects these atoms or groups must produce in other types of compounds in connection with which there is no stereochemical issue.

BIOCHEMISTRY AND CAUSAL MORPHOLOGY IN AMPHIBIAN REGENERATION

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THE importance of parallel biochemical and experimental studies in the unification of biology probably needs no emphasis to-day. It is generally realised that the essential problem of biology is the form-problem, and that whatever contributes to our understanding of the maintenance and nature of organic form is a valuable step forward. Biochemistry studies, first, the energy-changes involved in the formation and degradation of the comparatively small molecules which furnish the energy for the construction and running expenses of organic form. Secondly, it studies the processes of polymerisation and linkage of large molecules, where the molecular level adjoins the micro-morphological level. And thirdly, it investigates the nature and properties of specific molecules which exercise a stimulatory or inhibitory effect upon the morphological constructions themselves. Its province includes, therefore, energetics, fundamental structure, and the morphogenetic stimuli. Experimental morphology, of course, begins at the other end. We know that the organic forms of nature are not the only possible organic forms, and we know many ways of experimental interference whereby it is possible to control the appearance of organic forms. The experimental morphologist, therefore, is a kind of specialised morbid anatomist or pathologist ; with the difference that his abnormal forms are not the result of any exterior pathogenic agent, they arise from the abnormal play of perfectly normal interior forces. And thus working from each of the two ends, like parties of engineers in a tunnel, the experimental morphologists and the biochemists are all the time steadily approaching one another. The former are making generalisations about complex assemblages of facts, such as the potentialities of a limb-bud, which cannot as yet be expressed in any simpler terms ; the latter are studying the material substratum which carries these complexities. But in the meantime one thing is always necessary, namely that attempts should be

made to set side by side the concepts of biochemistry and causal morphology. And this can usually best be done when the morphological factor is a highly varying one. For these reasons it would be superfluous to offer any apology for a discussion of some of the recent work on the biochemistry and causal morphology of amphibian regeneration, quite apart from the fact that much of it is rather inaccessible in this country.

There is, of course, an extremely large literature on the regeneration of amphibian tails, limbs, and gills, after they have been cut off. The interesting work of Guyénot and his pupils such as Schotté, of Weiss, and of Harrison, has been reviewed in a number of books and monographs (Weiss [54]; May [24]; Huxley and de Beer [18]). Among recent short accounts that of Woerdemann [55] is excellent, and particular mention may be made of the article by Waddington [52] which appeared not long ago in this journal.

We may summarise in a brief paragraph the occurrences that follow the amputation of an amphibian limb. First, the wound surface is covered by a layer of epidermis. Then there appears in the centre of the new skin a small bud known as the regeneration blastema. As the bud grows, it becomes penetrated by blood-vessels, and the muscular and skeletal elements appear in it. But whereas it was formerly thought that tissues of the various kinds left behind simply proliferated to give the new morphological shapes required, muscle giving rise to muscle and bone to bone, we are now fairly certain that the whole mass is differentiated from indifferent cells. This was proved by grafting some red belly-skin on to the base of a newt's limb, and then amputating the limb at the base. If the regenerated limb epidermis was derived from the traces of the former skin remaining, it would have been expected to be red, but if from deeper tissues, black. And it was, in fact, black.

Perhaps the point of greatest interest about regeneration in amphibia, however, has been the question whether the regeneration blastema is an " equipotential system." In the early developing embryo, as embryologists very well know, the destinies of the various parts are not fixed and certain, but can be changed by transplanting a given portion from one environment to another. Later, during gastrulation, the period of " self-differentiation " sets in. In other words the fates of the principal parts *are* now fixed, and whatever the experimental interference, they will continue to carry out the differentiation upon which they were engaged at the time of the operation. We know that this settling of destinies is brought about by the action of the organiser centre, and later by

the separate organisers for the various structures. Now it has been demonstrated that something very similar takes place during regeneration. If the regeneration blastema is removed from its natural site early in its development and placed upon another site, it will deviate from its presumptive fate, and will produce a structure appropriate to its new site ; but if it is removed at a later stage then it will produce a structure appropriate to its original site. Some form of determination has therefore set in. This has been tested many times by transplantations between tail and limb sites. It has also been proved that the determination of a limb-bud's polarity takes place in separate stages, that is to say, the decision as to which is to be the upper and which the lower surface of the limb takes place at a different time from the decision as to which is to be its hand end and which its shoulder end.

There can be no doubt that a succession of such decisions does take place during normal regeneration. And it has become generally accepted that a blastema, when removed while still very young to another site (e.g. a very young tail-blastema removed to the site of an amputated limb), will deviate from its presumptive course, and pursue a course appropriate to its new situation. Recently, however, a doubt about this has been raised by Polezaiev [45]. This worker suggests that what really happens when a small blastema is grafted on to a wound surface belonging to another appendage is that resorption takes place, and then subsequently a blastema appears which is really derived from the new locality itself and therefore behaves appropriately. Although able to repeat many of the observations of earlier workers just described, he could not decide the question of resorption. Further work is clearly needed here. In order to overcome this difficulty Polezaiev grafted no less than eleven tail blastemata on to the wound surface of an amputated limb. All of these except the covering ones were deprived of their epidermal coats. What followed was that in nearly all his cases, this large mass of material developed into a tail, although the individual blastemata of which it was composed would have given rise to limbs had they been grafted alone. There is here a quantitative factor in determination which demands more study. Polezaiev regards it as still uncertain whether there can be a true deviation of presumptive direction of development however young the blastema, as between tail and limb.

Another requirement not yet fully met in these studies is the accurate knowledge of the normal rate of regeneration. Grotans [15] has recently completed an elaborate study of the speed of regeneration of amputated axolotl gills. At first it is rapid, then for several

weeks it continues at a steady rate, and thereafter it regularly falls off.   With increasing age, the initial rise is slower, the maximum less persistent, and the final fall more gradual.   Godlewski and Latinik [13] have made similar quantitative comparisons between the growth of the axolotl tail in normal development and during regeneration.

A question which has attracted much attention is the loss of regeneration power in anurans.   In the urodeles (newts), as is well known, a strong power of regeneration persists into adult life, but frogs and toads can only regenerate their limbs at the early stages of metamorphosis.   Afterwards the power is lost.   It seems, indeed, to arise and to disappear in a very definite way.   Thus Svetlov's recent work [48, 49] on anuran tail regeneration shows that in the first tail-bud stage no regeneration is possible, but that the power reaches a maximum about the time of hatching from the egg-membranes (8 days from fertilisation).   Before metamorphosis it has disappeared.   Svetlov seeks to correlate the presence of the regeneration-power with intensity of embryonic development, but this is a difficult concept to make clear, and his recourse to it is hardly convincing.

Polezaiev's experiments [46] on limb-regeneration are also interesting.   By traumatisation of the wound-surface (mincing it with a scalpel *in vivo*) he found it possible to obtain limb-regeneration at a stage subsequent to that at which the power normally disappears.   He explains this by assuming that the traumatisation stimulates the accumulation of the indifferent regenerative cells from which the blastema is formed, and that it is the lack of these which is involved in the normal decline of regenerative power.   He shows also that re-regeneration (the regeneration of a limb that has been produced itself by regeneration) may take place quite late in metamorphosis.   Much of the discussion about loss of regeneration power has, of course, turned upon the question of whether this loss is brought about by a humoral change in the whole body, or whether it is localised in the limbs.   Thus Guyénot first showed [16] that tails and limbs of little frogs which had lost their power of regeneration, did not regain it when they were transplanted on to salamander larvæ in the proper positions, although salamanders never lose regenerative power.   Transplanted and then amputated, they never grew again.   Liosner [21] completed the picture by transplanting backwards and forwards in time.   From frogs that had lost the power, limbs were transplanted to frogs which still had it ; and vice versa.   In this case also, the limb was unaffected by its new situation.   If it could regenerate

PLATE I

Fig 1 —Transformation of fibroblasts into cartilage cells by the action of cartilage extract (Nassonov)

a Connective tissue fibroblasts   b Transitional zone   c Rounding fibroblasts
                                        d Cartilage

(From *Comptes Rend Acad Sci U R S S*)

Fig 2 —Ectodermal prominence resembling a regeneration blastema induced by the implantation of cartilage (Nassonov)

a, Disintegrating implanted cartilage   b, Epidermis   c Epidermal outgrowth
                                            d, Cavity containing blood

(From *Comptes Rend Acad Sci U R S S*)

before, it would do so just as well on an older body ; but if it had lost the power, transplantation on to a younger body where the power was still present, did not restore it. All this work seemed to prove that the change was a local one. But if this were so, limbs from tadpoles at the early regenerative stage, transplanted on to little frogs just about to lose their regenerative power, should lose their regenerative power later than the limbs of the host. On the other hand, if the body as a whole were concerned, the power ought to be lost in all the limbs approximately simultaneously. This experiment has been tried by Borssuk [6], with the result that the second of these alternatives was found to hold good. So we are faced with another paradox, which, however, offers fascinating prospects for further work.

It should be noted that investigations along these lines have great theoretical importance. Waddington [52] has pointed out that there is a connection between regenerative power and the individuation field which has to be assumed in early development. If we knew really why and how it is that animals lose regenerative power, we should be a long way towards the understanding of neoplasmatic growth. Further discussion of this will be found in the present writer's Oliver-Sharpey Lectures at the Royal College of Physicians (*Proc. Roy. Soc. Med.*, 1936).

The part played by the various tissues in the regeneration blastema is also a subject of great interest. It is known that if a limb-bone is removed it does not regenerate, yet if this limb be then amputated a new limb complete with bones does regenerate. This suggested that the skeletal system does not have much to do with regeneration. Nervous system and notochord have for some time been under suspicion (Woerdemann [55]). But recently Nassonov's work [28–31] has indicated that cartilage cells are important. First Nassonov [28] obtained duplication of limbs and appearance of digital processes, etc., by implantation of regeneration blastemata destroyed by mincing. He then found [29] he could obtain the same results by implanting desiccated regeneration blastemata, in which the cells had all been killed, though here the number of positive cases was smaller. This supported the earlier view that an important rôle in regeneration is played by the decomposition products of the traumatised tissues. Next he showed [30] that the addition of an aqueous extract of cartilage would, when added to a tissue culture of connective-tissue fibroblasts, cause the formation of a large amount of hyaline cartilage, sometimes even converting the whole of the culture. Plate I, Fig. 1, illustrates this. Finally, he found [31] that when implanted under the skin in

a limb-district, a piece of cartilage would give rise to an epidermal prominence resembling a regeneration blastema, as shown in Plate I, Fig. 2. Further organisation, however, did not occur. In this connection it is very interesting that according to Butler [10], the primary effect of X-radiation in inhibiting the development of a regeneration blastema in amphibia is upon the numerous mitoses in the bud, while the second effect is upon the cartilage of the stump, which dissolves and finally disappears. The effect is specific, for the same dosage does not seriously injure cartilage in other parts of the body. Butler thinks that as cartilage differentiation is completely stopped, together with all other differentiation in the blastema, cartilage dedifferentiation continues unchecked till all has disappeared. Plate II, Fig. 3, shows two of his beautiful sections, (a) a normal, and (b) an irradiated blastema. The importance of skeletal elements in amphibian regeneration also appears from the work of Litschko [23].

Then there is the epidermis. Efimov found [11] that by substituting skin from head or back for the skin of the blastema of a limb, he could completely inhibit the regeneration. This has since been confirmed by Polezaiev and Favorina [47], who further found that regeneration is completely inhibited by the removal of the epidermis from the bud, and its replacement by no other epidermis. In such a case histological differentiation proceeds, but not morphological differentiation, that is to say, bone, muscle fibres and connective tissue cells are formed, but they are not organised into a limb. Plate II, Fig. 4, illustrates the chaos resulting, as compared with a normal epidermis-covered bud. There is a hint here that the epidermis may carry the individuation-field. But muscle also is said to be of importance. Liosner and Voronzova [22] transplanted muscle tissue from limb to tail and vice versa, so that it formed a muff around the bones, with the result that determination was overridden, and tail-blastema that should have produced limb on limb-site, changed back again and formed tail on limb-site.

We now come to a series of more chemical researches. Okunev [34] studied the hydrogen ion concentration of the tissues of the regenerating amphibian blastema, using an electrode of the Biilmann-Lund-Cullen type. The pH of the normal tissues was found to be 7·2; during the period of wound-healing, it fell to 6·8, rose again slightly, then fell to 6·6, a minimum at the first appearance of the blastema as a noticeable bump (6 days); after which it continuously rose to regain its original value at 18 days after amputation. This process repeated itself very regularly in all Okunev's experiments. In a subsequent paper [35] he made

PLATE II
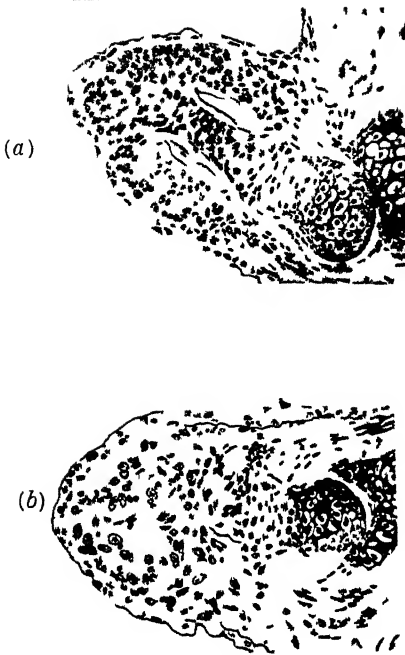
Fig 3 —Effect of X radiation on limb regeneration (Butler)

(a) Longitudinal section through normal blastema of limb 5 days after imputation
(b) Longitudinal section through irradiated blastema of limb 7 days after imputation Regeneration is at a standstill The outgrowth of cartilage is inhibited

(From *Journ Exp Zool*)
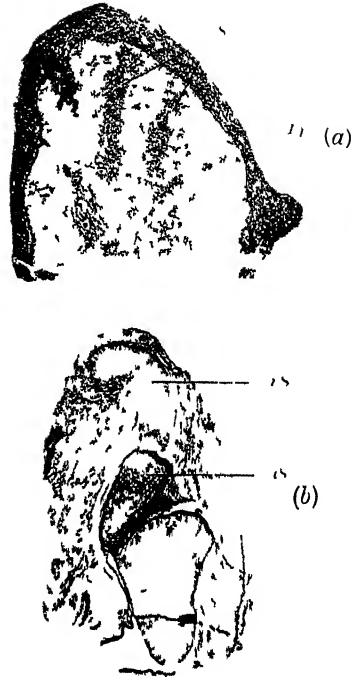
Fig 4 —Effect of removing epithelium from a regeneration blastema (Polezaev and Favorina)

(a) Normal blastema of hind limb covered with epidermis
(b) Blastema from which the epidermis has been removed

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san Skeletal rudiments
Ep Epidermis
nS newly forming bone
aS remains of old bone

(From *Archiv f Entwicklungsmech*)

a parallel investigation on a Pacific crab (*Paralithodes kamschatica*) at the Vladivostok marine station, with the result that the same acidosis could be observed during the regeneration of the claw. The normal level was here 7·0 and the minimum 6·7. Later [36] he studied the buffering power of the amphibian blastema, and found the very interesting fact that it was at a maximum during the time at which the pH itself was at a minimum. The change was regular and very repeatable. Speculation about the meaning of the phenomenon was not pushed very far, but the suggestion was made that substances of the nature of chondroitin sulphuric acid, produced by the dedifferentiation of the cartilage stump, might be responsible for both the low pH and the high buffering power. This links in an interesting way with the work of Nassonov just referred to.

Okunev next [37] investigated the oxidation-reduction potential of the blastema tissue. From the normal level of 225 mv. the E_h fell to a minimum of 180 mv. at the tenth day from amputation, after which it rose, regaining its normal value at 30 days. Here again the values were steady and repeatable. Okunev suggested that this result might be connected with variations in the sulphydryl content of the blastema. The higher the concentration of such highly reducing substances as glutathione, the lower the oxidation-reduction potential might be expected to be. This hypothesis was tested by Orechovitch [41], who found 20 mgm. per cent. in the normal axolotl tail ; just over 45 mgm. per cent. in the regenerated tail from the fifth to the tenth day after amputation, after which the concentration fell to normal. The concentration was also raised (but variable) in the tissues immediately surrounding the blastema.

All this naturally reminds us of the state of affairs in embryonic tissue proper, where, as has been known for a long time, the sulphydryl concentration is high, and steadily declines during development (see the composite graph in Needham [32]). It may also be relevant to the supposed stimulating effect of such sulphur compounds on growth and regeneration, but the principal support for this effect is the work of Hammett and his collaborators, whose data are unconvincing. A critical review of some of it will be found elsewhere [32]. It has been used to explain the curious claim of Morosov [26] that embryo-extract, whether of mammalian or amphibian origin, injected into the peritoneal cavity of amphibia, accelerates the process of tail-regeneration. Morosov also states that sodium thiosulphate has a stimulating effect on limb-regeneration. Thiosulphate had previously been found by Binet and Magrou [1] to shorten metamorphosis.

The other suggestion made by Okunev [39, 40] in order to explain the low rH of the regenerating blastema was that deficiency of blood-supply brings about deficient oxygenation of the cells. This explanation does not by any means exclude the former one.

Of especial interest in the comparison of the blastema with embryonic and neoplasmic tissue is study of its carbohydrate metabolism. Yet as far as can be ascertained very little work has been carried out in this direction. Pentimalli's examination [44] of the respiratory and glycolytic rate was limited to repair tissue in mammalian muscle (he obtained a respiratory rate slightly higher than normal, and a small aerobic glycolysis). No information, however, is available for the amphibian blastema, and the subject is under study by Nowiński [33]. All that can be said is that two investigators have found a lactic acid content considerably higher than normal; thus Okunev [38] found 18 mgm. per cent. lactic acid in the tissues of the normal limb, and 39 mgm. per cent. in the as yet undifferentiated limb-blastema. Vladimirova [50] made a somewhat fuller study, and by dropping the blastemata into liquid air, tried to avoid glycolytic activity during the preparation of the specimens. This may account for the fact that her values were lower. In the normal limb she found 9 mgm. per cent. ; on the sixth day after amputation, 13 mgm. per cent.; on the sixteenth, 16 mgm. per cent. (the maximum), after which there was a steady return to normal. This may imply a high anaerobic glycolysis analogous to that of embryonic tissues, or there may exist, as in tumours, an aerobic glycolysis. We are reminded also of the accumulation of lactic acid which occurs in the hen's egg a few days after development has begun. The further investigation of the carbohydrate metabolism of the blastema is perhaps the most important immediate necessity in this field.

Another piece of work of Vladimirova's [51] was the estimation of the free amino-nitrogen to be found in the blastema. As in the case of lactic acid, the figures are very convincing, and from them it appears that there is also a rise to a maximum followed by a return to normal. At 0, $4\frac{1}{2}$, $7\frac{1}{2}$, 16, 21, 25, 60 and 75 days respectively, the figures were 31, 37, 39, 41, 40, 42, 34 and 33 mgm. per cent. A decreased amino-acid oxidation may be involved here as well as an enhanced turnover connected with the formation of the new tissues. Vladimirova's work was confirmed by Orechovitch and Bromley [42] who obtained 35 per cent. of the total N as amino-N in the blastema, as against only 17 per cent. in the normal tail. These workers also adapted the ninhydrin reaction to histochemical use, with favourable results.

Equally important is the state of the tissue proteases in the blastema, and these have received a good deal of attention in Belkin's laboratory. Bromley and Orechovitch [8] first found that the intensity of autolysis in the blastema, the normal tail, and the tissues surrounding the blastema, was very unequal. In terms of non-protein nitrogen liberated upon 12 hours autolysis, the figures were (a) for the normal tail 58 mgm. per cent., (b) for the blastema of the tail 100 mgm. per cent. and (c) for the surrounding tissue 233 mgm. per cent. The two latter showed thus a more thorough-going autolysis, indicating a higher activity of the tissue kathepsins. When directly tested [9], using glycerol extracts of blastema and surrounding material, acting on normal tail as substrate, this proved to hold good. It appeared also that there is a difference between the proportions of kathepsin bound and free in the blastema and the normal tail. Whereas the free (lyo-) kathepsin seemed equally active, the bound (desmo-) kathepsin seemed more active in the blastema. Finally Orechovitch, Bromley and Kozmina [43] made comparative estimations of kathepsin activity (in glycerol extracts) in the blastema at different stages of its regeneration, using gelatine as substrate.[1] The activity was found to reach a maximum fairly early (3 days) after amputation, while the blastema was as yet extremely small. Later it fell to normal slowly. The picture was completed by Orechovitch's estimation of sulphydryl compounds in the blastema, already mentioned [41], for as it is known that -SH increases kathepsin activity, the high concentration of it in the blastema would fit in with the increased enzyme activity. In contrast to these findings, Mystkowski's work [27] may be mentioned; throughout the development of the chick embryo, the kathepsin activity is low and constant. But of course this does not exclude the possibility that it is locally high in the limb-buds themselves.

Some of the quantitative relationships which have so far been mentioned are placed on the same time-scale in Fig. 5, from which it can be seen that there is a good deal of biochemical regularity already observable in the regeneration process. Since both limb and tail regeneration have been studied, the curves are not all exactly comparable, but they show sufficiently well the general course of events.

The proteases bring us to the last section of this review, a short discussion of one of the most curious problems of normal

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[1] A similar piece of work was carried out by Blacher and Tschmutova [5], but as their measurement of the proteolysis involved the mitogenetic ray technique, we shall not further consider it here.

development, the mechanism of perforation of the peribranchial wall.

The regeneration blastema of tail or limb seems to possess considerable histolysing activity. Orechovitch and Bromley [42] covered tail-blastemata with flaps of skin, but in nearly all cases
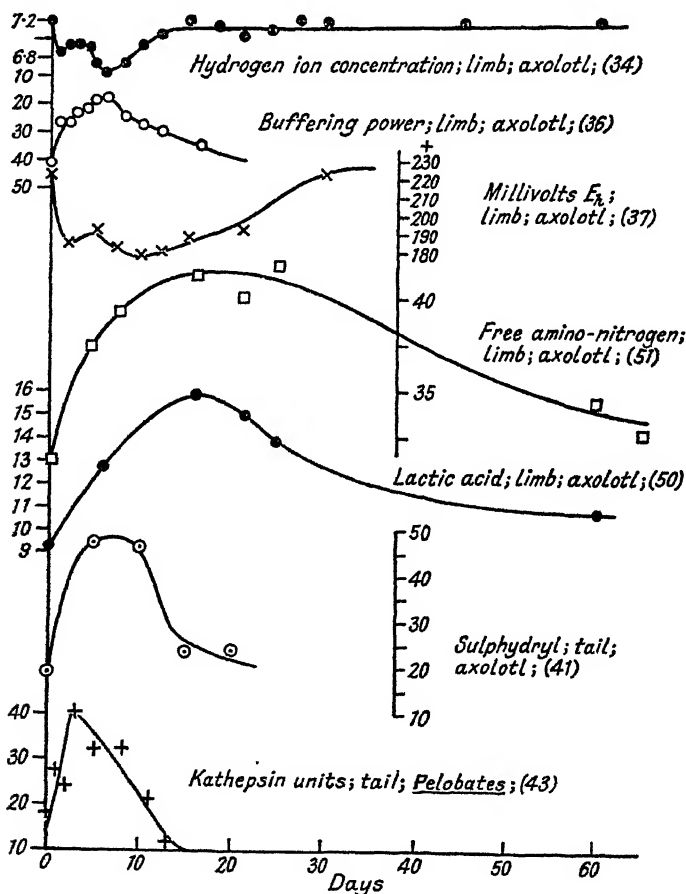
Fig. 5.—Composite graph illustrating iochemical events during regeneration (Okunev; Vladimirova; Orechovitch and Bromley).

the flaps were seen to decay after a certain lapse of time, and the regenerating member broke through. That this was due to enzyme action and not mechanical pressure was proved by implanting underneath back or side skin, small pieces of normal limbs or tails. Although the pressure was in some cases considerable, the overlying

skin never gave way. This histolysing power of the blastema agrees well enough with what has already been said about its kathepsin activity and the high concentration of free amino-nitrogen in it.

Now histolysis occurs also at many important points in normal development, and of these one of the most interesting is the perforation of the peribranchial wall or operculum in anuran tadpoles during metamorphosis. As is well known, the bud of the fore-limb develops underneath the operculum, and while the left fore-limb makes its way out through the open spiracle, the right one appears through a special perforation. During this process, the gills are rapidly degenerating, an event which, as Irichimovitch and Lektorsky suggest [19], is, together with the degeneration of the tail, geared with maximum growth intensity of the fore and hind limbs.[1] An interesting histological account of this degeneration of the gills has recently been given by Grotans [14]. It can be inhibited by ligaturing the sixth branchial arch, so that the lungs fail to develop and take over the burden of respiration, as Figge [12] has shown. The problem arises as to what is the determining factor in this singular perforation of the operculum ?

Already in 1906 Braus [7] removed the limb-bud and found that perforation could still proceed. This was subsequently known as Braus' phenomenon. Braus thought that both limb-bud pressure, and some internal factor, were normally responsible, and the case has always been used as support for the concept of " double assurance " in morphogenesis, in which it is supposed that several causes co-operate to produce the same result. Then Helff [17] examined the problem again and decided that the most important factor in perforation was some substance liberated in the histolysis of the gills. If the degenerating gills were grafted under the skin of the back, perforation occurred there too, and a similar effect could be obtained by grafting some histolysing muscle from the degenerating tail under the skin of the back.

The most recent examination of the question is that of Blacher, Liosner and Voronzova [4]. They first confirmed Braus' finding that removal of the limb-bud does not stop the perforation process. They next showed that transplantation of the whole limb-bud including the shoulder-girdle to a site under the skin of the back does not cause perforation there. This disposed of a suggestion previously made by Weber [53] that the glands at the base of the

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[1] The metabolism during this period (nitrogen-excretion, etc.) has been studied in work of much interest by Blacher and Efimov [2]; Blacher and Liosner [3], and Liosner [20].

limb-bud were responsible for the perforation. Next they confirmed the Helff effect by obtaining perforations of the back skin when the degenerating gills were implanted underneath it. All this indicated that a substance produced by gill-histolysis was responsible. But they then found that opercular skin grafted by itself to a site on the back, also perforates in due course; and this was controlled by transplantations from other places such as ventral surface and flank; these never perforated. Finally, skin from back or side transplanted into the place of the opercular membrane refused to perforate, and the limb was permanently retained within the cavity.

From this description it can easily be seen with what a conflicting series of facts, causal morphology has sometimes to deal. Further work will be necessary to find out exactly what is the part played by the following factors: (*a*) the assumed substance produced by gill-histolysis, (*b*) the inner determination of the operculum itself, (*c*) the influences of hormonal character (thyroxin, etc.) in the blood-supply of the operculum. What does seem to have been really ruled out is the limb-bud itself.

The concept of " double assurance " demands an article all to itself, but its value in causal morphology is perhaps doubtful. As Blacher, Liosner and Voronzova point out [4], the notion of determining factor only has meaning if it is a unity. Can a given alternative really be decided by a committee of unconscious factors? If, on the other hand, the additional determining factors were thought of as standing in reserve, and only coming into play when the necessity arises, we should have to abandon all hope of finding out what the normal processes in morphogenesis are, since the " necessity " in question must always arise when we interfere experimentally with the normal course of affairs.

In conclusion, it will no doubt have been remarked by the reader that a very large amount of the work that has been described has been done by Slavonic investigators, particularly from the Soviet Union and Poland. While science can acknowledge no nationality, it is often found that certain types of research flourish especially in certain countries at certain times. Thus until recently Germany was the home of amphibian experimental embryology, and the United States had almost a monopoly of the comparative embryology of invertebrates. We must wish every success to our Russian and Polish colleagues in their work on the phenomena of Regeneration.

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ADDENDUM

Interesting publications received just too late for inclusion in the above review are those of L. W. Polezaiev (*Bull. Biol.*, 1936, **70**, 54) on the capacity for regeneration from minced limbs, of M. A. Voronzova and L. D. Liosner on the rôle of the internal medium in metamorphosis (*Zool. Jahrb.*, 1936, **56**, 107), and of E. A. Scheremetjeva and V. V. Brunst (*Radiobiol. Gen.*, 1935, **4**, 57) and W. O. Puckett (*Journ. Morph.*, 1936, **59**, 173) on X-ray inhibition of regeneration.

CHÉRUBIN D'ORLÉANS: A CRITIC OF BOYLE

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WHEN Gobet edited for publication at Paris in 1777 a new edition of the well-known *Essays* of Jean Rey, which appeared originally at Bazas in 1630 and were "re-discovered" by Bayen in 1775 (*Observations sur la Physique*, 1775, **5**, 47), he drew attention to a work by Chérubin d'Orléans, which he quoted under the title of *Dissertation sur l'Impermeabilité du verre, sur la cause de l'augmentation du poids de l'Etain & du Plomb par la calcination* and described as having been published at Paris in 1679 and 1700, and from which he reproduced certain passages which, he alleged, confirmed Rey's ideas.[1] Since that time all trace of the work cited by Gobet appears to have been lost.

Father Chérubin of Orléans, a Capuchin of the province of Touraine, was however the author of several works on physics, some of which were widely known. These were all published at Paris and included *La Dioptrique Oculaire* (1671), *La Vision Parfaite* (2 vols., 1677–81), *De Visione Perfecta* (1678), *Effets de la Force de la Contiguité des Corps* (1679, and later editions to be detailed below), and *L'Experience Justifiée par l'Elevation des Eaux* (1681).[2] Beyond these facts, little is known of Chérubin except that his real name was François Lasseré. But he does not appear to have published any work under the title given by Gobet. This may explain why no mention of him is made either by Gmelin, Kopp, von Meyer or Bolton. It might be added that a recent and authoritative writer, in referring to Chérubin, was able to quote and consider only the extract given by Gobet (Metzger, *Les Doctrines Chimiques en France du début du XVIIe à la fin du XVIIIe siècle*,

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[1] *Essays de Iean Rey . . . Nouvelle Edition . . . Avec des Notes, par M. Gobet*, Paris, 1777, pp. xii and 213–16. Gobet stated that his attention had been drawn to Chérubin's *Dissertation* by Rouelle the younger.

[2] On the title-page of the work last named, the author is given as "Messire I.E.M.C.D.O."

Paris, 1923, p. 393, footnote 2). However, to his *Effets de la Force etc.* referred to above, Chérubin added among other matter an *Appendice* entitled *De l'Impermeabilité du Verre, où est demontré, que le Verre brut, n'a point de pores, qui penetrent ses deux superficies*, which is evidently the document to which Gobet was referring so imprecisely in 1777 that all trace of it seems to have been lost since his time. With regard to the general contents of the *Effets etc.*, it is enough to say here that its full title was *Effets de la Force de la Contiguité des Corps. Par lesquelles on répond aux experiences de la crainte du Vuide, & à celles de la Pesanteur de l'Air*, that the author was a Plenist and that he considered the so-called "weight of air" as scientific heresy.

The catalogue of the Bibliothèque Nationale contains entries relating to the following editions of the *Effets*, all published at Paris in 12mo with certain preliminary pages and 467 pages of text : E. Couterot, 1679 ; L. Lucas, 1688 ; and E. Ducastin, 1688. The British Museum copy was published at Paris by Lucas in 1689. The Bibliothèque Mazarine possesses an issue of 1700, published at Paris by Jombert. It was this latter and Couterot's edition of 1679 that Gobet referred to in 1777. In the preparation of this paper the author has used a copy of Lucas's edition of 1688, in which the *Appendice* on the impermeability of glass appears on pp. 343–62 ; and enquiry at Paris indicates that it appears on the same pages in the editions of 1679 and 1700, this being also the case in the edition of 1689 in the British Museum. A search through numerous library and booksellers' catalogues suggests that the *Effets* is a rare book.

The *Appendice* is directed in part against the conclusions advanced by Boyle in his *Discovery of the Perviousness of Glass to Ponderable Parts of Flame*, the fifth tract of his *Essays of Effluviums* (London, 1673), the contents of which the author has already analysed in this journal (1934, **29**, 253) ; and, although Chérubin's notions of the rôle of air in the calcination of metals are as crude as those of Rey, it is interesting to follow his criticism of Boyle's experimental method, a criticism which closely resembles that subsequently made by Lavoisier in 1774 in the first of those classic memoirs that led to the foundation of modern chemistry (*Observations sur la Physique*, 1774, **4**, 446).

In opening his general attack on those who believed that glass had pores penetrating it from surface to surface, Chérubin began by comparing glass with gold. Glass, he said, was the *chef d'œuvre* of Art, as gold was of Nature. Both these remarkable substances required long working, the one by Man and the other by Nature :

and both needed the application of heat (Art therein imitating Nature) and demanded the greatest skill in their production. Both were incorruptible and invincible against the effects of time. Glass, moreover, could be drawn out into fine, almost imperceptible, threads, thinner than the threads of the smallest spiders ; and such threads, thin as hairs, could be rapidly and continuously spun from a piece of molten glass for a whole day without breaking. Therefore glass fresh from the hand of the glass-maker could not have any pores penetrating it from surface to surface ; for, otherwise, these fine threads would be so unevenly weakened, and so unable to withstand bending in all directions, that they would break in parts that were weakened by the presence of pores. Since such fractures did not occur, there could not be any pores in the threads and therefore there were none in the glass from which they were made. Chérubin's first argument thus depended on the common observation that finely spun threads of glass could be bent in all directions without fracture.

For his second argument Chérubin referred to an observation of what would now be described as the torsional vibration of a thin glass fibre. He described how he had seen a skilful worker make a little sword from glass to amuse a child and how, having touched the newly made toy against a fragment of molten glass, the operator, holding the sword in one hand and the fragment of glass in the other, abruptly drew them apart to the full extent of his arms so that they remained joined only by an almost imperceptible glass fibre ; the fragment of glass being then placed on a mantelshelf, the sword, suspended by the glass fibre, hung below and, being deftly set rotating, continued this motion a hundred times without being touched and apparently would have continued it even longer without the fibre being broken. Whence, if a fibre of such extraordinary fineness had any pores in its surface, the resultant inequality of force along its length would have led to its being broken many times over in such a large number of rapid oscillations. Yet it was in this violent motion for a quarter of an hour and would have continued so much longer ; and it did not break. Therefore the fibre had no pores and, consequently, there were none in the glass from which it had been made.

Thirdly, argued Chérubin, the ebullition of liquids in the attenuated air of the receiver of the air-pump showed that glass had no pores ; for, otherwise, the highly attenuated air in the glass receiver would draw the more subtle parts of the external air through the pores of the glass much more easily than it drew out and disengaged the subtle parts of the enclosed liquids, parts that

were strongly held in the masses of those liquids by their natural union with other, more fixed parts.

Fourthly, continued Chérubin, an observation by the writer of a letter in the *Journal des Sçavans* for July 25, 1672,[1] provided another argument. This correspondent, unnamed by Chérubin, was Huygens : and in view of Boyle's subsequent curious handling of Chérubin's criticisms, of his unrivalled eminence in contemporary studies on air, and of his participation in some of the experiments reported in this letter, we are justified in taking some notice of its contents.   Huygens wrote that in December 1661 he had filled a glass tube C (Fig. 1) with water, freed from air by exposure for 24 hours in the evacuated receiver of an air-pump, and inverted it in D over more of the same water.   The glass B was then inverted over this and the whole was set on the plate A of an air-pump, B being suitably cemented to A.   On evacuation of B, the water did not descend in C, which remained perfectly full.[2]   There was no defect in the pump ; for repetition of the experiment after the introduction of a very small bubble of air into C led to the normal descent of the water almost to its level in D.   These results were obtained in tubes similar to C and ranging in length from 9 inches to more than 2 feet.   In one of these experiments, after B had been evacuated, a minute bubble began to form on the glass at the lower end of C ; it presently became detached from the glass and began to rise through the water in C ; and, when it reached the level of the water in D, it suddenly expanded and filled the whole of C, the water concurrently descending.   Huygens communicated his results to the Royal Society of London, who cast doubt upon them and suggested that the vessel B had not been properly evacuated.   In 1663 Huygens, when on a visit to London,
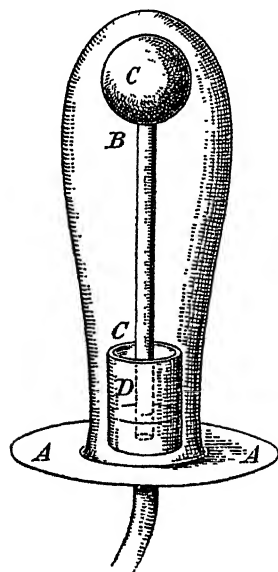
Fig. 1.

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[1] *Le Journal des Sçavans pour l'année MDCLXXII & MDCLXXIV, Nouvelle Édition*, Paris, 1724, p. 60.

[2] In similar experiments with ordinary water Boyle had been unable to bring the water in C down to the level of that in D ; water was left in C to the height of 1 foot, which Boyle ascribed to the presence of residual air in the receiver (*New Experiments Physico-Mechanicall, touching the Spring of the Air and its Effects, etc.*, Oxford, 1660, p. 140).

saw the experiment performed successfully before the Society. Boyle repeated it with mercury, freed from air, and succeeded in obtaining under vacuum in similar apparatus columns of mercury of 34, 52, 55 and finally 75 inches in height. Boyle noted that the mercury remained suspended even when the tube was removed to the open air; but the slightest bubble of air that entered the tube caused the mercury to fall to its more ordinary height of 27 or 28 inches. Huygens attempted to explain these unusual suspensions of water and mercury [1] as due to the pressure of a matter more subtle than air, easily penetrating glass, water, mercury and all other bodies that were impenetrable to air. This pressure, added to the pressure of air, was capable of sustaining the 75 inches of mercury, and perhaps more, as long as it acted only against the lower surface, *i.e.* the surface of the mercury in which the open end of the tube was immersed: but as soon as it could act also in the other direction (as, for example, when by tapping against the tube or by causing a small bubble of air to enter it), its pressure became equal on both sides so that there was then nothing different but the pressure of the air, which accordingly sustained the column of mercury at its ordinary height of 27 inches. It would be asked, Huygens admitted, why the water in C and the mercury in Boyle's tube, suspended in this way, were not likewise affected by the pressure of this matter, when those vessels were still full, since he had supposed that the matter could penetrate glass as well as water and mercury, and why its particles did not coalesce and begin to exert a pressure, since they came and went through the whole mass of the water and the mercury and since the glass in no way prevented their communication with those outside. Huygens admitted that his explanation was unsatisfactory since he had supposed glass, water and mercury to be permeable to this subtle matter; but he suggested that it might be supposed that the particles of that matter could not find passages in the glass, water or mercury large enough to pass several together through the particles composing those substances or to move in those passages with enough force to displace the component particles of the suspended mercury or water, the particles of either of these liquids being to some degree united with one another. To Chérubin, Huygens, regarding glass as penetrable in this way, was merely recognising the unsatisfactoriness of an explanation of facts that once more proved the very opposite, namely, the impenetrability of glass.

[1] Neither Boyle nor any of his associates at the Royal Society were able to explain these curious results, so contradictory to all that Boyle had discovered with regard to the "spring of the air."

Fifthly, proceeded Chérubin, the same experiment provided a further argument ; for the mercury, which had been freed from its light and subtle parts for the purpose of this experiment, was suspended at a height of 75 inches and fell to the same height as common mercury, namely, to 27½ inches. Yet the same subtle parts of the external air, which, it was supposed by some philosophers, by their penetration of glass occupied the space vacated by common mercury in its descent in a Torricellian tube, should as readily occupy the space deserted by the purified mercury used here. Therefore, either Nature, the invariable follower of the easiest path, was here supporting a completely avoidable column of mercury, 75 inches high, which could be easily replaced by the subtle matter of the air through the pores of the glass tube, and was thereby uselessly doing herself great violence, which was manifestly absurd, or else there were no pores in the glass.

Further, added Chérubin, who now gave up numbering his arguments and whose view of the matter was that in a Torricellian tube the lightest and most subtle parts of the mercury were drawn out in a strong tension to occupy the space vacated by the mercury,[1] it would follow that, if glass had pores, this tension could not occur (as indubitably it did, according to his own experiments !), because the subtle parts of the air, whose existence, as already indicated, was alleged by some philosophers, would enter through the glass and occupy not only the upper part of the tube above the column of mercury, but also the whole tube—which was manifestly contrary to experience.

Moreover, proceeded Chérubin, he must not omit the evidence continually provided by chemical experiments,—evidence none the less valuable because it was so ordinary, indeed more convincing since it was universally recognised,—namely, that chemists used

[1] Chérubin, though himself a Plenist, did not accept the views of the philosophers referred to in this and the preceding paragraph. His explanation, as quoted above, resembled in some respects the Funicular hypothesis already advanced by Franciscus Linus in his *Tractatus de Corporum Insepara-bilitate* (London, 1661). Chérubin however wished to make it plain to his readers that his first knowledge of this work was gained after his own book was already licensed for printing (*Effets*, Preface). He had himself verified Huygens's experiment, using mercury that had been freed from air by an exposure of 3 or 4 days in the receiver of the air-pump : and the different behaviour of this mercury in a Torricellian tube from that of common mercury in the same tube convinced him that the lightest and most subtle parts of the mercury had been removed under the pump and hence there were none such to draw out to fill the space vacated by the mercury and therefore to allow it to fall, like common mercury, to a height of 27½ inches (*ibid.*, pp. 64 ff.).

glass vessels to extract the most spirituous and most subtle essences, and to preserve them " under the seal of Hermes " without any diminution either in their quality [1] or in their weight, whereas, if glass had pores, these essences would be emitted and transpired through such vessels. And this with greater reason in the long and violent operation of the fire that was necessary in similar chemical operations, because, if glass had pores, the vehement heat of the fire must necessarily open them ; and these quintessential spirits, highly rarefied by the violence of the fire and driven impetuously from the centre of the glass vessels towards the circumference, where the pores were thus opened, must unavoidably escape from them. Consequently all such chemical operations would be vain and purposeless, because the glass vessels would not be capable of retaining the spirits. Experiment invariably contradicted this and showed it to be false ; and therefore glass had no pores.

As for the opinion of the vulgar, who, observing that glass vessels containing essences were often moist on the outside, concluded thence that the essences had penetrated the glass, this objection, arising only from ignorance, did not merit a reply, since the intelligent were not unaware that the moisture observed on such glass vessels had none of the properties of the essences contained therein, being nothing but the humid vapour of the circumambient air condensed on the glass by the cold.

Chérubin then went on to discuss the transmission of light through glass, in which process the pores of glass, if there were any, must, he thought, serve as paths for the rays of light in their passage. It was established, he said, firstly, that the refraction of the rays was always proportional to their incidence, and, secondly, that the incidence of the rays on a given glass varied with the position of the light. Now, if glass had pores, completely penetrating it and serving as paths for the transmitted rays of light, it followed, firstly, that these pores must regularly follow in their direction through the glass the same inclination as the transmitted rays of light in order that refraction should always be proportional to inclination, and, secondly, that they must always be changing their direction in the glass according to the position of the light. Each of these was clearly impossible. As for the first, if there were any pores in glass, they would occur there only in a random arrangement and not in regular order, whence, if the rays followed the direction of the pores, their refractions could never be proportional to their inclinations, which was contrary to observation : and, as for the second, it was both impossible and ridiculous to

[1] *Quantité* in original, but there are many misprints in the book.

imagine that the pores could at need change their situation in the body of hard and inflexible glass to accommodate themselves to the various positions of the light. Consequently, glass had no pores even for the rays of light that passed through it.

Indeed, if glass had pores transmitting light in the way referred to, their relative positions being unchangeable by any shape that could be given to the glass, it would necessarily follow that a perfectly plane glass, which transmitted the rays of the sun as parallels, would never be able, if cut in any convex form, to transmit those rays except as parallels, just as it did when plane, and therefore could not assemble them by refraction into a focus or burning-point. Again a conclusion contrary to experimental fact was reached, and therefore once more it was clear that glass had no pores penetrating from surface to surface. And moreover, if light could not penetrate glass other than by pores which were really openings, it necessarily followed, since experience proved that light penetrated glass in all directions and in all its parts, that glass would be all pores and openings and therefore without any solidity or firmness, which was false and absurd.

Chérubin now proceeded [1] to deal with the results announced by Boyle in his *Discovery of the Perviousness of Glass etc.*, which we have mentioned above. He recognised that Boyle was very far from believing that glass was easily permeable, since Boyle had written that he was " not at all of their mind, that think Glass is easily penetrable, *either*, as many do, by Chymical Liquors ; *or*, as some, by Quicksilver ; *or*, as others, at least by our Air : Those opinions not agreeing with the Experiments I made purposely to examine them, as you may find in another Paper " (*Discovery of the Perviousness of Glass etc.*, London, 1673, p. 68).[2] As for Boyle's contention that the gain in weight of tin and lead, heated in the

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[1] The remaining part of the *Appendice*, from this point to its conclusion, is what was reproduced, with slight introductory amendment, by Gobet in 1777 in his edition of Rey's *Essays*.

[2] In referring to this tract, the fifth tract in the *Essays of Effluviums*, already mentioned, it should be noted that the *Essays* are not paginated continuously. Boyle took care to make it clear that he wished to make out no more than that glass was porous " to some of the Ponderable parts of Flame." The other " Paper " he mentions here is probably his *New Experiments Physico-Mechanicall, touching the Spring of the Air, and its Effects, etc.* (Oxford, 1660), on pp. 276–7 of which he argued that glass was impervious to air, and on p. 279 of which he stated that the perviousness of glass to air was " contrary to the testimony of a thousand Chymical and Mechanical Experiments." Much later Boyle reiterated these opinions in his *Experiments and Considerations about the Porosity of Bodies* (London, 1684, pp. 119–45).

fire in hermetically sealed retorts for two hours, indicated that glass was penetrable, Chérubin held that the experiments proved the very contrary, namely, that glass was impenetrable. Quoting Boyle's own description of an experiment (*ibid.*, pp. 64–5),[1] in which 1 ounce of lead was heated for two hours in a sealed glass retort, upon the opening of which air was heard to rush in and a gain in weight of 6 grains observed, the metal being partly converted into calx, Chérubin held that Boyle had assumed that, since the enclosed metal could not have received any increment of weight except by penetration of the glass, the observed increment must have been due to the penetration of the flame through the glass, which he (Chérubin), on the contrary, did not think at all possible. Indeed, he argued, Boyle's own words and results proved it to be incorrect ; for, in order to prove the permeability of glass by this experiment, Boyle should have weighed the retort intact with its contents before he opened it, and then re-weighed it with all the fragments and the contents after opening it, to find out whether the external air, which rushed in with a noise when the retort was opened, had caused any change in the weight observed before opening. If he had found these weights identical, Boyle could then have concluded legitimately that glass was pervious, but without these data he had no positive proof of it. A similar criticism of Boyle's method was, as we have noted above, made by Lavoisier in 1774 in more historic circumstances.

Chérubin went further and contended that, if this procedure had been followed, Boyle would doubtless have found that the weight of the retort, etc., was greater after it was opened. The audible inrush of air on the opening of the retort proved, he argued, not only that the vessel was sound and intact (as Boyle had concluded), but also that the air inside it had been very highly rarefied by the violence of the fire which had consumed, in the partial calcination of the metal, all the humid and heterogeneous parts of the enclosed air. It followed that this air, on cooling, had then become strongly attenuated and had therefore changed in its consistence and its " spring " : it no longer pressed outwards, but was drawn in upon itself. It was therefore in a state in which it attracted the external air strongly when the seal was broken, as the noise heard by Boyle plainly proved. Moreover, it was widely known that all calcined bodies had a very strong attraction for air ; and here, to counter, as Chérubin said, one learned Englishman with another, he referred to Digby, who had asserted that his experiments proved that calcined bodies showed a notable increase of substance by the

[1] *Cf.* author's paper in this journal, 1934, **29**, 263, Experiment III.

attraction of air.[1] And this, added Chérubin, was proved by Boyle's own experiments ; for the metal calcined in the sealed retort had been not only deprived of all humidity, but also imbued with a very ardent igneous quality which led to its becoming impregnated with the moist parts of the external air at the moment when, the neck of the retort being broken, it was enabled to exert its attraction upon them. The gain in weight observed by Boyle was evidently produced in this way : and Boyle was plainly mistaken in his opinions about the penetration of flame and the perviousness of glass. Indeed, concluded Chérubin, the gain in weight of the metal, far from proving the perviousness of glass, proved precisely the contrary, since it was evident that the glass vessel forcibly restrained the air in a state of unnatural attenuation, which could not occur if the glass had pores ; for this attenuated air, which, as was proved by the noise heard by Boyle, strongly attracted the external air, would not suffer such restraint, but would necessarily attract the subtle parts of the external air in order to occupy the capacity of the vessel and recover its free and natural consistence. With this comment, Chérubin concluded the *Appendice*.

Boyle's attention was evidently drawn to Chérubin's criticism, since he defended his opinions in a letter subsequently published in his collected works (*Works of the Honourable Robert Boyle*, ed. T. Birch, London, 1744, Vol. V, pp. 233–4). The date of this letter and the name of the person to whom it was addressed are alike missing, but the contents throw interesting light upon Boyle's attitude towards the theory that the gain in weight of metals on calcination was due to the addition of matter from the air. In the letter Boyle stated that he would not imitate Chérubin's " dogmatical way of writing," but would consider only his criticisms, and that it did not seem to him that his argument was " disabled " by Chérubin's supposition that the " stretched air " in the sealed retort strongly attracted the neighbouring air (an attraction which he did not admit) when the retort was opened.

I think I may be pardoned [he wrote] if I did omit the weighing the retort, and all that was in it, before I broke it ; for I did not then know,

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[1] Sir Kenelm Digby, *A Late Discourse made in a Solemne Assembly of Nobles and Learned Men at Montpellier in France*, trans. R. White, London, 2nd edition, 1658, pp. 36–7 and 66. According to the *D.N.B.*, this edition, though described on its title-page as the second, is the earliest one known and is probably the original. The French edition appeared in 1658. Digby referred merely to the gain in weight of salt of tartar (potassium carbonate, produced by calcining wine-lees) on exposure to air.

that any body was like to make the exception the P. *Cherubin* has proposed ; and I thought it would be tedious to the reader, as well as to me, to set down all the imaginable objections, that could possibly be devised : and though this, that is made, had been actually suggested to me, only as a possible doubt, though not as the grand objection of a learned man, I should yet perhaps have saved myself the labour of solicitously clearing it, because, the circumstances of the experiment seemed sufficient to justify the conclusion inferred from it, as will by and by appear. And one, that were a sceptick, and would have proposed all the suspicions, that might have been devised, would perhaps have acquiesced in such a trial as my censurer declares would have satisfied him, because of another jealousy, that came to my mind, when I was conversant about the experiment. For, on this occasion, I must let you know, for a certain reason, that needs not now be told, I remember I did weigh a sealed retort, with matter in it, and found it encreased in weight, but thought not fit to lay much stress on that circumstance.

This malobservation of the increase in weight of a sealed retort was unfortunate ; for it confirmed Boyle's explanation. It is tempting to speculate as to what theory Boyle would have put forward if he had made correct observations here, but such is not the part of the historian. As for Chérubin's reference to Digby, Boyle found it curious, as appears below, and, in disposing both of Chérubin and of Digby, he reveals to the modern reader how far he and his contemporaries were from any suspicion of the true rôle of air in calcination, since his examination of the theoretical possibility takes into account only the humid, *i.e.* the heterogeneous, parts of the air. He wrote here :

As for the authority of Sir *K. Digby*, I am glad my censurer has so much deference for it, since that famous knight relates, that he had made quicksilver pass in small drops through glass itself; which, if true, will quite destroy P. *Cherubin*'s denial of the porosity of glass : but, for a reason I have elsewhere declared, I will lay no weight on this ; but shall consider, wherein the strength of the censurer's objection lyes ; namely, that by Sir *K. Digby*'s experiments it appears, that calcined bodies do notably augment in substance, by the attraction they make of the air ; to which I briefly answer, that if the P. were as well versed in chemistry, as he is in opticks, he would not probably have looked upon this objection as irrefragable ; for he will give one, that is a piece of a chemist, leave to tell him, that calcined bodies may be of very differing kinds, and that, though many of them, especially vegetables, being calcined, do in time increase in weight, by the insinuation made into the pores of their more saline parts, not by the simple air, but by the moist vapours that rove up and down in the air, and easily adhere to, and penetrate lixiviate salts ; yet there are other calcined bodies, that are of a very different condition, such as are the calces of metals, at least of tin and lead ; in which neither taste, or for ought I have observed, any other means, has yet manifested a lixiviate salt, like that of burned vegetables. And therefore it need not seem strange, that though calcined tartar, left in a moist air, will in time thereby be resolved to a liquor, yet

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no such thing will happen to the ashes or calces of burned tin or lead ; so that it is very improbable, that, in a few minutes, those metals, though but partly calcined in my retorts, should from air gain so great an increase of weight ; especially considering, that there is another manifest cause to be assigned for that increment, divers experiments purposely made having assured, that either of those metals being calcined, will make a notable acquist of weight upon the operation of the fire. And I opportunely remember, that this very year, having had occasion to melt together, and steep for a pretty while in fusion, a mixture of tin and copper, amounting in all to a pound (I say just, because, though I dreamed not I should make this use of the experiment, yet the use I did design required all things should be very carefully weighed,) I found, that though a small portion of the mixture was calcined, yet the total had gained in weight no less than 233 grains.

With this, Boyle concluded his answer to Chérubin's criticism, ending his letter with the comment " that in the P. *Cherubin*'s objections, I have yet seen no reason to do what perhaps few are more disposed to do, alter my former opinions, his against them seeming to me to have more of confidence than strength."

This letter does at least provide us with evidence as to how far Boyle had considered the possibility that metals took up air on calcination : he denied that any bodies took up " the simple air," while " vegetables, being calcined " admittedly took up " moist vapours " from the air, but metals could not acquire their great increase of weight from air after a calcination extending over a mere few minutes. Boyle's reluctance to admit this absorption of air may at first appear to some extent curious, since it would seem that he should have been very ready to consider its presence as " simple air " in the pores of various substances ; for he appears to have been the first to collect " air," as he called it, from the interaction of an acid and a metal, in a glass vessel inverted over water, previously filling the vessel with " Oyl of Vitriol and fair water, of each almost a like quantity, and casting in half a dozen small Iron Nails," repeating the experiment with *aqua fortis* in place of oil of vitriol, and thereby initiating the method of collecting gases by the displacement of water. But Boyle's conclusion from these experiments went no further than recognizing that " in general air may be generated anew " and that such a production of air might occur through the differing agitation or re-arrangement of the minute parts of other bodies (*New Experiments Physico-Mechanicall, touching the Spring of the Air, and its Effects, etc.*, Oxford, 1660, pp. 176–80).[1]

[1] It was probably Boyle who invented this method of collecting gases, one of the most important practical advances in the history of chemical discovery : he describes it in the work named above, which was published

In conclusion, the author wishes to express his thanks to Professor E. N. da C. Andrade, D.Sc., F.R.S., Quain Professor of Physics in the University of London, for his kindness in lending a copy of the 1688 edition (Lucas) of the *Effets* for the preparation of this paper ; to the Librarians of University College and the National Central Library, London, and of the Bibliothèque Nationale, Paris, for their assistance ; and to M. A. Lacroix, Secrétaire perpétuel de l'Académie des Sciences, for answering various enquiries.

in 1660, and he had evidently devised and used it earlier than this, since his book, "written by way of Letter to the Right Honorable Charles Lord Vicount of Dungarvan," as the title-page states, is subscribed on its last page (p. 399) "*Becon's-field* this 20th of December, 1659."

THE MOLECULAR STRUCTURE OF CELLULOSE AND STARCH

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THE STRUCTURE OF POLYMERS—LARGE OR SMALL MOLECULES ?

ON account of their great biological and industrial importance, cellulose and starch have long engaged the attention of chemists, and numerous structural formulæ have been assigned to them. The two carbohydrates were early recognised to be polymerised materials related to glucose, and the earliest attempts to formulate their constitutions were made within the framework of the ordinary structural theory of organic chemistry ; the structural units of the molecules were assumed to be united by primary valency linkages to form open or closed chains. In the course of time, however, there grew up a new conception of the nature of polymerisation according to which both the natural and synthetic polymers were composed of small molecules held together by means of residual valency forces. This association theory reached its zenith in the field of the natural polymers between 1920 and 1926, when under the influence of Karrer, Hess, Pringsheim and Bergmann, cellulose and starch were regarded as consisting of small molecules such as, for example, anhydrides of glucose, bioses or trioses. The various forms of this theory were in many cases supported by molecular weight determinations by the freezing-point method, but the conclusions arrived at regarding the molecular weight of cellulose and starch are now considered to have been vitiated by the unreliability of the method, and by the fact that the materials used, although supposed to be merely physically depolymerised, had actually suffered severe chemical attack. Further support was obtained from the results of X-ray investigations, since it was thought that the small unit cell found for the crystal lattice of cellulose necessitated a small molecule. This view was abandoned by Sponsler and Dore [1], who by making use of available chemical evidence regarding the structure of glucose, showed how the X-ray fibre diagram

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of ramie cellulose could be explained by a structure consisting of chains of glucose residues arranged parallel to the fibre axis ; the glucose residues were linked by primary valencies, so that each chain was a very long molecule. The model of the structure of cellulose suggested by Sponsler and Dore, although of great importance in indicating a new method of attacking the problem, was not in accord with the chemical evidence as to the mode of linking of the glucose residues and this defect was remedied in the model proposed by Meyer and Mark [2] in 1928. The constitution of cellulose embodied in this model has since been strongly confirmed by further chemical evidence and is now almost universally accepted. Since starch does not give a fibre diagram, X-ray analysis has made no contribution to the elucidation of its structure, but by the application of chemical methods of investigation similar to those used with cellulose, it has been concluded that starch also has a molecular chain structure. Concurrently with these developments the investigations of Staudinger [3], Carothers [4] and others on synthetic polymers have shown conclusively that a large class of these substances, the so-called linear polymers, consist of long molecules built up by the combination of small repeating units through the operation of primary valencies. Other types of polymers are known in which the molecules have a three-dimensional, instead of a linear, structure, but here also the structural units are bound by primary valencies. The resemblances between the physical properties of the natural polymers and the synthetic linear polymers of high molecular weight are so striking that there can be little doubt that cellulose and starch are also linear polymers.

CHEMICAL CONSTITUTION

Cellulose and starch resemble each other in having the composition expressed by the formula $C_6H_{10}O_5$ and in being completely convertible into glucose by acid hydrolysis. They also possess three hydroxyl groups per glucose residue, as shown by the maximum degree of ester or ether formation of which they are capable, and the fact that both trimethyl-cellulose and trimethyl-starch yield 2 : 3 : 6-trimethyl-glucose on hydrolysis indicates that these hydroxyl groups occupy the 2 : 3 : 6 positions. Under certain conditions the two polysaccharides may be partially converted into disaccharides, and it is here that the first indication of a constitutional difference is found ; whereas cellulose gives rise to cellobiose, starch yields maltose. A knowledge of the chemical constitutions of glucose, cellobiose and maltose is thus fundamental to an understanding of the structure of cellulose and starch. Haworth [5] has shown that

the normal form of glucose should be represented as a six-membered (pyranose) ring, and that the structural relation of cellobiose and maltose to glucose is that indicated by the following formula (I), which shows that both disaccharides consist of two glucose residues united by a glucosidic linkage between the first carbon atom (reducing group) of one residue and the fourth carbon atom of the other.
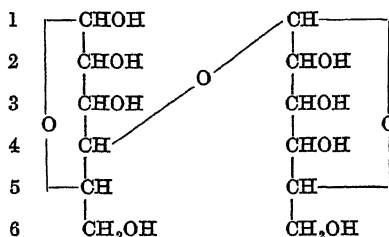
I. MALTOSE AND CELLOBIOSE

The sole constitutional difference between cellobiose and maltose is a difference in configuration at the linkage between the glucose residues similar to that which distinguishes the α- and β-stereoisomeric forms of methylglucoside ; the cellobiose linkage is of the β type, that of maltose of the α type. This difference is not shown by Formula I, which is written without regard to stereochemical considerations, but may be seen from the perspective formulæ of Haworth (II and III).
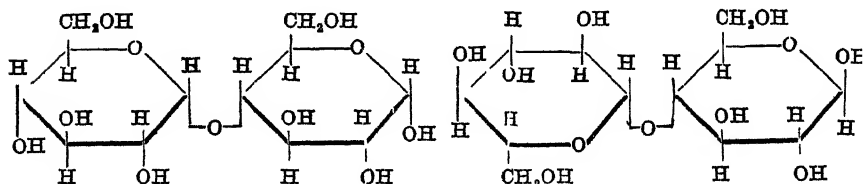
II. MALTOSE (α- FORM)   III. CELLOBIOSE (β- FORM)

On the basis of these accepted constitutions for cellobiose and maltose the constitutions of cellulose and starch may be formulated in the following way :
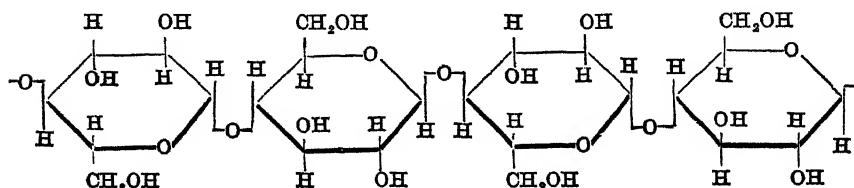
IV. CELLULOSE
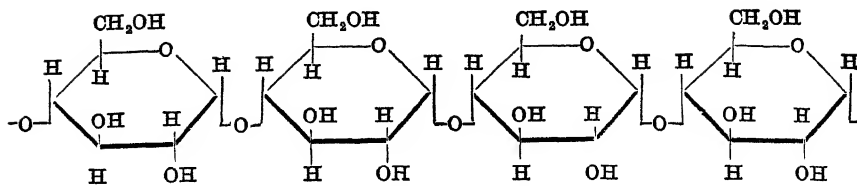
V. STARCH

These formulæ, which represent long chains of glucose residues bound by β- and α- linkages respectively, are amply confirmed by all the evidence available. For example, the length of the cellobiose unit is exactly that of the period of identity of the cellulose crystal lattice in the direction of the fibre axis, and the model of the structure of cellulose proposed by Meyer and Mark [2], in which the molecular chains consisted of glucose residues linked as in cellobiose, was found to account satisfactorily for all the details of the X-ray diagram. Confirmation of the structures has also been obtained by the isolation from cellulose and starch of sugars, or methylated sugars, containing three and four glucose residues linked as in cellobiose and maltose. The study by Freudenberg [6] of the optical rotatory powers of the methylated sugars—bioses, trioses and tetraoses—obtained from cellulose and starch and of their relation to the rotatory power of trimethyl-cellulose and trimethyl-starch has so far provided the most convincing evidence that all the linkages in cellulose and starch have the β- and α-configuration respectively. Freudenberg and Kuhn [6] have also investigated the kinetics of the hydrolysis of cellulose, starch and some of the sugars related to them, and the results obtained are consistent with the chain structure ; they also show that starch is more easily hydrolysed by acids than cellulose, in agreement with the greater ease of hydrolysis of maltose when compared with cellobiose. A final confirmation of the chain theory has been provided by the detection of the end-groups of the molecular chains. One of the terminal units of a chain of glucose residues bound by glucosidic linkages should be distinguished by the possession of four alcoholic hydroxyl groups, and Haworth [7] has proved the presence of such terminal residues by the isolation of a small proportion of tetramethyl-glucose from the hydrolysis products of fully methylated cellulose and starch. The glucose residue at the other end of the chains should, according to the theory, bear a reducing group, and hence cellulose and starch would be expected to show reducing power to a degree depending on the average length of the chains. This expectation is borne out by experi-

ment; although cotton cellulose that has been freed from non-cellulosic constituents by boiling with dilute alkalis has an extremely low reducing power, this is partly due to the destruction of reducing groups by the alkaline treatment and the frequently reported failure to detect reducing properties in starch has been shown to result from a defective method of measurement [8]. Progressive hydrolysis, which according to the molecular chain theory results in production of shorter chains, is with both materials marked by a progressive increase in reducing power [8, 15].

THE HETEROGENEITY OF CELLULOSE AND STARCH

It is a characteristic of linear polymers that with the exception of those of very low degree of polymerisation (number of structural units in the molecular chain) they are never obtained as homogeneous substances of definite molecular weight, but are always composed of chain-molecules of similar structure and varied chain-length; in the phraseology of Staudinger [3] they are mixtures of "polymer-homologues." Within a given polymer-homologous series the physical properties are found to vary continuously with increasing chain-length, but when the degree of polymerisation is high the properties of consecutive members of the series are not sufficiently different to permit the isolation of the polymer-homologues as chemical individuals. In such cases separation of a polymeric product into fractions differing in average chain-length is the most that can be achieved. Staudinger [3] has shown that the viscosity of solutions of linear polymers belonging to a given series is a function of the chain-length and measurements of viscosity provide the simplest method of comparing the average chain-lengths of different materials from the same polymeric series.

The heterogeneity of cellulosic materials has often been demonstrated. It has been shown in numerous investigations that by means of fractional dissolution or precipitation, cellulose esters may be separated into fractions differing in viscosity and some other physical properties, but having approximately the same degree of esterification. These observations find a simple explanation in terms of the molecular chain theory; the various fractions differ in average chain-length, but in every chain, whatever its length, the same proportion of the hydroxyl groups has been esterified. Celluloses that have suffered some shortening of their molecular chains as the result of chemical attack are soluble in sodium hydroxide to a greater or less extent, and the heterogeneity of such materials is clearly shown by the fact that when various weights of a "chemically modified" cellulose are extracted with a given volume of

alkali solution, the proportion of the material dissolved is constant. By varying the conditions in successive extractions it is possible to separate a modified cellulose into fractions of different viscosity, and it is found that the more easily a fraction is dissolved the lower is its viscosity [9].

In starch two main constituents, now usually called amylose and amylopectin and differing in solubility, osmotic pressure and viscosity, have been described in the literature. It has been claimed that these constituents show structural and stereochemical differences, but recent investigations lend no support to this view. Natural starches may contain small proportions of mineral and fatty material, and some investigators have maintained that the differences between amylopectin and amylose are due to a higher proportion of combined phosphoric or fatty acid in the former ; amylose fractions have, however, been obtained with phosphorus contents as high as that of the potato starch from which they were prepared [7]. There is no agreement concerning the proportions in which amylose and amylopectin occur, even in one variety of starch, and by varying the pre-treatment of the starch or the conditions of extraction of the amylose, the fractions may be obtained in almost any proportions, so that they cannot be regarded as definite substances or as fractions of uniform composition. According to Haworth [7] and his co-workers, fully methylated amylose and amylopectin, although exhibiting differences in viscosity similar to those of the unmethylated materials, yield the same proportion of tetramethyl-glucose on hydrolysis and for this reason their molecular chains are believed to be equal in length. On the other hand, it has been shown that the reducing power of amylose is higher than that of amylopectin [8], and this constitutes strong evidence that, prior to methylation, fractions differing in solubility differ also in average chain-length.

A solution of amylose on standing at a low temperature is partially precipitated and the precipitate is frequently difficult to re-dissolve. This change is known as retrogradation. A similar change takes place in solutions of starch that have been modified by the action of acid, and here the precipitated fraction has been shown to consist of chains longer than the average of the material as a whole [8]. From this it has been inferred that the retrogradation of amylose is due to precipitation of the longer, less soluble chains, the solubility of which is exceeded when the solution is cooled.

THE LENGTH OF THE MOLECULAR CHAINS

Many estimates have been made of the lengths of the chain-molecules in cellulose and starch, and the results of different investigators have often shown considerable divergences. This is not surprising when it is remembered that the terms cellulose and starch do not denote substances of a definite molecular weight, and that the average length of the chain-molecules may be greatly changed by a very slight degree of chemical attack ; this latter consideration is particularly important when, as is often the case, the degree of polymerisation of cellulose and starch is inferred from the observed molecular weights of their esters or ethers. Moreover, there is at present no adequate basis for assessing the reliability of the various methods that have been employed.

The values recorded for the chain-lengths of celluloses lie between 200 and 1300 glucose units ; the higher figure is that found by Kraemer and Lansing [10] by means of the Svedberg ultracentrifuge, and the lower was deduced by Haworth [7] from the chain-length of trimethyl-cellulose as determined from the yield of tetra-methylglucose. The viscosity method of Staudinger [3] has yielded values of 750–1000 ; this method is not however an absolute one, and the accuracy of the results obtained must depend on the reliability of the method used to standardise it. Measurements of the osmotic pressure of cellulose esters indicate very varied degrees of polymerisation [10], but these are in general of the same order of magnitude as the values found for cellulose. In the present state of knowledge all that can safely be concluded is that the molecular chains of natural celluloses contain at least several hundred glucose units.

There is still greater uncertainty concerning the average chain-length of starch. A single determination in the ultracentrifuge indicated a molecular weight of nearly one million, equivalent to more than 5000 glucose units [11]. By an improved method of measuring reducing power [8] the average chain-lengths of a number of unmodified starches were found to lie between 400 and 1500, and values within a similar range have been obtained by osmotic pressure measurements [12]. On the other hand, Haworth [7] has found by his end-group method that methylated starches of various origins all have chain-lengths of only 25–30 glucose units, in spite of having different viscosities [7] and showing high and varied molecular weights by the osmotic pressure method [13]. These observations have led him to the conclusion that the maximum length of the primary valency chains in starch is 25–30 glucose

units, and that these chemical molecules may, by association side by side or end to end, form much larger aggregates, the size of which is measured by the physical methods [14]. On this view, the difference between amylopectin and amylose is a difference in degree of aggregation, and the retrogradation of freshly prepared amylose is regarded as an example of the re-formation of aggregates following a disaggregation process. This hypothesis has also been applied to cellulose, although the need for it here is not apparent from the published results. Whether it provides the true explanation of the results obtained with starch only further investigation can show.

PHYSICAL PROPERTIES

Although cellulose and starch are so nearly allied chemically, they are very dissimilar in their physical properties. These differences must be inherent in the molecular structures of the two substances. It has been seen that their constitutions are distinguished only by the presence of β-linkages in cellulose and of α-linkages in starch, and hence the physical differences must be due to the different conformations of the molecular chains. The construction of three-dimensional models of the structures shows that whereas with β-linkages the chains may be given an almost straight form, this is not possible with the α-linkage, which gives rise to chains of pronounced zigzag or spiral form. This difference has important consequences, since it permits much closer packing of the chains in cellulose than in starch. The X-ray diagram obtained with a bundle of ramie fibres placed at right angles to the X-ray beam is similar in type to that obtained with a single crystal rotating about a crystallographic axis, and shows that in this fibre the cellulose chains are arranged parallel to each other and to the fibre axis. Such an arrangement accounts for the great strength of cellulose fibres ; in order to break them it is necessary either to break the primary valency chains or to produce slipping of the chains over each other against the lateral cohesive forces between them. The strong cohesion between parallel chains also accounts for the insolubility of cellulose in water, and for the fact that celluloses of sufficiently great chain-length may be highly swollen by alkalis without dissolving. In starch, on the other hand, the zigzag form of the molecular chains prevents any considerable degree of crystallinity, so that only a rather diffuse " powder " diagram can be obtained from X-ray examination. The impossibility of orientation of the chains and the consequent low degree of cohesion between them account for the failure of starch to form fibres, and

for the brittleness of the films prepared from it ; they also explain why starch is more hygroscopic than cellulose, and why it can be dispersed into solution by boiling with water.

THE PROPERTIES OF CHEMICALLY MODIFIED CELLULOSES AND STARCHES

On account of their great importance to the textile industry the changes in the chemical and physical properties of cellulose fibres that result from slight chemical attack on the cellulose have often been studied, and in particular they have formed the subject of systematic investigations in the laboratories of the British Cotton Industry Research Association [15]. These investigations have revealed a number of relations between the various properties, which have proved of the utmost value in the control of the chemical processing of cotton and in the assessment of the degree of chemical modification of cotton cellulose, but which are very difficult to interpret in terms of any of the older theories of the structure of cellulose. It is one of the merits of the molecular chain theory that it explains much of the behaviour of the chemically modified celluloses that was formerly obscure [9].

The action of acids on cotton cellulose (formation of so-called " hydrocellulose ") is marked by an increase in reducing power, a fall in tensile strength and in viscosity in cuprammonium solution, and an increase in solubility in cold sodium hydroxide solution, and it is found that the inter-relations of these properties are always the same whatever the conditions of the acid treatment. According to the theory, the action of acids consists in the hydrolysis of glucosidic linkages in the chain-molecules, with the production of shorter chains and the liberation of reducing groups. The hydrolytic attack occurs at points randomly distributed along the chains, so that as hydrolysis proceeds the frequency distribution of chain-length is progressively altered in the direction of lower average chain-length. Thus the frequency distribution is uniquely determined by the reducing power, which is a measure of the proportion of the linkages that have been broken. The viscosity and tensile strength are related to the chain-length, and the solubility is determined by the proportion of the material that consists of chain-molecules below a certain length ; consequently these three properties are all functions of the chain-length frequency distribution, and hence show definite relations to the reducing power and to each other.

The behaviour of cellulose towards oxidising agents is much more complicated, for the chemical properties of the products

("oxycelluloses") corresponding to a given oxygen consumption depend on the oxidising agent employed. The oxidation process always leads to a loss of strength, a fall in viscosity, and an increase in solubility, showing that chain-molecules are broken, but although in any given series of oxycelluloses these properties show definite relations to each other and to the chemical properties, the relations vary from one type of oxycellulose to another. Progress in this field is hampered by lack of knowledge regarding the organic chemistry of the oxidation processes, but by the study of the inter-relations of the properties of the oxycelluloses in the light of the molecular chain theory a better understanding of these substances is gradually being obtained [9].

The chemical modification of starch has been much less extensively studied than that of cellulose, but it has been shown that, in accord with the molecular chain theory, the action of acid results in an increase in reducing power and solubility, and a fall in viscosity [8]; the so-called "soluble" starches differ from natural starch in having a lower average chain-length. It is probable that a more systematic investigation of the chemically modified starches would be of great value in helping to clear up such controversial questions as the relation of amylose and amylopectin to each other, and the nature of the retrogradation process.

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RECENT ADVANCES IN SCIENCE

MATHEMATICS. By J. H. C. Whitehead, M.A., Balliol College, Oxford.

Combinatorial Analysis Situs, I.—Topology, or analysis situs, is the geometry of continuity. That is to say notions like distance, angle or straightness do not appear, two spaces being regarded as equivalent if one is the image of the other in a (1–1) continuous transformation. Thus the surface of a cube is topologically equivalent to the surface of a sphere but not to a torus (*i.e.* the surface of a ring). To exhibit the distinction between a sphere and a torus one can show that any simple closed curve on a sphere separates it into two regions. On the other hand a meridian circuit on a torus does not separate it into two regions, and is therefore called a non-bounding circuit. The fact that non-bounding circuits can be drawn on a torus, but not on a sphere, is a topological distinction. One can also express this distinction in terms of intersections. For one can draw two circuits on a torus, a meridian and a longitudinal circuit, which cross each other at a single point. If there was a (1–1) continuous transformation of the torus into a sphere these circuits would correspond to a similar pair of circuits on the sphere, which is an absurd conclusion.

The business of a topologist is to investigate the various kinds of n-dimensional space, drawing distinctions analogous to that between a 2-sphere and a torus, and, above all, to recognise topological equivalence through all manner of disguise.

In combinatorial analysis situs one conceives a space not as a collection of points but as a mosaic, generally called a complex, made up of cells. No two cells overlap but two k-dimensional cells, or k-cells, may meet in one or more cells of lower dimensionality on the boundary of each. A k-cell is to be thought of as a block of k-dimensional space, topologically equivalent to the region in k-dimensional Euclidean space given by $0 \leqslant x_\lambda \leqslant 1$, $0 \leqslant x_1 + \ldots + x_k \leqslant 1$ ($\lambda = 1, \ldots, k$). Some writers think of a cell as the open region defined by the strict inequalities, but we shall imagine a cell as containing its boundary. However, the cells

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of the combinatorial theory are the primitive geometrical objects and so are undefined.

More precisely, a complex is a collection of undefined 0-cells, 1-cells, ..., n-cells, together with a function of cells $F(E)$, defined for each cell E. If E_k is a k-cell $F(E_k)$ is an aggregate of $(k\text{-}1)$-cells and is called the boundary of E_k. A collection of cells of the same dimensionality is called a chain and is generally written as a sum

$$C_k = E_k^1 + \ldots + E_k^p \qquad . \qquad . \qquad . \qquad (1)$$

The boundary of C_k is defined in terms of $F(E)$ as

$$F(C_k) = F(E_k^1) + \ldots + F(E_k^p).$$

Thus $F(C)$ is a linear function of chains. If there is a $(k+1)$-chain C_{k+1}, such that

$$F(C_{k+1}) = C_k$$

we write

$$C_k \sim 0 . \qquad . \qquad . \qquad . \qquad . \qquad (2)$$

and say that C_k bounds, or is " homologous " to zero.

We now introduce the concept of orientation. With each cell E_k we associate two " oriented " cells E'_k and E''_k, the oriented cells being new undefined entities. One of these, it does not matter which, but say E'_k, we describe as positively oriented and the other as negatively oriented. An oriented chain is defined as a sum of oriented cells and we now simplify our notation by writing E_k for E_k' and $-E_k$ for E_k''. Then any oriented chain is a sum of the form

$$C_k = e_1 E_k^1 + \ldots + e_p E_k^p,$$

where $e_i = \pm 1$, and we see that there are two oriented chains $\pm C_k$, associated with each unoriented chain C_k. We write

$$F(-E) = -F(E),$$

and the symbol $C_k \sim 0$ will mean the same for oriented as for unoriented chains. From now until further notice it is to be assumed that all chains to which we refer are oriented.

The cells composing a chain need not be distinct and we have the more general definition of a chain

$$C_k = \sum_{i=1}^{p} \lambda_i E_k^i,$$

where $\lambda_1, \ldots, \lambda_p$ are integers which may be positive, negative or zero. We write

$$0 . E_k = E_k - E_k = 0$$

and $C_k + 0 = C_k$, where C_k is any chain. Thus for each value of k the totality of k-chains is a lattice, or free Abelian group, of which the k-cells form an independent basis.

A chain whose boundary is null is called a cycle, and from the linearity of the function F(C) it follows that the k-cycles constitute a free sub-group of the group of all chains. We now subject F(C) to the final condition

$$F[F(C)] = 0.$$

That is to say every bounding chain is a cycle.

If

$$C_k^i = F(C_{k+1}^i) \ (i = 1, \ \ldots, \ p)$$

it follows from the linearity of F(C) that

$$\sum_{i=1}^{p} \lambda_i C_k^i = F(\sum_{i=1}^{p} \lambda_i C_{k+1}^i).$$

Thus the bounding k-cycles constitute a sub-group of the group of k-cycles. If E_{k+1}^1, E_{k+1}^2, $\ldots$ are the $(k + 1)$-cells the k-chains

$$F(E_{k+1}^1), \ F(E_{k+1}^2), \ \ldots$$

constitute a basis for the group of bounding k-cycles. For if $C_k = F(C_{k+1})$, where

$$C_{k+1} = \Sigma_i \lambda_i E_{k+1}^i,$$

we have

$$C_k = \Sigma_i \lambda_i F(E_{k+1}^i).$$

To simplify matters, let us assume that there are, in all, a finite number of cells. By a simple arithmetical process one can then find a basis

$$C_k^1, \ \ldots, \ C_k^p, \ C_k^{p+1}, \ \ldots, \ C_k^{p+q}, \ C_k^{p+q+1}, \ \ldots, \ C_k^{\alpha_k},$$

for the k-cycles such that

1. $C^i \sim 0 \ (i = 1, \ \ldots, \ p)$,
2. no linear combination of $C_k^{p+1}, \ \ldots, \ C_k^{p+q}$ bounds,
3. $\zeta_s^{(k)} C_k^{p+q+s} \sim 0$, where $\zeta_s^{(k)}$ are certain integers exceeding unity,

and no linear combination of the C_k's bounds except in consequence of these relations. The number q is called the kth Betti number of the space and is generally written B_k. The numbers $\zeta_s^{(k)}$ are called the k-dimensional coefficients of torsion. Of course, either or both of the last two sets of chains may be empty, in which case we say that $B_k = 0$ and that there is no torsion.

The simplest example of a space with torsion is a projective plane. Topologically speaking, a straight line in a projective plane is a closed curve and since two straight lines meet in a single point a straight line does not bound. But a straight line does bound if counted twice. One can see this by cutting round the middle of

a Möbius band (*i.e.* a band twisted through π), which is topologically equivalent to a projective plane with a hole in it. The two rims of the cut follow each other round the boundary of the resulting band, and the rim of the original band does not appear in the boundary since it bounds a 2-cell which has been removed from the projective plane. Thus the first Betti number of a projective plane is zero and there is one coefficient of torsion, whose value is 2.

For a torus, $B_1 = 2$, $B_2 = 1$ and there is no torsion. More generally for an orientable, or two-sided surface of genus p we have $B_1 = 2p$, $B_2 = 1$ and there is no torsion. Any one-sided surface has one coefficient of torsion, whose value is 2, B_1 can be arbitrary and $B_2 = 0$.

In this brief sketch of the elements of the combinatorial theory we have not provided for an adequate theory of topological equivalence. Before showing how topological equivalence can be defined in combinatorial terms let us return to the conception of a space as a continuum of points. Further, let us assume that a given space is " covered by a combinatorial complex," meaning that it is cut up into cells which do not overlap but which fit together exactly. Then we can apply the combinatorial theory to this particular complex and calculate the Betti numbers and torsions. Of course the space can be cut up into cells in infinitely many ways and one might expect these numbers to differ between two complexes covering the same space. An important theorem states that they do not. Thus the Betti numbers and torsions are characteristic of the space itself and do not depend on the method of cutting it up into cells. The image of a complex K, covering a space S_1, in a (1–1) continuous transformation of S_1 into a space S_2, is a complex covering S_2. The Betti numbers and torsions are calculated purely from the incidence relations of K and are therefore the same for S_1 and S_2. That is to say they are what are called topological invariants.

The Betti numbers and torsions, together with various extensions of these invariants which are calculated by slight modifications of the methods I have indicated, are called the homology characters of a space. We have seen that two spaces are topologically distinct if their homology characters are distinct. One naturally asks " Are two spaces topologically equivalent if their homology characters are the same ? " The answer is in the negative. To see this let us confine our attention to what are called manifolds. An n-dimensional manifold is a space which is " locally equivalent " to Euclidean space of n dimensions. That is to say each point is contained in an n-cell which is a " complete neighbourhood " of

this point (a more precise definition of a manifold, from the combinatorial point of view, will be given later on). Assuming all our manifolds to be covered by complexes, a closed manifold is one which is a finite cycle (though every cycle is not a manifold). Thus a closed 1-dimensional manifold is a closed curve without double points and a closed 2-dimensional manifold is a closed surface without singularities. Then the situation relative to the homology characters is that a closed 2-dimensional manifold is completely characterised by these invariants, but one can construct distinct 3-dimensional manifolds with the same homology characters. One can even construct closed 3-dimensional manifolds with the same homology characters as the 3-sphere (i.e. the boundary of a 4-cell) which are topologically distinct from the 3-sphere. In outline, one method is this. Take a rubber tube, knot it and then weld the two ends together to form a knotted tube whose surface is a torus S (it is to be understood that this tube is in ordinary Euclidean space, which we have converted into a 3-sphere by the addition of an ideal point at infinity). Let R be the region outside this tube, that is to say the region containing the point at infinity. Let m be a circuit on S which bounds a 2-cell inside the tube. Such a circuit is described as a meridian circuit. One can show that there is a " longitudinal " circuit l, on S, cutting m in a single point, which bounds a surface lying in R. The essence of this construction is that, while l bounds some kind of a surface in R, it does not bound a 2-cell in R, since the tube is knotted. Now remove the inside of the tube, leaving the bounded region R, together with its boundary S, and let T be the tube, detached from R, bounded by another torus S'. Let m' be a meridian circuit on S' and let l' be any circuit on S' which meets m' in a single point. Let $m*$ be a circuit which winds round S' in a spiral so that the chain $m* - l' - m'$ bounds on S'. We can think of $m*$ as a circuit of l' followed by a circuit of m', slightly deformed so as to be a non-singular circuit.

We construct the required manifold M, by mapping S on S' in such a way that l corresponds to l' and m to $m*$, and identifying each point of S with its image on S'. Of course, one cannot do this physically, any more than one can cut up a torus made of elastic membrane into an " elastic " jig-saw puzzle and piece it together again on a flat table-top. We construct the manifold on the principle of a solid jig-saw puzzle, indicating the pairs of 2-cells which are to be regarded as identical. This determines the combinatorial structure, which is all that we care about. Since l' is to be regarded as identical with l, it bounds in M. Also m', and therefore $m*$ bounds. Thus m bounds, since it is identified with

m^*, and therefore any circuit on S bounds. It follows without difficulty from this that every circuit in M bounds. Thus $B_1 = 0$ and there is no 1-dimensional torsion. After proceeding a little further with the study of bounding relations in a 3-dimensional manifold, it appears that the remaining homology characters are also the same as those of a 3-sphere.

The topology of M depends upon the original knot. If it is a clove hitch one can show that M is not a 3-sphere by showing that what is called its fundamental group is of order 120 (it is the so-called double icosahedral group).

The fundamental group of a space was discovered by Poincaré and is defined as follows.

1. To each oriented curve c, beginning and ending at a fixed point 0, corresponds an element $g(c)$ of the group.

2. To the degenerate curve consisting of the single point O corresponds the unit element of the group.

3. To the oriented curve $-c$, the same curve as c with the opposite orientation, corresponds the inverse of $g(c)$.

4. To the curve $c_1 + c_2$, consisting of c_1 followed by c_2, corresponds the product $g(c_1) \, g(c_2)$.

5. If c_1 can be deformed into c_2, 0 being held fixed throughout, $g(c_1) = g(c_2)$.

It is clear from this definition that the groups of two equivalent spaces are simply isomorphic. Therefore the group of a space is a topological invariant. Any closed curve in a 3-sphere can be shrunk into a point, and therefore the group of a 3-sphere consists of the unit element alone. Therefore the manifold M, described above, is not a 3-sphere.

The Betti numbers and torsions of a closed 3-dimensional manifold are determined by its group. So one may ask " If the fundamental groups of two closed 3-dimensional manifolds are simply isomorphic are the manifolds equivalent ? " This question has been answered in the negative by J. W. Alexander (*Trans. American Math. Soc.*, **20**, 1919, 339–42) who discovered a pair of manifolds, each with a cyclic group of order five, which are not equivalent. However, there remains the possibility that the 3-sphere may be the only closed 3-dimensional manifold whose group is unity. It was conjectured by Poincaré that this is the case and the question remains open.

Analysis Situs, as we know it now, was created by Poincaré in a mémoir (*J. de l'École Polytech.*, (2), **1**, 1895, 1–121), to which he added five complements. At the end of the second complement

he announced that any two manifolds were equivalent if they had the same homology characters. He corrected this error himself by giving the example which I have described, though he gave it in an altogether different form. However, he calculated the fundamental group in order to show that the manifold in question is not a 3-sphere. The possibility of constructing these "Poincaré spaces" by means of knots was discovered by M. Dehn (*Math. Annalen*, **69**, 1910, 137–68). Poincaré's method was based on a paper by P. Heegaard (*Bull. Soc. de Math de France*, **44**, 1916, 161–242—translated from Heegaard's dissertation, Copenhagen, 1898). A more recent account of this construction is to be found in a book by H. Seifort and W. Threlfall (*Lehrbuch der Topologie*, Berlin, 1934). This and two books by O. Veblen (*Analysis Situs*, Colloquium Publications, Vol. V, New York, 1931, 2nd edition) and by S. Leschetz (*Topology*, Colloquium Publications, Vol. XII, New York, 1934) are three of the best books on the subject.

ASTRONOMY. By R. W. WRIGLEY, M.A., F.R.S.E., Royal Observatory, Edinburgh.

WHITE DWARF STARS.—The remarkable group of stars known as white dwarfs show by their colour that their surfaces are hot and brilliant, while their low intrinsic luminosity implies that they must be very small in size. According to the generally accepted theory, they are exceedingly dense bodies, composed of very highly ionised atoms from which most of the exterior electrons have been stripped. Normal atoms are composed largely of empty space, and, when their exterior electrons are lost, they are able to pack much more closely together, with the result that in a white dwarf the mass of an ordinary star is condensed into the size of a planet, with an almost incredible increase in its mean density. Probably owing to their faintness rather than to their actual rarity, the number of white dwarfs so far discovered is only about half a dozen. The companion of Sirius is the best known example, and, as its orbital motion and also its parallax are accurately known, its mass can be readily computed. In addition, a shift towards the red has been observed in its spectral lines, more or less in accordance with the theory of relativity, which requires a slowing down of the vibrations of light in an intense gravitational field, the shift for any star being directly proportional to its mass and inversely proportional to its radius. This observation is, however, very difficult owing to the brilliance and proximity of the primary star.

Unfortunately there is an uncertainty attaching to these conclusions owing to the existence of a strong suspicion that Sirius B

is really a close double star, and, if this is the case, its small luminosity is obviously not in need of so elaborate an explanation. Information from other white dwarfs is desirable, and the announcement of the discovery of an additional two is therefore of considerable interest. They have been found by G. P. Kuiper with the Lick 36-inch refractor during an examination of the duplicity of stars of large parallax, being identified from their magnitudes and colour estimates (*Publications of the Astronomical Society of the Pacific*, 1934, October). The stars are A.C. + 70°, 8247, (R.A. 19h. 1m.·1, Dec., + 70° 30', 1900) and Wolf 1346, (R.A. 20 h. 30m.·1, Dec., + 22° 44', 1900). The former was assigned by Kuiper to Spectral Class B0, its absolute magnitude estimated as 11·9, its radius as 0·8 that of the earth, and its density 3·55 × 10^6 times that of water. The second star was of class B7, absolute magnitude 9·8, and radius 2·8 × earth.

The spectra of these two stars have been examined at Mount Wilson by Adams and Humason (*Publ. Ast. Soc. Pac.*, 1935, Feb.). They were found to be remarkable for the extraordinary width and diffuseness of the few lines which were visible. No helium lines could be found, but a faint K-line seemed to be present in both spectra, and Adams and Humason were inclined to assign the stars to spectral type A rather than to B, while considering that the special characteristics of white dwarfs almost necessitate the use of a separate classification for their spectra. The extreme diffuseness of the lines made measurement very difficult. In *Publ. Ast. Soc. Pac.*, 1935, Dec., G. P. Kuiper gives a more detailed account of the star A.C. + 70°, 8247, which he describes as the smallest star known. Three slitless spectra and one slit spectrum were secured, the last with an exposure of seven hours with a two-prism spectrograph attached to the Lick 36-inch telescope. To his surprise, no lines and no trace of continuous hydrogen absorption could be found on any of the spectra, and, therefore, according to the literal definition of spectral classification, the star should be classed as O0. The continuous spectrum was compared with that of the star 10 Lacertæ, one of the bluest stars in the northern sky, and chosen as being probably very little affected by space reddening. The two spectra showed almost exactly the same distribution of continuous light, the only difference being the presence of lines in the comparison star, and, from this similarity, Kuiper deduced a surface temperature of 28,000° for the white dwarf. The photographic magnitude was determined by Miss D. N. Davis by comparison with three stars of the Polar Sequence, and found to be 13·12, which, using the appropriate colour index,

gave the visual magnitude as 13·50. The parallax is given in *Schlesinger's Catalogue* of 1935 as $0''\cdot065 \pm 0''\cdot011$, and accordingly, if it be assumed that the value lies between $0''\cdot050$ and $0''\cdot080$, it appears that the radius of the star must be between 2700 and 1700 miles, and its absolute magnitude between 12 and 13. These figures bear out Kuiper's contention that this white dwarf, with a diameter roughly half that of the Earth, is smaller than any other known star. Unfortunately, there is no available method for a direct determination of its mass, and the total absence of lines in its spectrum precludes the measurement of the relativity red shift from which the intensity of the gravitational field at its surface might have been deduced. However, Chandrasekhar has evolved a theoretical relation between the radius of a white dwarf in its final state and its mass, assuming the mean molecular weight to be known (*Monthly Notices R.A.S.*, **95**, 217). For the star A.C. + 70° 8247 Kuiper adopts a molecular weight 1·4, equal to the average found for the three white dwarfs whose masses have been determined, and derives therefrom a stellar mass 2·8 times that of the sun. It follows that its average density is 36,000,000 times that of water, roughly one thousand times that of Sirius B, and equal to 620 tons per cubic inch. In suggesting an explanation of the absence of lines in its spectrum Kuiper points out that the enormous temperature and surface gravity will produce a correspondingly high density gradient in its atmosphere, so steep that the whole atmosphere can hardly be more than a dozen feet deep. The star's colour corresponds to spectral type O9, a group in which ordinarily the Balmer hydrogen lines are weak, while in van Maanen's star, also a white dwarf, they are so shallow and wide as to be barely visible. In the present case the very high surface temperature and the enormous pressure in the atmosphere might easily widen the lines until they become invisible, for the conditions are much more extreme than any hitherto considered by Pannekoek in his work on line profiles (*Proc. Amsterdam Acad.*, **38**, 5).

A star of the extreme density calculated above would necessarily have other interesting characteristics. According to the principle of the conservation of angular momentum, if a star suddenly shrinks to a fraction of its former size and retains its relative density distribution the rotational velocity must increase enormously. White dwarfs, therefore, if they are formed by the collapse of ordinary stars, must rotate at very great speeds. Further, if A.C. + 70°, 8247, had a companion moving in a circular orbit close to its surface the period of revolution would be only two seconds, assuming that the companion was dense enough not to

be disintegrated by tidal action. These and other problems cannot yet be answered by Dr. Kuiper, and it is obvious that many more white dwarfs must be discovered, and the many difficulties besetting their study must be overcome, before the nature of these extraordinary bodies is fully understood.

SIZES OF NOVÆ.—Calculations by Sergei Gaposchkin in *Bulletins of the Harvard College Observatory*, 899 and 901, show that the initial radii of novæ, although considerably larger than the radii of white dwarf stars, are much nearer to them than most other stars. According to the somewhat meagre observational data at present available, there is no great change in the spectrum of a nova before its maximum brilliance is reached, and it seems probable that both the temperature and surface brightness of the star remain reasonably constant during the rise. Gaposchkin therefore assumes that the sudden change in the star's absolute magnitude is due to expansion either of its whole volume or of its outer layers, thus following the most generally accepted theory. There are only seven novæ, all observed since 1901, for which both spectroscopic and photometric data before the maximum are available. The observed radial velocity at maximum has been adopted as the average velocity of expansion during the whole period of brightening, and this assumption, together with the time taken for the rise, and the difference in the initial and final apparent magnitudes, enables the initial radius of each star to be estimated. It is concluded that novæ showing a rapid and large increase in brightness are initially dwarfish stars. They are, in fact, the only intrinsically variable dwarf stars, and are even smaller than the W Ursæ Majoris class of eclipsing variables. There is also another class of nova, including Nova Pyxidis, 1902, which consists of initially giant stars distinguishable by their small range of brightness and slow rate of increase. The absolute magnitudes and parallaxes of the other six novæ were then calculated with the aid of the Stefan-Boltzmann law of black body radiation, and the results compared with those derived from trigonometrical methods. As the initial spectra of the novæ were virtually unknown, it was assumed in order to estimate their temperatures that the spectra had not materially changed before maximum was reached. With small parallaxes of this order, trigonometrical determinations cannot be regarded with very great confidence, but it is nevertheless interesting that Gaposchkin finds them to be uniformly larger than those which he has obtained by calculation, the differences being most marked for Nova Persei, 1901, and Nova Cygni, 1920. He discusses the effect on the latter of possible errors in his adopted values of radial

velocity, time of rise to maximum, total luminosity, and surface brightness, and he comes to the conclusion that the chief uncertainty in the calculations lies in the time of rise. If the trigonometrical parallaxes are regarded as of the right order, the estimates of the initial radii of the novæ must be considerably reduced. These would then vary between 0·05 and 0·65 that of the sun, with an unweighted average of 0·28, which brings them distinctly nearer to white dwarfs in luminosity and radius.

A SHORT PERIOD VISUAL BINARY.—Hitherto the visual binaries of shortest known period have been the brighter component of β1000 which was found to be double by B. H. Dawson, and the system δ Equulei, the former having a period of 4·56 years and the latter one of 5·70. In *Pub. Ast. Soc. of the Pacific*, 1936, Feb., G. P. Kuiper announces his discovery that the close binary B.D. − 8°, 4352, has a still shorter period. It has been observed for nearly 14 months with the Lick 36-inch refractor, generally with a magnification of 1530, and the measures cover an arc of about 247°. The measured distances are all between 0″·13 and 0″·20, and a plot of position angles against the time shows an approximately linear relationship, so Kuiper infers a nearly circular orbit with the primary in the centre, indicating that both the eccentricity and the inclination are small. On this assumption, the observations give a period of only 1·66 years, and the weighted mean distance is 0″·19. The weighted mean parallax of the system is given as 0″·148 ± 0″·004, resulting in a semi-major axis of 1·25 astronomical units, and a total mass 0·71 times that of the sun. The components differ very little in magnitude, and, if the combined mass is assumed to be equally divided between them, each must have 0·36 times the mass of the sun, and only three stars of smaller mass are at present known. These are Krüger 60 A and B, and O_2 Eridani C whose masses are 0·23, 0·16, and 0·20 times the sun respectively. Kuiper points out that the observed absolute magnitudes of these four small stars show considerable differences from the corresponding values calculated from their masses by means of a mass-luminosity curve, but it should be noted that a small error in the measured distance of a very close pair has a serious effect on the mass computed therefrom. For the star − 8° 4352, a systematic increase in the distance of only 0″·02 would make the mass too large by no less than 40 per cent.

But Kuiper's most interesting discovery of a double star was made on July 4, 1935, when he found Nova Herculis 1934 to consist of two components separated by 0″·21 (*Publ. Ast. Soc. Pac.*, 1935, Aug.). The two stellar nuclei differed in magnitude by 0·6. The

observation of duplicity was confirmed by Baldet at Meudon and by Van Biesbroeck at Yerkes (*Astrophysical Journal*, **82,** 433). According to further observation by the latter on March 18, the components had increased their distance almost uniformly to $0''\cdot31$, but the magnitude difference and the position angle remained unchanged. This phenomenon is not unique for a nova, for it was observed in the case of Nova Pictoris by van den Bos and Finsen at Johannesburg (*Monthly Notices R.A.S.*, **92,** 1). Here two distinct components were observed gradually to increase their distances from the central star, while the presence of a fourth body was strongly suspected. In this case the companions appeared to be gaseous condensations emitted from the nova, for they faded in brightness much more rapidly than their parent. The observed constant difference between the magnitudes of the two components of Nova Herculis suggests the possibility that this star has actually split into two.

However, in *Proc. Nat. Acad. Sciences*, 1936, April, F. L. Whipple and C. P. Gaposchkin give reasons for considering the two components to be gaseous masses of low density, ejected from the central star probably very early in the outburst. Definite signs of doubling could be observed before the end of 1934 in the bright lines which characterised the star's spectrum, and it is thought that the red and violet components of the doubled lines correspond respectively to the fainter and brighter components observed visually by Kuiper. From an analysis of the changes in the spectrum of the nova it is deduced that these two bodies derive their light from the central star, that they are considerable in size, but that their density and mass are low. They are therefore in no sense stellar. It is probable that they were expelled from the central star at the very beginning of its rise to its first maximum, and were therefore connected with the disturbance which caused the outburst, but their separation did not become large enough for their visual detection until last July.

PHYSICS. By W. N. Bond, M.A., D.Sc., F.Inst.P., The University, Reading.

The Ohm.—Of the various developments of physics, fresh discoveries and novel applications probably attract most attention. Theories and philosophical discussions come next in order of interest, and laborious measurements of high precision are apt to be regarded as prosaic and dull. Nevertheless, there is an importance and even a fascination in making three decimal places grow where only one grew before.

The idea of " the resistance " of an electrical conductor first took precise form as the result of the work of Dr. G. S. Ohm in 1826–27, but for many years resistances were expressed in terms of units that were quite arbitrary. Gauss, in 1832, had suggested that magnetic measurements could be made in terms of " absolute " units, based on the units of length, mass and time ; and in 1851 W. Weber devised three absolute systems of electrical units (*Ann. Phys. Chem.*, **82**, 337, 1851. Translated in *Phil. Mag.* [4], **22**, 226 and 261, 1861), two of which (the electrostatic and electro-magnetic) are in use at the present time. It at once became evident that electrical measurements would usually have to be made in terms of intermediate standards, that were themselves measured on an " absolute " system.

The first measurement of the resistance of a wire in absolute units was made by Kirchhoff in 1849. Weber made measurements by two methods in 1851, but his results differed among themselves by nearly 10 per cent. In 1861 the British Association for the Advancement of Science appointed a committee on electrical units and standards. This committee decided that the practical electrical units should be based on the c.g.s. system of mechanical units, and appointed a sub-committee to make an absolute determination of the unit of resistance. The experiments made by this sub-committee (Maxwell, Stewart and Jenkin) resulted in the construction of a standard resistance called " the ohm " (*B.A. Report*, **33**, 163, 1863). This " B.A. ohm " is now known to be about 1·5 per cent. less than the absolute ohm, but it was nearly twenty years before the discrepancy was definitely established.

At an International Conference on Electrical Units and Standards held in London in 1908 it was decided to define the " International Ohm " as the resistance offered to an unvarying electric current by a column of mercury at 0° C., 14·4521 grammes in mass, of a constant cross-section, and of a length of 106·300 cm. This " International Ohm " was intended to be very nearly equal to an absolute ohm, and it remained for subsequent experimenters to compare the two.

The problem is, however, rather complicated, for the various national standardising laboratories do not continually use a mercury column (as defined above) as their " International Ohm." Let us consider, for instance, the method of maintaining the international ohm at the Bureau of Standards (*Journal of Research*, Nat. Bureau of Standards, **16**, 43, 1936). It started in 1910 with about 20 coils, each having a nominal value of 1 international ohm. Ten of these were selected, and it was assumed that for a particular

period (about six months) the mean value of the 10 remained constant. At the end of the period, all the 20 coils were compared again, and the 10 were chosen that appeared to have changed the least. By continuing this process, coils that show abnormal changes in resistance get rejected. In fact, there are 7 of the coils which have been among the 10 selected at every intercomparison since 1910.

Between the years 1900 and 1935 there had been only two complete determinations of the " international ohm " in terms of the " absolute ohm." That made at the National Physical Laboratory (F. E. Smith, *Phil. Trans.*, **214**A, 27, 1914) gave :—

1 N.P.L. international ohm = 1·00052 ± 0·00004 absolute ohms

and the determination made at the Physikalisch-Technische Reichsanstalt (completed in 1914 ; Grüneisen and Giebe, *Ann. Phys.*, **368**, 179, 1920) gave :—

1 P.T.R. international ohm = 1·00051 ± 0·00003 absolute ohms.

An intercomparison of the resistance standards showed that (at that time),

(1 P.T.R. international ohm) − (1 N.P.L. international ohm)
= 0·00003 ohms.

At the present time, investigations on the absolute value of the ohm are in progress at the National Physical Laboratory and at the Electrotechnical Laboratory of Japan. Experiments were started by the late E. B. Rosa, at the Bureau of Standards in 1906, but discontinued about 1910, after an accuracy of rather less than one part in 10000 had been attained. The research was resumed in 1921, and the work which has been done since that time is described in a recent paper (H. L. Curtis, C. Moon and C. Matilda Sparks, *Journ. of Research*, Nat. Bureau of Standards, **16**, 1–82, Jan. 1936).

The " absolute ohm " is by definition equal to 10^9 c.g.s. electromagnetic units of resistance. The c.g.s. electro-magnetic unit of resistance may be written, somewhat cryptically, as " 1 cm./sec." Measuring, in electro-magnetic units, the resistance of a coil that is supposed to be equal to " 1 international ohm," is somewhat like measuring a velocity of about 10^9 cm./sec., involving essentially the accurate measurement of a length in centimetres and a time in mean solar seconds.

The self-inductance of a helix, expressed in e.m.u. or " centimetres," may be deduced from its linear dimensions, though till comparatively recently the detailed calculations had only been made for *mutual* inductances. The resistance of a " 1 international ohm " coil might therefore be determined in terms of a self-induct-

ance and a time.   This is the method that has been adopted in the work at the Bureau of Standards (referred to above).   Three helices were prepared by winding bare copper wire of 0·71 mm. diameter on ground cylinders of porcelain, glass and quartz, the pitch of each helix being about 1 mm.   The wire was kept in position by a thin coating of shellac varnish on the under side.   The diameters of the cylinders were 29, 46 and 28 cm., and the lengths of the helices 41, 26 and 100 cm.   Extreme care was taken in the linear measurement and in the computing of the inductances.   The self-inductance of a long helix is approximately equal to

$$(\text{length})\left(\frac{\text{circumference}}{\text{pitch}}\right)^2.$$

Owing to the circumference being equal to about a thousand times the pitch, the self-inductances were of the order of magnitude 5×10^7 cm.   It is necessary also to measure accurately a time, which is best arranged to have a value of about 0·01 second, because the resistance being measured is about 10^9 cm./sec.

Three electrical bridge circuits were used.   A resistance was measured in terms of the inductance and an intermediary capacitance by means of a Maxwell-Wien alternating current bridge.   Then the capacitance was measured in terms of a resistance and time, using a Maxwell capacitance bridge.   In addition, a Wheatstone bridge was used for comparing resistances.   The frequency of the alternating current supplied to the Maxwell-Wien bridge need not be known, but it must be kept fairly constant, so that the vibration galvanometer (used as a detector) shall always be at its maximum sensitivity.   In the case of the Maxwell capacitance bridge, however, the frequency of charging and discharging must be constant and accurately known.   In the early part of the work, a tuning fork (100 oscillations per second) was used to maintain the constant frequency, the frequency being found in terms of a standard pendulum clock.   In the later experiments, a standard frequency generator, controlled by a piezoelectric oscillator, was used to *drive* the fork.   The forced oscillations of the fork had a frequency equal to that of the standard frequency generator, namely 100 per second to an accuracy of one part in a million.   In the first experiments, platinum wires attached to the prongs of the fork dipped into mercury cups, to make and break the circuits.   Subsequently, tungsten contacts in a vacuum were used.   Each contact consisted of a pair of tungsten wires in a glass tube, one end of the tube being closed by flexible glass bellows, through which a glass rod was sealed.   The rod was attached to a prong of the tuning fork,

and controlled the contact between the pair of tungsten wires. Yet a third type of contact was used in some experiments. This consisted of a tungsten reed, loaded at its free end and situated between platinum contacts. The whole system was attached to one prong of the tuning fork, and the inertia of the mass at the end of the reed caused the reed to vibrate relative to the contacts, with a frequency equal to that of the fork.

The accidental (or random) errors were investigated, as well as possible sources of error such as change in inductance with frequency, effect of steel rods in the ceiling of the room, current leakage between the wires of the inductances, and irregularities in the pitch or diameter of the inductances. So far as possible, systematic errors were eliminated. This was accomplished by using, whenever practicable, two or more instruments by which to determine any required constant. This procedure several times led to the discovery of unexpected errors, which were rectified either by eliminating the cause or applying a suitable correction.

The final result obtained was :

1 N.B.S. international ohm = 1·000,450 absolute ohms.

The authors estimate that this result differs from the true value by less than 20 parts in a million.

The investigations that are in progress at the National Physical Laboratory and at the Electrotechnical Laboratory of Japan have already been mentioned. Preliminary values have been published (*Ann. Report of N.P.L.*, p. 57, 1934 ; *Comité International des Poids et Mesures. Procés-verbaux*, **16**, 28, 1933) as follows :

$$1 \text{ N.P.L. Int. ohm} = \begin{cases} 1\text{·}00049 \text{ abs. ohms (Lorenz method)} \\ 1\text{·}00047 \text{ abs. ohms (Campbell method).} \end{cases}$$

1 E.T.L. Int. ohm = 1·00046 abs. ohms (Campbell method).

Measurements made at the International Bureau of Weights and Measures indicate that the maximum difference between the international ohm as maintained at the three laboratories (N.B.S., N.P.L., and E.T.L.) is only 3 parts in a million. Hence the results given above may be regarded as four estimates of the same quantity, differing slightly because of experimental errors.

It is intended that, from January 1, 1940, the " practical absolute system of electrical units " shall be used in place of the " international system " that is in use at the present time. A provisional list has been published of the ratios of various units in the two systems. (See, for instance, *Journ. Sci. Inst.*, **13**, 106, 1936 ; *Terrestrial Mag.*, **41**, 88, 1936.)

Monomolecular Layers.—The surface tension of contaminated water surfaces, and of water surfaces covered with a thin film of oil, was investigated many years ago by Miss Pockels and by the third Baron Rayleigh. The amount of contamination on unit area of the surface was controlled by confining the greased portion between strips of glass or metal dipping into the water ; by pulling these apart, the area of the greased surface was increased, and therefore the mean thickness of the surface layer diminished. The surface tension was found by measuring the force required to detach a disc of known area from the surface, or by measuring the downward pull exerted on a thin vertical plate partly immersed in the liquid. The results of these experiments gave evidence that some of the thin oil layers were exactly one molecule thick.

Considerably more detailed information was obtained later by Dr. Irving Langmuir (*Journ. Amer. Chem. Soc.*, **38**, 2221, 1916 ; **39**, 1857, 1917 ; *Trans. Faraday Soc.*, **15**, 62, 1920). In Langmuir's experiments, the " contamination " consisted of definite known organic compounds. The surface tension was found by measuring the resultant horizontal force exerted on a thin solid barrier lying on the water surface, with pure water on one side and the contaminated water surface on the other. To prevent the contamination passing round the ends of the barrier, small air-blasts were directed obliquely on the water surface. The writer saw an apparatus of this type in use in 1920, and can appreciate the experimental skill required to make the effects of the two air-blasts equal, and to prevent their exerting much force on the barrier. These difficulties were overcome by N. K. Adam (*The Physics and Chemistry of Surfaces*), who used fine flexible metal strips at the ends of the barrier, to prevent the " contamination " passing to the clean water side. He also considerably improved the method of measuring the resultant force on the barrier.

The results obtained by Langmuir and by N. K. Adam showed that various types of surface film may occur. The pull exerted on the barrier is reduced by the presence of the contamination ; and this reduction in surface tension is best regarded as being due to the molecules of the surface film bombarding or impinging against the barrier. Some of the surface films exhibited phenomena in two-dimensions corresponding to the three-dimensional transition from gas to liquid. In particular, when the area of the film is sufficiently reduced, a compact layer is formed which is one molecule thick. In most cases the organic molecules of the surface film are of long chain form ; and they are packed side by side, each having one end attached to the water surface. The lengths and areas of

cross-section of the chain-like molecules were determined, and found to agree fairly well with results obtained by the X-ray investigation of the organic compounds concerned.

An extension of Dr. Langmuir's work has recently been made, which is of considerable interest, and promises to have various further developments. Dr. Katharine B. Blodgett (*Journ. Amer. Chem. Soc.*, **56**, 495, 1934, and **57**, 1007, 1935) describes a method by which a known number of monomolecular layers of soaps may be superposed on a solid surface, so as to produce a composite layer (which may be some hundreds of molecules thick). The layer may be deposited on surfaces of glass, polished chromium, nickel, iron, silver, platinum, steel or brass.

The plate of glass or other material is arranged vertically, with one edge dipping into an aqueous solution, on the surface of which is a monomolecular film of a substance such as calcium or barium stearate or stearic acid. The hydrogen ion concentration of the solution has to have a suitable value, and the monomolecular film must be maintained under a suitable (two-dimensional) pressure. The glass plate is then alternately lowered and raised. Two varieties of film are formed, according to the value of the hydrogen ion concentration of the solution. In one case, a monomolecular film is deposited during the first raising of the plate, and also during each subsequent lowering or raising. In the other case, the film is deposited only during each lowering. In the former case, alternate layers of molecules are in reverse directions ; so that, if such a film were used for diffraction of X-rays, the effective " grating space " would be equal to the thickness of two molecular layers.

In forming the films, the monomolecular layer on the surface of the aqueous solution is surrounded by a loop of thread, lying in the surface. Oil is placed on the surface beyond the thread, in order to produce the required two-dimensional pressure. As successive layers are removed from the liquid surface and deposited on the glass plate, the loop of thread is gradually reduced in area.

With the composite layers, the optical effects of thin films may be observed. By stopping the deposition at various stages, a series of glass plates may be prepared which, when arranged in a row in order of increasing film thickness, show a " Newton " series of colours. In order to measure the thickness of the films, they were formed on glass of high refractive index (1·64) so as to increase the amount of light reflected at the back of the film. Using a sodium vapour lamp, and normal incidence, the brightness of the light reflected was found to change sinusoidally as the number of

molecular layers in the film was increased. By finding the numbers of layers required to give maximum or minimum brightness, information is obtained concerning the thickness of a single monomolecular layer. To deduce the actual thickness, it is necessary to know the refractive index of the film. This was determined by measuring the polarising angle, which was located by finding the angle at which the phase of the reflected light abruptly changes by $180°$.

The thickness of films of calcium soaps (calcium palmitate, stearate and arachidate) were measured. The pressure in the plane of the film appears to have forced the molecules to assume a position with their length perpendicular to the film surface (not inclined, as in the usual crystalline form of the acid). The values of the film thickness were a little greater than the long spacings of the corresponding fatty acids determined by other workers by means of X-ray diffraction. It is suggested that the pressure parallel to the surface of the film (which is in some cases as great as 118 atmospheres) causes a straightening and lengthening of the chain-like molecules. (An increase in film thickness with pressure had previously been measured by Adam.) Heating the films produced a reduction in thickness, which was apparently due to a fresh orientation of the molecules, and not to loss of water. When light was reflected at the boundary between the film and the glass plate, it was found that the boundary acted as if it had been located about 20 to 40 A. beneath the surface of the glass. When the deposit was on polished chromium, a single molecular-layer could be seen by means of polarised light.

An X-ray diffraction study of films built by Dr. Blodgett's method was made by G. L. Clark, R. R. Sterrett and P. W. Leppla (*Journ. Amer. Chem. Soc.*, **57**, 330, 1935); and C. Holley and S. Bernstein have published X-ray photographs taken with a film of 301 molecular layers of barium stearate prepared by Dr. Blodgett (*Phys. Rev.*, **49**, 403, March 1, 1936). This photograph shows the M line and three orders of the L line of tungsten. The M line is almost as sharp as is to be expected theoretically from the limited number of molecular layers that co-operate in producing the diffraction pattern. It may be possible to investigate the width of the X-ray lines; to get more information about the true value of X-ray wavelengths (which has been the subject of discussion recently); to deduce values of Avagadro's number and the electronic charge; and to discover whether the innermost and outermost molecular layers are in any way abnormal.

GENERAL AND ORGANIC CHEMISTRY. By O. L. BRADY, D.Sc., F.I.C., University College, London.

IMPORTANT work remains to be done on some of the reactions described in the most elementary text-book on chemistry, but it is not often that within a short space of time two such everyday things as the formation of sulphuric acid mist and the action of chlorine on lime receive serious attention.

FORMATION OF SULPHURIC ACID MIST.—Every teacher of elementary chemistry finds it necessary to impress on his students that, when a mixture of sulphur trioxide and air or other inert gas is passed through water, the sulphur trioxide is not appreciably absorbed but is converted to a mist which cannot be absorbed even by alkalis but that the sulphur trioxide is completely absorbed from such a mixture by 98 per cent. sulphuric acid. Sackur (*Z. Elektrochem.*, 1902, **8**, 77) suggested that the SO_3 was converted into an insoluble form S_2O_6 in these circumstances, but Walker, Lewis and McAdams (*Principles of Chemical Engineering*, 1923, p. 42) put forward a better explanation in the following words : " If one attempts to dissolve the trioxide in water or dilute sulphuric acid, the trioxide vapour first comes in contact, not with the liquid but with the water vapour which has evaporated from the liquid into the gas. It reacts with this vapour, producing minute droplets of sulphuric acid in the form of a fog, and these droplets are effectively insulated from the absorbing liquid by the gas film. One must therefore use as an absorbent a liquid, the water vapour pressure of which is negligible, *i.e.* strong sulphuric acid. This is the reason why for absorption one must use acid between 97 and 98 per cent. If more dilute, the pressure of water vapour is sufficient to produce a fog, if more concentrated the partial pressure of SO_3 over it is great enough to prevent complete absorption."

Dooley and Goodeve (*Faraday Soc., Discussion on Smoke,* April, 1936) have now obtained important evidence in favour of this explanation. Goodeve, Eastman and Dooley (*Trans. Faraday Soc.,* 1934, **30**, 1), from a study of the kinetics of the reaction between sulphur trioxide vapour and water vapour, found that the reaction H_2O (vapour) $+ SO_3$ (vapour) $\rightarrow H_2SO_4$ vapour was mainly homogeneous, approximately stoichiometric and very rapid under low pressures of reactants, about one collision in a hundred between reacting gas molecules being effective. These characteristics result in a very narrow reaction zone whose position is determined by the relative concentrations of the two vapours and the diffusion velocities. The stoichiometric nature of the reaction necessitates that the rate of arrival of the sulphur trioxide vapour and water

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vapour in the reaction zone should be the same, and if the flow
rate be altered the zone changes its position until these conditions
are fulfilled. The arrival of water vapour at the reaction zone
depends on its rate of evaporation from a liquid surface. It can
be calculated from the work of Alty and Mackay (*Proc. Roy. Soc.*,
1935, **149A**, 104) that the rate of evaporation of water into a vapour
whose pressure is 1 mm. below the equilibrium pressure is, at least,
2×10^{19} molecules per second per sq. cm.; unless the rate of
arrival of sulphur trioxide is greater than this the water surface
will always be protected by a layer of water vapour.

The mechanism of formation of sulphuric acid mist put forward
by Dooley and Goodeve is, therefore, as follows. The high velocity
of the reaction between sulphur trioxide vapour and water vapour
does not permit the sulphur trioxide to reach the water surface.
The sulphuric acid formed is in the state of a supersaturated vapour,
but as its concentration increases it condenses to droplets of sul-
phuric acid, a few molecules only penetrating the protecting water
vapour layer and entering the liquid water. Finally, the agglom-
erates of sulphuric acid are carried by the air stream into a part
containing excess of water vapour which is absorbed until equili-
brium is attained with the production of particles of strong light
scattering power, that is, sulphuric acid mist.

The mechanism can be demonstrated by a simple experiment,
Fig. 1, where a mixture of air and sulphur trioxide vapour is passed
over a drop of water in a tube.
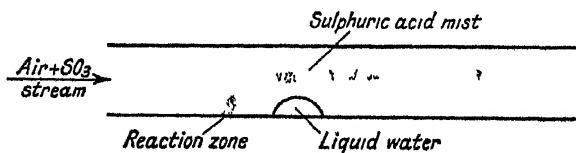
Fig. 1.

The clear space between the reaction zone and the surface of
the water and the thin layer of sulphuric acid mist are plainly
visible. The drop of water evaporates rapidly, but since some
sulphuric acid diffuses through the protecting water vapour layer
to the liquid water lowering its vapour pressure progressively it
does not, generally, completely disappear; in these circumstances
as the sulphuric acid concentrates in the drop the reaction zone
approaches nearer until the vapour pressure of water is so low that
the stream of mist disappears.

The stability of sulphuric acid mist to alkali, etc., presents
another problem; it has been suggested that it is due to adsorbed

air films or to electrical charges on the particles, but Remy and Koch (*Z. anorg. Chem.*, 1924, **139**, 69) have shown that the particles of sulphuric acid mist are uncharged and the existence of strongly adsorbed air films on the surface of liquids has not been established nor is likely. The investigations of Tuovila and of Whytlaw-Gray and collaborators (*cf. Smoke*, by Whytlaw-Gray and Patterson, 1932) have, however, shown that with, for example, stearic acid smoke, every collision between mist particles results in their coalescence and the stability can be accounted for by the low diffusion velocity and low collision frequency of the particles. It seems very probable, therefore, that the stability of sulphuric acid mist is due to the same low diffusion velocity.

THE CONSTITUTION OF BLEACHING POWDER.—The constitution of bleaching powder has been the subject of many investigations. Although the reaction may be regarded as essentially $2Ca(OH)_2 + 2Cl_2 \rightarrow Ca(OCl)_2 + CaCl_2 + 2H_2O$ it never goes to completion in this sense, commercial bleaching powder usually containing from 15–25 per cent. of calcium hydroxide and only about 35 per cent. of available chlorine. Moreover, the physical properties of bleaching powder, *e.g.* the lack of marked deliquescence, are not those of a simple mixture of calcium hypochlorite, calcium chloride and unacted-upon calcium hydroxide. Numerous views have been put forward on its constitution of which only a few need be mentioned. Odling (1861) suggested the existence of a salt Ca(OCl)Cl and Kraut (*Annalen*, 1882, **214**, 354) a double salt $CaCl_2.Ca(OCl)_2$, whilst Neumann and Haebler (*Z. Electrochem.*, 1929, **35**, 909) favoured a basic calcium chlorohypochlorite

$$3Ca\begin{array}{c}OCl\\\diagdown\\Cl\end{array}.Ca(OH)_2.$$ Urano (*World Engineering Congress*, Tokyo, 1929, **31**, 171) from a study of the system $CaCl_2-Ca(OCl)_2-H_2O$ claimed evidence for the existence of an ill-defined double salt $Ca(OCl)Cl.4H_2O$ or $CaCl_2.Ca(OCl)_2.8H_2O$, but O'Connor (*J. Chem. Soc.*, 1927, 2700) studying the system

$$CaCl_2-Ca(OCl)_2-Ca(OH)_2-H_2O$$
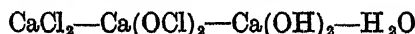
found no evidence of the existence of a double salt of calcium chloride and hypochlorite. Bunn, Clark and Clifford (*Proc. Roy. Soc.*, 1935, **151**, 141) have now made an important contribution to the study of this subject. A phase-rule study of the system $CaO-CaCl_2-Ca(OCl)_2-H_2O$ has been made, the solid phases existing between 25–40° C. being found to be $Ca(OCl)_2.3(?)H_2O$,

$3Ca(OCl)_2.2Ca(OH)_2.2H_2O$, $Ca(OCl)_2.2Ca(OH)_2$, $CaCl_2.6H_2O$,
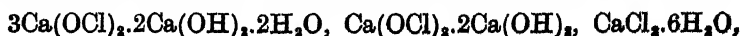
$CaCl_2.Ca(OH)_2.H_2O$, $CaCl_2.3Ca(OH)_2.12H_2O$, and $Ca(OH)_2$, while no indication was obtained of compound formation between calcium chloride and calcium hypochlorite. The optical properties of these phases have been studied together with those of other likely constituents of bleaching powder such as the lower hydrates of calcium chloride, anhydrous calcium chloride and calcium hypochlorite, and X-ray examinations have also been made of these compounds.

In the light of the data thus obtained bleaching powder prepared under controlled conditions has been examined by microscopic and X-ray methods and it has been shown that the first stage in the chlorination of lime is the formation of a basic hypochlorite, $Ca(OCl)_2.2Ca(OH)_2$, and a basic chloride
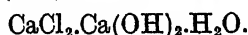
$$CaCl_2.Ca(OH)_2.H_2O.$$

When the available chlorine in the sample reaches about 30 per cent. the phases present are $CaCl_2.Ca(OH)_2.H_2O$, $Ca(OCl)_2.2Ca(OH)_2$ and a new unknown phase. When the available chlorine content reaches 35-37 per cent., the average for a commercial bleaching powder, $CaCl_2.Ca(OH)_2.H_2O$, is still present in large quantities but the basic hypochlorite, $Ca(OCl)_2.2Ca(OH)_2$, has disappeared and been replaced by the unknown phase. This unknown phase is considered to be, probably, a mixed crystal whose chief constituent is calcium hypochlorite, the other constituents may be calcium hydroxide, calcium chloride, or water, or all of these. The non-deliquescent nature of ordinary bleaching powder and the difficulty in introducing more than 35 per cent. of available chlorine are due to the presence of the basic chloride $CaCl_2.Ca(OH)_2.H_2O$ which appears to be a very stable substance, the calcium hydroxide present in it resisting further attack by chlorine whilst that in the basic hypochlorite $Ca(OCl)_2.2Ca(OH)_2$ apparently reacts.

ISOTOPES.—A large amount of work is still being recorded on derivatives of deuterium. Hexadeuterobenzene C_6D_6 has been obtained by several methods, by the distillation of thoroughly dried calcium mellitate with calcium deuteroxide

$$C_6(CO_2)_6Ca_3 + 3Ca(OD)_2 \rightarrow C_6D_6 + 6CaCO_3$$

(Erlenmeyer and Lobeck, *Helv. Chim. Acta*, 1935, **18**, 1464), by the action of deuterium chloride on benzene in the presence of anhydrous aluminium chloride (Kenner, Polanyi and Szego, *Nature*, 1935, **135**, 267), by shaking benzene with 90 per cent. sulphuric acid of enhanced deuterium content (Ingold, Raisin and Wilson, *Nature*, 1934, **134**, 734 ; Horiuti and Polanyi, *ibid.*, 847) and by passing benzene and deuterium oxide repeatedly over a nickel catalyst deposited on kieselguhr until equilibrium was attained,

separating the organic constituent and repeating the process three times with fresh deuterium oxide when almost pure C_6D_6 is obtained (Bowman, Benedict and Taylor, *J. Amer. Chem. Soc.*, 1935, **57,** 960). Hexadeuterobenzene has boiling-point 79·4° and freezing-point 6·8° compared with 80·12 and 5·5 for benzene. These figures differ considerably from those obtained by Clemo and McQuillen for the product obtained by passing dideuteracetylene over tellurium (*J. Chem. Soc.*, 1935, 853). Investigations have started on the Raman and Infra-red spectra of deuterobenzene (Angus, Bailey, Ingold, Leckie, Raisin, Thompson and Wilson, *Nature*, 1935, 680, 1033 ; Klit and Langseth, *ibid.*, 956 ; Barnes and Brattain, *Jour. of Chemical Physics*, 1935, 446). An interesting result of these measurements is that the reason for physicists rejecting the plane hexagonally symmetrical benzene model of the chemists has been removed, namely the appearance of coincident frequencies in the Raman and infra-red spectra of that compound. It was assumed that these were coincidences of principle and not of accident and the conclusion followed from Placzek's theory (*Leipziger Vorträge,* 1931, p. 100) that benzene had no centre of symmetry. The work on deuterobenzene has shown that certainly all but one, and very probably all the coincidences are accidental.

In view of the many claims that water obtained from the combustion of naturally occurring organic compounds was richer in deuterium than ordinary water a careful investigation of the question by Dole (*J. Amer. Chem. Soc.*, 1936, **58,** 590) is of importance. He points out numerous sources of possible error in the work of previous investigators which a greater knowledge of the behaviour of deuterium and of oxygen of mass 18 have made evident, for example, the oxygen used in combustion may not have the same atomic weight as normal atmospheric oxygen, that the oxygen in the water produced by combustion of organic compounds containing oxygen may not have a normal atomic weight, that the isotopic interchange between H_2O^{18} and CO_2^{16} may be involved, that during manipulation a fractionation of the oxygen isotopes or of deuterium may occur. By a careful investigation of the combustion of benzene the author has shown that the only one of these possible sources of error which has an appreciable effect is the atomic weight of the oxygen employed. A much more important effect, however, is the surprising discovery, details of which are promised later, that the atomic weight of atmospheric oxygen differs appreciably from the atomic weight of oxygen present in normal water. Water made from atmospheric oxygen is denser than that made from oxygen originally in water by six parts per

million. When this correction is applied the excess density of water made by the combustion of benzene over that of Lake Michigan water is only 0·5 parts per million which is not considered significant. This difference in density of water prepared with oxygen from different sources is not far from the figures for the excess density of water obtained by combustion of many organic compounds, and it is suggested that the hydrogen in naturally occurring organic compounds is more nearly normal than has been supposed.

The existence of an isotope of hydrogen of mass three has now been established and it has been named Tritium with symbol T ; its abundance in ordinary water is of the order of seven parts in ten thousand million. Selwood, Taylor, Lozier and Bleakney (*J. Amer. Chem. Soc.*, 1935, **57**, 780) record the results of electrolysing 75 metric tons of water to 0·5 c.c. when the final product contained one part of tritium in ten thousand as determined by the mass spectrograph. The authors point out that by the further electrolysis of concentrated deuterium oxide preparations now produced commercially it should be possible to obtain residues rich in tritium fairly easily, the deuterium being recoverable without appreciable loss by the recombination of the electrolytic gas produced.

Much labour has been expended on attempts to concentrate the O^{18} constituent of water. After numerous contradictory statements, now explicable, concentration by electrolysis seems to have been established though the efficiency of the process is very low. Selwood, Taylor, Hipple and Bleakney (*J. Amer. Chem. Soc.*, 1935, **57**, 642) calculate from the concentration they obtained by this method that, in order to obtain 1 c.c. of 99·5 per cent. H_2O^{18} it would be necessary to start with at least 10^{50} c.c. of ordinary water, a volume far greater than that of all the water on the surface of the earth. These authors point out that the failure of some workers to obtain evidence of concentration of O^{18} during electrolysis was due to the use of carbon dioxide to carbonate the alkali used as electrolyte before distilling off the enriched water, this results in equilibration of the oxygen isotopic ratio to that of the carbon dioxide used. Urey and Greiff (*J. Amer. Chem. Soc.*, 1935, **57**, 326) have calculated the equilibrium constant of the reaction $2H_2O^{18} + CO_2^{16} \rightleftharpoons 2H_2O^{16} + CO_2^{18}$. The enrichment factor 1·039 favours the concentration of O^{18} in the carbon dioxide and the authors state that they are testing an apparatus designed on the countercurrent principle for the concentration of O^{18} on these lines.

In the field of pure organic chemistry nothing of sufficient general interest to justify its inclusion here has come to the notice of the writer, though the total amount of work published shows no marked decrease.

PHYSICAL CHEMISTRY. By H. W. MELVILLE, D.Sc., Ph.D., Colloid Science Laboratory, Cambridge.

RECENT EXPERIMENTS ON THE SORPTION OF GASES BY SOLIDS.— A cursory glance through any monograph dealing with the sorption of gases by solids soon convinces the reader that the phenomena described appear to be of infinite variety. Moreover it does not seem that more experiments employing existing technique are likely to be successful in unravelling the numerous problems encountered in this field. As often happens in physical chemistry a new technique has to be devised before further progress may be made in the line of enquiry. Such an advance has been made by J. K. Roberts (*Proc. Roy. Soc.*, A, **129**, 146, 1930 ; A, **135**, 192, 1932 ; A, **142**, 518, 1933 ; A, **152**, 445, 1935 ; *Trans. Faraday Soc.*, **31**, 1710, 1935 ; *Proc. Camb. Phil. Soc.*, **32**, 152, 1935) who has developed two new independent methods of studying the mode of formation and the nature of films produced when gases interact with tungsten. As will be evident from what follows, the experiments are not necessarily confined to tungsten, but in view of the experimental difficulties, tungsten at present is the most convenient metal with which to work.

The development of the technique was based on the discovery that when the surface of tungsten is cleaned by flashing *in vacuo* to at least 2000° K. and maintained clean, the accommodation coefficient [1] for inert gases is very much smaller than that hitherto obtained. For example, without taking special precautions, the accommodation coefficient of neon on tungsten is 0·6, whereas if the surface is clean it sinks to the low value of 0·07. It may be mentioned that such a surface can only be kept clean if the neon is continuously circulated through charcoal tubes cooled in liquid air. Even with these precautions it is not an easy matter to prevent the surface from becoming contaminated with impurities.

Since the accommodation of neon is so sensitive to the presence of adsorbed films, it is evident that a method is available to study

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[1] *i.e.* the ratio $\dfrac{T_L - T_o}{T_s - T_o}$ where T_L is the temperature of the molecules leaving the surface which is at a temperature T_s; T_o is the temperature of the impinging molecules.

the nature of these films by observing the value of the accommodation coefficient.

The second method of attack consists in the measurement of the heat of adsorption of gases on a single clean tungsten filament, the latter therefore functioning as its own calorimeter. Here again the device is not an easy one with which to work, as it will readily be realised that the maximum number of atoms adsorbable on a tungsten surface cannot exceed $1\cdot4 \times 10^{15}$ per cm.^{2} since there are $1\cdot4 \times 10^{15}$ tungsten atoms per cm.^{2} in the 110 plane of the crystal None the less it has proved practicable to measure with considerable accuracy a rise of only $0\cdot01°$ in the temperature of a wire $0\cdot006$ mm. in diameter.

Only hydrogen and oxygen have so far been employed. With hydrogen the surprising fact emerges that within a few minutes of its admission to the stream of neon (used for measuring the accommodation coefficient) the value of α (the accommodation coefficient) rises from 0 08 to $0\cdot17$ at $295°$ K. Employing the calorimetric method it is similarly found that on each admission of the gas there is a considerable evolution heat and simultaneously there is complete absorption of the gas, e.g. the initial pressure was of the order 10^{-4} mm., whereas a Pirani gauge sensitive to 10^{-7} mm. could not detect any residual traces of gas. When sufficient hydrogen had been admitted to cover the tungsten surface completely, i.e. one atom of hydrogen to one of tungsten, there was no further adsorption and no heat effect in the filament. By admitting small enough doses, the variation of the heat of adsorption with the extent of the surface covered may be measured. This is a linear function of the number of spaces vacant and varies from 18 for a completely covered surface to 45 kg cal. per mole for a bare surface. These facts permit of some conclusions being drawn about the properties of the hydrogen film. For example, the energy required to remove a hydrogen atom from a bare surface is 74 kg. cal. and from a completely covered surface 59 kg. cal. Unlike cæsium adsorbed on tungsten, the dipole moment of the W-H system is apparently unaffected by the fraction of the surface covered. A complete film of hydrogen changes the work function by $5\cdot2$ volts.

Essentially similar phenomena are observed with oxygen but in addition there are a number of interesting complications. Considering the accommodation coefficient method first, it is found that the value of α increases to about $0\cdot35$ on successive additions of oxygen. On raising the temperature of the filament to repeat these experiments, however, the accommodation coefficient falls to about $0\cdot25$ (α is of course measured at room temperature) and

remains there if the temperature is kept at 1100° K. On admitting oxygen again the value of α once more rises to 0·35. In other words there appears to be a second film formed which is less stable than the first since the latter is only removed on heating the filament above 2000° K. The measurement of the heat of adsorption confirms the supposition. The heat of adsorption of the first layer of oxygen is about 140 kg. cal. per mole of oxygen, whereas that of the second film is only about 45 kg. cal. But although the amount of gas required to form the first film corresponds very nearly to one atom of oxygen to one of tungsten, the amount needed for the second film is only 8 per cent. of that of the first. It is very probable that the first film consists of oxygen atoms attached to tungsten though there is no direct proof of this contention. It is certain that the second film is molecular, as will become evident from the following argument. If the heat of evaporation of a substance is known then, providing there are no complications such as association in the gas phase, its vapour pressure may be calculated approximately from the empirical rule of Trouton, or conversely. Making a similar postulate Roberts has deduced an equation for the evaporation of unifilms. It has the form

$$t_f = -\,2 \cdot 06 \times 10^{-13}\,(\log_{10} f)(M^{\frac{1}{2}}.T^{\frac{1}{2}}/\phi)e^{\phi/RT}$$

where t_f is the time in seconds taken for a film to evaporate to such an extent that a fraction f of the initial number of spaces occupied remains occupied, M is the molecular weight of the evaporating particles, ϕ is the energy of desorption in calories per mole. The numerical factor is obtained from the experiments of Langmuir and Villars on the variation of the rate of evaporation with temperature of oxygen from tungsten (*J. Amer. Chem. Soc.*, **53**, 495, 1931). The rate of evaporation of the second film yields an energy of desorption of 30 kg. cal. Assuming the film to be molecular the energy of desorption is of the same order of magnitude as that of adsorption, namely 48 kg. cal., whereas had the film been atomic, the energy of desorption would have been 82 kg. cal., taking the heat of dissociation of molecular oxygen to be 117 kg. cal. The film is consequently molecular in type.

The reason for the formation of the second film is based on the following ingenious argument. Presuming the first film to be atomic and assuming that oxygen atoms are practically immobile on the tungsten surface (the adsorption experiments are conducted at room temperature) a consideration of the kinetics of the formation of the film shows that the film is necessarily discontinuous in character. Oxygen molecules strike the surface and are dis-

sociated on combining with the tungsten. The condition for the occurrence of this reaction is that two adjacent tungsten atoms must be free. As the tungsten surface becomes covered, single isolated W atoms remain and it is evident that dissociation of the oxygen molecules cannot occur at these gaps. Statistical analysis indicates that 8 per cent. of the tungsten atoms remain bare and consequently it is logical to conclude that it is at these gaps the second film is formed by the adsorption of molecules of oxygen. It is an interesting fact that whereas the accommodation of neon on a unifilm of atomic oxygen is only 0·27, that on the film of molecular oxygen is no less than 0·7. Though it is dangerous to speculate on matters of this nature it would appear that the ability of the oxygen molecule to rotate enables it to communicate its energy more readily to the neon atom, than the oxygen atom vibrating against a tungsten lattice.

Using the calorimetric method it can be shown that oxygen displaces adsorbed hydrogen from the tungsten surface. From the heat effect of the process it appears that there is no combination between the hydrogen and oxygen and that the hydrogen leaves the surface in molecular form. Whether the oxygen is first adsorbed on top of the hydrogen and thereafter the displacement occurs, as may be diagrammatically indicated by $W - H - O \rightarrow W - O - H$, is not yet known.

The next point to discuss is the question as to whether molecules or atoms evaporate from the oxygen (first layer only) or hydrogen covered surface when the temperature of the filament is raised. The rapidity with which these films are formed and the independence of the rate of adsorption with temperature indicates that very little if any energy of activation is required for the process. In so far as the oxygen is concerned the calorimetric measurements give an energy of binding of the oxygen atom to the tungsten surface nearly equal to that found from the temperature coefficient of the rate of evaporation of oxygen from the surface. Considering these three facts together the conclusion might fairly be drawn that oxygen atoms evaporate as such. There is however no direct experimental evidence for this supposition. A similar agreement exists in the hydrogen experiments for the temperature at which the hydrogen film becomes unstable corresponds to a heat of desorption of about 60 kg. cal. which is nearly equal to that calculated for the desorption of atoms from the surface. This figure gives the rate of desorption of atoms from tungsten by substitution in the above formula. The maximum rate of desorption of hydrogen atoms may also be calculated from the value of the equilibrium

constant by applying the principle of detailed balancing to hydrogen gas contained in a tungsten box. The three processes which require to be balanced are (1) evaporation and condensation of atoms, (2) combination of two atoms to form a molecule which evaporates and condensation of a molecule to form atoms, and (3) one atom strikes an adsorbed atom, one molecule evaporating and one molecule strikes the surface, one atom sticks, the other evaporates. Combining this with the fact that the equilibrium must be established in the gas phase, it is found that the calculated rate of evaporation of atoms is very much smaller than that computed from the instability of the adsorbed film at higher temperatures. It will therefore be necessary to devise further experiments to determine the atom/molecule ratio of the particles evaporating from the unifilm of hydrogen on tungsten. Similar remarks probably apply to oxygen.

In order to demonstrate how difficult it is to discriminate between the evaporation of atoms and molecules from a unifilm of oxygen atoms, the energy diagram for the system is drawn below. The
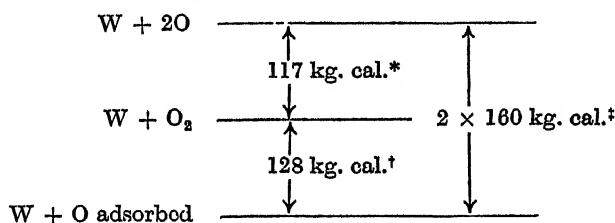
measured heat of sorption places the energy level corresponding to oxygen adsorbed 128 kg. cal. below the tungsten-gaseous oxygen level. The tungsten-gaseous atomic oxygen level is 117 kg. cal. above. When the temperature coefficient of the rate of desorption of oxygen is measured in order to calculate the energy of desorption the value so obtained is about 160 kg. cal. What has not so far been determined is whether this magnitude refers to atoms or molecules. The energy diagram cannot give any guidance on this matter, for energy of the order 160 kg. cal. will desorb a gram molecule of oxygen. Similarly 2×160 kg. cal. would be required to desorb two gram atoms of oxygen, i.e. 160 kg. cal. per gram atom.

ACCOMMODATION COEFFICIENTS OF MOLECULES ON LIQUID SURFACES.—In principle the technique of measuring accommodation coefficients on solid surfaces is straightforward. When liquid surfaces come in for consideration a number of new difficulties arise.

* Heat of dissociation of O_2. † Heat of adsorption. ‡ Energy of desorption.

None the less, experiments have now been made by Alty and Mackay (*Proc. Roy. Soc.*, A, **149**, 104, 1935 ; cf. *Phil. Mag.*, **15**, 82, 1932) on the interaction of water molecules with a liquid water surface. With such a system it is also possible to measure the fraction of impinging water molecules which condense to liquid water in addition to the fraction which come into thermal equilibrium with the water surface.

Briefly the method of determining the former quantity is to allow a drop of water to form at the tip of a glass tube placed in an enclosure where the vapour pressure of water is less than the saturation value and measure the rate of evaporation. By performing the experiment in this manner the weight (m) of water evaporated per drop formed and the surface temperature (T_s) of the liquid water (from the surface tension) are simultaneously measured. If p_s is the saturation pressure corresponding to T_s, then the maximum rate at which water can enter the liquid which is also the maximum rate at which it can evaporate is given by the equation

$$m = 0 \cdot 24 p_s T_s^{-\frac{1}{2}} \int_0^{t_o} A dt \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where A is the area in cm. of the drop at time t (sec.). If, however, only a fraction f of the molecules colliding with the surface enters the liquid the rate of evaporation will be smaller by this factor and hence equation (1) becomes

$$m = 0 \cdot 24 p_s T_s^{-\frac{1}{2}} . f . \int_0^{t_o} A dt$$

In practice evaporation does not take place into a vacuum and if p is the pressure of water vapour in the evaporation chamber the equation becomes

$$m = 0 \cdot 24 (p_s - p) T_s^{-\frac{1}{2}} . f . \int_0^{t_o} A dt$$

All the quantities except f in the equation may be measured and hence f can be immediately evaluated. It amounts to the low value of $0 \cdot 036$ at $10°$ C.

In order to determine the value of the accommodation coefficient (α) it is necessary to compute the heat leaving and entering the drop during its formation. The drop receives heat (1) from warm water entering the drop (W_1), (2) by conduction along the glass tip (W_2) and (3) by impact of water molecules from the gas phase (W_3). It loses heat by evaporation (W_4) and by the drop falling from the tip of the capillary (W_5). Hence

$$W_3 = W_4 + W_5 - W_1 - W_2$$

If α is the accommodation coefficient then W_3 is easily shown to be given by the equation

$$W_3 = 0 \cdot 24 p T_g^{-\frac{1}{2}} \int_{\cdot}^{t_0} A dt . C_p (T_g - T_s) \alpha$$

where T_g is the temperature of the gas. W_1, W_2, W_4 and W_5 can all be measured and hence α may be evaluated at once. At $40°$ C. $\alpha = 1 \cdot 0$.

This is a surprising result. Although water molecules come into complete thermal equilibrium on striking a liquid water surface they do not condense to form liquid water with anything like the same efficiency. So far water is the only liquid which has been studied by this technique. It will be of interest to see how other liquids behave, especially having regard to the fact that water is not a normal liquid.

SOME EXAMPLES OF THE USE OF ISOTOPES IN CHEMISTRY (EXCLUDING DEUTERIUM).—Attention was called in the first part of this review to the necessity for devising new technique in physical chemistry in order to attack problems which are at present either partially or completely insoluble by the well-established classical methods. Two comparatively recent developments have provided physical chemists with yet another aid in this direction. They are (a) the isolation of finite amounts of isotopic atoms, and (b) the discovery that radioactive isotopes of the elements of lower atomic weight may be fairly easily produced and that their lifetimes are sufficiently long to allow of their being used for chemical purposes.

The idea of employing isotopes for following certain chemical processes is not of course a new one. The first investigations in this line were made many years ago by Paneth, who determined the area of solid lead sulphate by allowing the solid to come into contact with a solution of lead sulphate containing iso-morphous thorium B sulphate in a known amount. The radioactive atom exchanged with the ordinary lead in the surface layer and a measurement of the radioactivity of the solution after equilibrium had been established furnished the data for a calculation of the surface of the lead sulphate. Another application of a somewhat similar nature was the demonstration that bismuth hydride BiH_3 could be prepared (*Ber.*, **51**, 1704, 1918). In this experiment an alloy of magnesium, bismuth and radium C (radium C is an isotope of bismuth) was treated with dilute hydrochloric acid and the gases evolved were shown to contain traces of a radioactive gas which had the properties to be expected from BiH_3. With the new discoveries such experiments are not now restricted to the heavier elements of the periodic

system, but extend to the lowest members, namely to hydrogen. We shall, however, omit any reference to deuterium since the subject has now grown too large to be profitably dealt with at present.

The methods used for isolating deuterium have also been partially successful in isolating the heaviest isotope of oxygen O^{18}. One use for it has already been found. Among organic chemists— though the question was first raised by van't Hoff—there has been much indecisive controversy as to the whereabouts of the bond which is broken on hydrolysis of an ester. The two possibilities are shown below.

$$(a) \qquad\qquad\qquad (b)$$

The arguments in favour of one or the other or both need not be mentioned here. Polanyi and Szabo have settled the matter once for all (*Trans. Faraday Soc.*, **30**, 508, 1934) by carrying out the hydrolysis of amyl acetate with water containing the heavy isotope of oxygen, O^{18}. The amyl alcohol so obtained was dehydrated with aluminium oxide and the density of the water remaining showed that none of the heavy oxygen had found its way into the alcohol. Hence, the heavy oxygen must go into the acetic acid. Scheme (*b*) therefore depicts the mechanism of hydrolysis.

The next example provides some definite information about the mechanism of the Walden inversion. In this reaction it has generally been assumed that substitution of the new atom or group for the old invariably results in the inversion of the order of the groups round the carbon atom. For example, if one halogen atom is being replaced by another it is supposed that the halogen ion, in virtue of the orientation of the dipole in the asymmetric molecules, approaches on the side of the carbon atom opposite to that where the halogen atom is situated, as is shown below :
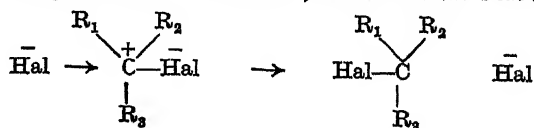
That this is really the mechanism and also that substitution or exchange, if the atoms are like, does not take place simply by the replacement of the halogen atom in the same position in the molecule may be elegantly shown by employing the radioactive isotope of iodine (Hughes, Juliusburger, Topley and Weiss, *J. Chem. Soc.*,

1525, 1935). When iodine is bombarded with neutrons, expelled from beryllium under the bombardment of α-particles, a radioactive isotope is formed which has a convenient, though for the present purpose rather short, half life-time of 24 min. The experiment to show that the above-mentioned contention is correct consisted in measuring the rate of exchange of radioactive iodine, contained in sodium iodide with dl-sec. octyl iodide and the rate of racemisation of d-sec. octyl iodide. Within 10 per cent. these reactions went at the same speed, which means that exchange is invariably accompanied by inversion.

Employing similar methods the exchange of iodine with iodide may readily be investigated (Topley, Juliusburger and Weiss, *J. Chem. Physics*, **3**, 437, 1935). For example, if radioactive iodine contained in sodium iodide is dissolved in water and iodine added, then on subsequently extracting the iodine with carbon disulphide part of the activity is found in the iodine extracted. In the complex ion $I_3{}^-$ the molecule of iodine loses its identity in the sense that the radioactive atom combines with a normal atom when the ion dissociates and so the radioactive atom passes into the carbon disulphide. It is surprising to find that the iodine atom in CH_3I and $CH_2 = CHI$ readily exchanges whereas that in ethyl iodide, n-propyl iodide, methylene iodide and iodoform does not exchange under the same experimental conditions. As is the case with the exchange reactions of deuterium these substitutions provide further examples of reactions in solution the kinetics of which will no doubt be of considerable assistance in due course, in yielding support or otherwise to recent theories of reaction in solution.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., D.Sc., The University, Glasgow.

PETROLOGY OF IGNEOUS ROCKS.—In his paper " Zur mineralogischen Klassifikation der Eruptivgesteinen," P. Niggli (*Schweiz. Min. Petr. Mitt.*, **15**, 1935, 295–318) examines the data of W. E. Tröger (in his book *Speziellen Petrographie der Eruptivgesteine*) in relation to his own mineralogical classification, and concludes that it should not now be difficult for the authors of various European systems of classification to come to agreement regarding the unification of nomenclature and the quantitative delimitation of igneous rock families.

S. J. Shand (*Journ. Geol.*, XLIII, 1935, 609–17) discusses twelve of the twenty recent proposals for the classification of igneous rocks. He finds a considerable measure of agreement in regard to the main factors of classification with serious divergences in details, and

points out where concessions by opposed parties would lead to approximate agreement. Shand has also recently described "An Olivine trachyte from Mont Dore, Auvergne" (*Geol. Mag.*, LXXII, 1935, 86–9) with a new chemical analysis, using it for the purpose of testing various classifications. He finds that his own system and Lacroix's are preferable to those of Niggli or Johannsen.

Further development of the saturation principle in classification according to Shand's method has led N. Svitalski (*Ukrainian Acad. Sci., Inst. Geol., Kiev.-Journ. Geol.*, **1**, Pts. 3–4, 1935, 1–44 (Russian), 44–8 (English summary)) to propose the division of igneous rocks into nine classificatory groups, which differ from each other by a gradual diminution of silica and by a concomitant gradual increase in the degree of undersaturation of their magmas with silica. The method of calculation adopted has a close resemblance to that of F. von Wolff.

E. Tröger gives chemical analyses and modal mineral compositions of ten igneous rocks from type localities which have not yet been quantitatively investigated (*Min. u. Petr. Mitt.*, **46**, 1934, 153–73). These include greisen, beresite, weiselbergite, sanukite, ornoite, bojite, corsite (orbicular diorite of Corsica), lakarpite, alnoite, and olivine-melilitite.

In their investigation of "The Rosiwal Method and the Modal Determination of Rocks," E. S. Larsen and F. S. Miller (*Amer. Min.*, **20**, 1935, 260–73) come to the conclusion that the mineral constituents in a thin section can be measured by this method with an error of only about 1 per cent. Greater accuracy cannot be attained and is not necessary in view of the much larger error which may be introduced by faulty sampling of a rock mass or even of a single hand specimen.

R. A. Daly (*Proc. Nat. Acad. Sci.*, **21**, 1935, 657–63) has further tested Idding's normative method of calculating the densities of rocks from their chemical analyses, and gives some applications of the results. The normative density of "plateau-basalt" comes out at 3·071, of Kilauean basalts at 3·131, and of Soderholm's weighted average of the Pre-Cambrian rocks of Finland at 2·705. The latter may represent a fairly accurate estimate of the density of the upper part of the sial layer.

In his "Bemerkungen zum geologisch-tektonischen Problem der magmatischen Differentiation," P. Niggli (*Schweiz. Min. Petr. Mitt.*, **15**, 1935, 153–9) discusses the question of the origin of granite, and contrasts the theories of assimilation with re-melting (Holmes) and of pure differentiation from basic magma (Bowen, Niggli, etc.) with conclusions favourable to the latter.

The posthumous memoir by the late J. H. L. Vogt, " On the Theory of the Parental Magma of Basaltic Composition " (*Norske Vidensk.-Akad., Oslo,* I. Math.-Nat. Kl. 1935, No. 6, 47 pp.), contains a large amount of diverse petrological material which may be summarised as follows : Vogt accepted a parental magma of basaltic composition, but believed that at an early stage it incorporated considerable amounts of heated rocks of more acid composition giving rise to " average " magmas with about 65 per cent. silica. The differentiations which have produced the common co-magmatic provinces took place within this " real " parental magma (65 per cent. silica), and not in the " original " parent magma (50 per cent. silica). His method, however, seems to overlook or at least obscure the differences between the igneous rocks of continental and oceanic regions respectively.

According to A. B. Edwards (*Proc. Roy. Soc. Vict.,* XLVIII, Pt. 1, 1935, 13–26) investigation of the olivine-basalt—trachyte associations of Victoria, Kerguelen, and Otago (N.Z.) supports the view that the alkali rocks are the normal differentiation products of olivine-basalt magma, provided that there is no undue amount of contamination by contemporary syntexis. Alkali rocks will thus develop within continental regions provided that their loci are not within an active orogen, quite as readily as in oceanic regions.

The great importance of ferromagnesian silicates as rock-forming minerals renders the study of equilibrium in the system $MgO—FeO—SiO_2$ by N. L. Bowen and J. F. Schairer (*Amer. Journ. Sci.,* **29**, 1935, 151–217) of much value. The results, which are too numerous to be summarised here, bear upon such petrological problems as pyroxenes and their equilibrium relations in magma, olivines and olivine-rich rocks, fractional crystallisation in igneous magmas, olivines and pyroxenes of metamorphic rocks, and the origin of quartz-basalts.

V. Sobolev discusses the " Siberian Traps as an Example of the Phenomena of Crystallisation Differentiation " (*Problems of Soviet Geology,* 5, No. 7, 1935, 635–42). While agreeing generally with Bowen's scheme of crystallisation-differentiation he points out that the FeO/MgO ratio in these basic magmas increases on differentiation more rapidly than the Ab/An ratio, but the accumulation of FeO in the granitic residual solutions is not absolute but relative, as pointed out by Eskola.

Discussing the petrogenesis of the Sudbury (Ont.) nickel irruptive in the first of a projected series of papers W. H. Collins (*Trans. Roy. Soc. Canada,* 1935, 123–77) arrives at the conclusion that the parental magma was originally homogeneous and separated, while

still in the liquid state, into the well-known norite and micro-pegmatite fractions. In a full review of this paper by D. L. R. (*Geol. Mag.*, LXXII, 1935, 285–7) the possibility is suggested that the micropegmatite layer was developed *in situ* as a result of the feldspathisation of overlying quartzites by means of alkali-rich emanations.

In a discussion of the same paper T. L. Walker (*Contrib. to Canadian Mineralogy*, 1935, *Univ. of Toronto Studies*, *Geol. Ser.*, No. 38, 23–30) suggests that the gradual increase in the amounts of TiO_2 and P_2O_5 with increasing density, and the fact that the heaviest rocks occur in the transition zone next to the norite, and the lightest on the micropegmatite side, is more readily explained by gravitative differentiation than by differentiation in the liquid state.

From a study of the differentiation of Hawaiian lavas, combined with a critical discussion of Barth's views (SCIENCE PROGRESS, Jan. 1933, 440), H. A. Powers (*Amer. Journ. Sci.*, 30, 1935, 57–71) arrives at the conclusion that the theory of fractional crystallisation alone is not adequate to explain the facts. He also criticises Barth's views on the sequence of pyroxene crystallisation in basaltic magma.

An exhaustive mineralogical and chemical study of differentiation in the Keweenawan basaltic lavas of Michigan leads T. M. Broderick (*Bull. Geol. Soc. Amer.*, 46, 1935, 503–53) to the conclusion that crystal settling (olivines and perhaps pyroxenes) and crystal floating (plagioclase) has occurred in some of the flows. At the same time in many flows a process of gaseous transfer has also taken place, whereby the ratios orthoclase/plagioclase and albite/anorthite become abnormally high, and normative quartz, orthoclase, albite, magnetite, ilmenite and apatite, with Mn, Cu and S, are concentrated in the upper parts of the flows. Broderick also discusses the deep-seated regional differentiation of the Keweenawan province with the conclusion that volatile transfer has played an important part.

Much valuable petrological material is contained in C. S. Ross's work on the " Origin of the Copper Deposits of the Ducktown Type in the Southern Appalachian Region " (*Prof. Paper* 179, *U.S. Geol. Surv.*, 1935, 165 pp.) relating to the later magmatic and hydrothermal stages of differentiation. A long and stimulating review of this work by C. N. Fenner (*Econ. Geol.*, 30, 1935, 928–36) deals particularly with the rôle of gases and vapours in the transport of minerals and ores.

P. Geijer discusses " Die nordschwedischen Eisenerze und verwandte Lagerstätten als Beispiele eruptiver Spaltungsprozesse "

(*Geol. Rundsch.*, **26**, 1935, 351–66). He states that these apatite-rich magnetite ores were differentiated in the late magmatic stage from the associated porphyries by the process of gaseous transfer.

N. Sundius's work on " The Origin of Late Magmatic Solutions containing Magnesia, Iron and Silica " (*Sver. Geol. Unders. Årsbok* 29, No. 7, 1935) leads to conclusions similar to Geijer's. The sulphide and skarn ores of Fennoscandia often show evidences of magnesia and iron metasomatism. Sundius believes that the metasomatising fluids did not always come from remote external sources, but may have been derived from associated local magmas as water-rich residual solutions. A more extended summary of this paper appears in *Nature* (April 11, 1936, 624).

In a paper on " Facies and Phases of Eruptive Rocks " (Russian, with English summary) M. A. Ussov (*Problems of Soviet Geology*, 5, No. 9, 1935, 795–812) stresses the stability of the minerals of eruptive rocks (*i.e.* lavas) and the slowness of their alteration in regions of stable tectonic constitution. He regards the " greenstone phase " as due to the action of magmatic emanations under conditions of intensive folding setting in soon after eruption.

The conversion of basic feldspar into albite by the action of solutions of sodium carbonate and silica is generally regarded as the basal reaction by which the spilites are generated from basalts. Experimental confirmation of the validity of this reaction has now been obtained in laboratory work under the direction of P. Eskola, with the aid of U. Vuoristo and K. Rankama (*C.R. Soc. Géol. de Finlande*, No. 9, 1935, 1–8). Anorthite heated under water vapour pressure with soda and silica, the latter either in form of quartz or of orthosilicate, together with an excess of CO_2 from sodium bicarbonate, was converted into albite-rich plagioclase.

From the study of Permian keratophyres, spilites and albite-diabases in eastern Oregon, and from an exhaustive review of the literature of the spilitic series, J. Gilluly (*Amer. Journ. Sci.*, 29, 1935, 225–52 ; 336–52) comes to the conclusion that spilitic rocks are derivatives of normal alkali-calcic magmas ; that their enrichment in albite is metasomatic in origin and entirely subsequent to their consolidation, the albitic solutions being derived either by the engulfment of wet sediments (Daly) or as residues of deep-seated magmas differentiated along trondhjemitic lines. A theory on this basis explains the common association of spilites with albite-granites, their frequent association with normal basalts, and their geosynclinal loci.

R. D. Terzaghi, in her paper on " The Origin of the Potash-rich Rocks " (*Amer. Journ. Sci.*, 29, 1935, 369–80), recalls Vogt's

suggestion that, near the end of the crystallisation process when water is present in relatively large quantity in the liquid phase, potash and plagioclase feldspar may be in equilibrium with a liquid having a higher Or : Ab + An ratio than if the melt were "dry." High pressure may be expected to displace equilibrium in the same sense. It is also probable that some potash-rich rocks apparently of igneous origin may have been produced by metasomatic processes such as the feldspathisation of quartzite.

In an important memoir on " The Genetic Significance of Biotite-Pyroxenite and Hornblendite," Miss D. L. Reynolds (*Min. Petr. Mitt.*, **46**, 1935, 447–90) further develops and generalises the views put forward as a result of her work on the Newry Complex (SCIENCE PROGRESS, April 1935, 700). The two above-mentioned rocks are shown to be derived from highly fluid magmas rich in volatiles. During intrusion they " soak " into the country rocks producing hybrids of various kinds, and giving off an advance-guard of alkali-rich emanations which cause widespread feldspathi-sation of quartzites and schists. The feldspathisation of sediments and their subsequent " soaking " by biotite-pyroxenite magma results in the production of the series shonkinite—monzonite—syenite, while similar action by hornblendite magma produces the series appinite—diorite—plagioclasite. These views are applied to the interpretation of numerous regions and series of igneous rocks.

F. F. Grout and W. W. Longley (*Journ. Geol.*, **43**, 1935, 133–41) show that in the Duluth sections where there are apparent grada-tions from granite to anorthosite there are no direct differentiation relations, but the apparent transition arises by the inclusion of anorthosite fragments and their reaction with the (granitic) " red rock."

Miss G. A. Joplin (*Geol. Mag.*, **72**, 1935, 227–34) comments on the frequent fine grain of basic xenoliths as compared with that of their host-rock. She suggests that the grain-size of such xenoliths may be reduced ; (1) by the development of granoblastic textures if the temperature is high enough ; (2) by the development of poikilitic crystals of hornblende or biotite, and their eventual dis-ruption ; and (3) by the formation of granular masses of pyroxene or criss-cross flakes of biotite during hybridisation processes.

PETROLOGY OF IGNEOUS ROCKS—GREAT BRITAIN.—The Haddo House gabbro mass described by H. H. Read (*Quart. Journ. Geol. Soc.*, **91**, 1935, 591–638) is made up of olivine-gabbro, quartz-gabbro, norite and cordierite-norite. It has been intruded into andalusite-cordierite-schists (Fyvie Series) and cordierite-biotite-gneisses (Ellon Series). Abundant xenoliths of all types of country-rocks except

the most widespread—the andalusite-cordierite-schist—are abundant in the norite and cordierite-norite. It is concluded that the norites have been produced by the assimilation of argillaceous materials.

Older and Newer Granites occur in the region described by W. O. Williamson in his paper on " The Composite Gneiss and Contaminated Granodiorite of Glen Shee, Perthshire " (*Quart. Journ. Geol. Soc.*, **91,** 1935, 382–422). The Older Granite (Duchray Hill Gneiss) was generated by the invasion of the pelitic types of Ben Lui Schists by oligoclase-quartz magma, with the production of oligoclase-mica-gneiss. The Younger Granite is a granodiorite which is thought to be intensely contaminated by Ben Lawers Schist material.

Williamson has also described some minor intrusions of the Glen Shee district (*Geol. Mag.*, **73,** 1936, 145–57) as aplites and pegmatites, granodiorite-porphyries and biotite-granodiorites, which occur around the Newer Diorite. There is also an interesting porphyry with clouded plagioclases which has been contaminated by cordierite-hornfels, and quartz-felsites containing cordierite, andalusite and garnet.

J. G. C. Anderson describes the Caledonian intrusions of the Arrochar region which extend across the upper reaches of Loch Lomond and Loch Long (*Geol. Mag.*, **72,** 1935, 263–83). In order of age the rock-types are fine-grained diorite, medium-grained diorite, coarse diorite and appinite, quartz-diorite and granite. Small masses of pyroxenite and kentallenite also occur. The complex is compared with the neighbouring well-known Glen Fyne—Garabal Hill mass.

The " Cairnsmore of Carsphairn Igneous Complex," described by W. A. Deer (*Quart. Journ. Geol. Soc.*, **92,** 1935, 47–76), is another Caledonian mass which consists of a central granite and successive zones of acid hybrids, tonalite and basic hybrids. The complex was initiated by the intrusion of gabbro followed, while still hot, by tonalite producing the basic hybrids. The tonalite was not fully crystallised when the granite intrusion followed, producing the uniform acid hybrids. The tonalite itself is considered to be due to hybridisation.

A " Xenolithic Pegmatite in the Dalbeattie Granite " is described by Malcolm MacGregor (*Geol. Mag.*, **73,** 1936, 171–85). Assimilation by reciprocal reaction, followed by mechanical disintegration of the xenoliths, has taken place. The xenoliths consist of granodiorites and various country-rocks. The assimilation is attributed to the high volatile content of the pegmatite.

The following is F. Walker's summary of his memoir on " The

late Palæozoic Quartz-dolerites and Tholeiites of Scotland " (*Min. Mag.*, **24**, 1935, 131–59) : " The E.-W. dikes of quartz-dolerite and tholeiite in Scotland and the related sills show marked resemblances, both chemical and mineralogical, to the Whin Sill of the north of England. They appear to belong to the same period of intrusion, which is roughly coeval with the Permo-Carboniferous folding and faulting. Chemical analyses of an unaltered residual glass in one of the tholeiites shows it to be highly siliceous, and there is therefore no reason for considering the micropegmatite of the very similar quartz-dolerites to be other than magmatic. It is suggested that the suite may have arisen through mild contamination of the parental Carboniferous olivine-basalt magma by prolonged contact with the ' sial.' "

Comparisons of three Scottish magmas, Devonian, Carboniferous and Tertiary, have been made by S. Elder (*Geol. Mag.*, **72**, 1935, 80–5) by means of graphs in which various bases are plotted against silica. The well-known calc-alkalic character of the Devonian and Tertiary magmas as compared with the Carboniferous, a distinctly magnesian tendency in the Devonian, and a ferriferous tendency in the Tertiary magma, have been brought out by this method.

D. Balsillie describes the first example of leucite-basanite in Great Britain which occurs among limburgitic intrusions in Carboniferous volcanic vents near Tantallon Castle, East Lothian (*Geol. Mag.*, **73**, 1936, 16–19). The leucite seems to be largely replaced by analcite, but enough remains for identification.

" The Tertiary Geology of Raasay, Inner Hebrides," described by C. F. Davidson (*Trans. Roy. Soc. Edin.*, **58**, Pt. II, 1935, 375–407), concerns igneous rocks alone. They consist of basalt lavas, granite and granophyre sills, sills of the olivine-dolerite—crinanite—picrite series, minor intrusions of many kinds from basalt to pitchstone, and volcanic vent-breccias. The petrography of these rocks is discussed with the aid of four new analyses.

Difficult and dangerous work by A. M. Cockburn on the crags of St. Kilda has resulted in the first detailed description of the geology of this remote island group (*Trans. Roy. Soc. Edin.*, **58**, Pt. II, 1935, 511–47). The islands consist of various gabbro intrusions, invaded by thick sheets of dolerite and by three later masses of granophyre. Dikes are not common, but there are many thin inclined sheets of both basic and acid types. It is probable that the islands represent the peripheral remnants of a ring complex originally about seven miles in diameter. The petrography of the rocks is described with the aid of ten new analyses.

The Craignish (Argyll) dikes described by A. Allison (*Geol.*

Mag., **73,** 1936, 73–87) are members of the Mull Swarm about 20 miles south-east of its focus. Crinanites and olivine-dolerites, tholeiites and allied basalts, porphyritic basalts, leidleites and allied andesites, are the main components. The intrusion of the dikes has necessitated a crustal stretching of about one in forty.

A valuable summary account of the Tertiary igneous geology of north-east Ireland has been contributed by J. K. Charlesworth, J. E. Richey, and S. I. Tomkeieff to an excursion pamphlet of the Geologists' Association (*Proc. Geol. Assoc.*, **46,** 1935, 464–78). Dr. Richey also publishes " Further Evidence concerning the Age of the Gabbros of the Slieve Gullion District " (*ibid.*, 487–92), with analyses of biotite-gabbro and quartz-gabbro. He shows that these rocks, formerly attributed to Tertiary igneous activity, really belong to the Caledonian suite.

The Tertiary north-west dikes constituting the Mourne Swarm have been described by S. I. Tomkeieff and C. E. Marshall (*Quart. Journ. Geol. Soc.*, **91,** 1935, 251–92). The rocks include only a small proportion of olivine-bearing types, the majority being olivine-free and of intermediate composition. All of them are considered to be of abnormal composition, and some are definitely hybrids due to the resorption of granite. A theory of syntectic differentiation is believed to account for the origin and composition of the swarm.

J. K. Charlesworth and J. J. Hartley have delimited and described two new Tertiary dike-swarms in north-eastern Ireland (*Irish Nat. Journ.*, **5,** No. 8, 1935, 4 pp. reprint), the Tardree and Hillsborough Swarms, which they believe are associated respectively with the Tardree rhyolite mass and the Hillsborough quartz-porphyry.

Basic hybrids and xenoliths which occur at the junction between eucrite and limestone at Barnavave, Carlingford, are described by S. R. Nockolds (*Geol. Mag.*, **72,** 1935, 289–315) with a wealth of mineral and chemical detail. Contaminated granites from the same locality are also dealt with. The mineral transformations which have taken place are complex and are followed in great detail.

B. Simpson shows that the Eskdale Granite of Cumberland has a laccolithic form, and was intruded under great pressure into the Borrowdale volcanic series (*Proc. Geol. Assoc.*, **45,** 1934, 17–34). Three granites are involved in the mass, and there has been much contamination by country-rocks. Widespread sericitisation and chloritisation represent deuteric effects.

" The Basic Intrusive Rocks associated with the Cambrian Inlier near Malvern," described by F. G. H. Blyth (*Quart. Journ. Geol. Soc.*, **91,** 1935, 463–78), are members of a spilitic series some

of which have affinities with the teschenites. Shales adjacent to the intrusions have been adinolised and the margins of the sills themselves have also been enriched in albite, suggesting reciprocal exchange of material between the magma and the shales.

BOTANY. By PROFESSOR E. J. SALISBURY, D.Sc., F.R.S., University College, London.

ECOLOGY.—Irrigation experiments of upland pasture in Scotland are reported by R. G. Heddle and W. G. Ogg (*Jour. Ecol.*, XXIV, 1936). These show that whereas the untreated pasture had an exchangeable calcium ranging from 0·03 per cent. to 0·37 per cent., flushed pasture adjoining gave values ranging from 0·08 per cent. to 0·46 per cent., and that whereas the reaction of the untreated pasture varied between pH.4·25 and pH.4·66, the flushing reduced the acidity to from pH.4·92 to pH.6·09. This amelioration of the soil conditions is accompanied by an increase of the number of vegetation shoots per unit area from an average of 4560 to 6450. This improvement is chiefly noticeable in *Agrostis*, *Festuca rubra*, *Holcus lanatus* and *Trifolium repens*, whereas a reduced abundance of the following species was noted : *Festuca ovina*, *Deschampsia flexuosa*, *Galium saxatile* and *Vaccinium myrtillus*.

In the same Journal P. W. Richards records observations on the Rain Forest of Mount Dulit, Sarawak, which includes useful data respecting the physiognomy. There is a discontinuous stratum of tall trees averaging about 34 m., below which there is a stratum of trees of varying heights in which trees averaging about 18 m. and others averaging about 8 m. with deep narrow crowns tend to produce two sub-strata.

A detailed account of the autecology of *Mercurialis perennis* by the late Dr. S. R. Mukerji contains a large number of interesting observations on the distribution and biology of this species. The seed output is normally low and only about 1 per cent. to 10 per cent. (av. 5 per cent.) appear to be capable of germination, which normally occurs from February to April. The fruit is explosive and ejects the seeds to distances of 3 to 4 m. but extended range is usually due to vegetative spread which proceeds at a rate of from 10 to 15 cm. per annum though occasionally as much as 40 cm. The edaphic factors responsible for the frequency of the species are fully dealt with.

In an account of the Arborescent Senecios of the Congo L. Hauman lays stress on the slow rate of growth of these plants which occupy a zone of continually low temperature. They occur between 2700 m. and 4500 and attain their chief development

between 3300 m. and 4300 m. These altitudes correspond to the isotherms of 12° to 3° and from observations of leaf development the author estimates that the largest specimens of these trees are probably of the order of 200 years old. Pollinating insects were not observed and self-pollination appears to be the rule. The paper is illustrated by a number of excellent photographs of these interesting plants in their natural habitats. (*Rev. de Zoo. et Bot. Africaines*, XXVIII, 1–75, 1935.)

E. L. Flory (*Ecology*, **17**, 63, 1936) has studied the environmental conditions in prairie and an adjacent area cultivated and cropped with maize. The data furnished cover a three-year period and show a slight increase in soil density resulting from conditions of cultivation, especially in the surface six inches where the increased density amounted to 16 per cent. During a heavy fall of nearly 2 inches of rain the greatly increased run-off under cultivation was demonstrated. This amounted to 60 per cent. of the total precipitation as compared with only 1 per cent. run-off under prairie. The consequent erosions were in the ratio of 100 : 1. The pore space was reduced by cultivation by about 12 per cent. and the rate of percolation through the soil to about one-half. The average daily loss of water per square foot of prairie during the three years was 1·46 lb. of which 0·59 lb. was due to transpiration. It was estimated that the total leaf surface on a square foot of prairie attained a summer maximum of about 1000 sq. in.

Root-hair production in relation to the development of the piliferous layer has been investigated by K. Wilson (*Ann. Bot.*, **4**, 121–54, 1936). He shows that the development of root-hairs increases with the favourability of the environment for growth and is not associated with retardation of elongation of the root. By means of a " Root-Hair Index " analogous to the " Stomatal Index " it is shown that root-hair production varies greatly. Index values observed were 5 per cent. to 14 per cent. in *Stratiotes aloides* ; 6·1 per cent. to 17·5 per cent. in *Hydrocharis* ; 11 per cent. to 18 per cent. in *Carex paludosa* ; 13 per cent. to 46 per cent. in *Nasturtium officinale* ; 33 per cent. to 53 per cent. in *Scrophularia aquatica* ; 12 per cent. in *Veronica beccabunga* ; 37 per cent. to 40 per cent. in *Ranunculus sceleratus* ; 20 per cent. in *Hydrocotyle vulgaris*. The increase of absorbing surface consequent on root-hair development was 1·6 times for *Alisma plantago* ; approximately 2½ times for *Ranunculus sceleratus, Nasturtium officinale, Hydrocotyle vulgaris* and *Veronica beccabunga* ; about 4 times for *Scrophularia aquatica* and *Elodea canadensis* ; about 6 times for *Carex paludosa, Potamogeton densus* and *Callitriche stagnalis* ; for

Stratiotes ca. 9·5 times and for *Hydrocharis* 14·8 times. It is evident therefore that the surface increase is very appreciable, and it is suggested that though in water plants this can have no significance as regards water absorption it may have an important bearing on the absorption of salts. The author emphasises that though in certain species the cells of the piliferous layer are differentiated into trichoblasts and epidermal cells and that the proportion of the former is relatively constant, yet the proportion of the trichoblasts (or potentially root-hair forming cells) which actually develop root hairs varies widely.

An elaborate study of the root development in *Betula* has been published by E. Laitakari (*Acta Forestalia Fennica*, **41**, 1936) In the root-system of the Birch a horizontal and vertical part can be distinguished although the latter may not infrequently be absent. The average rate of growth in length of the roots varied from 13 to 43 cm. per annum though extreme increments of 2 m. in a year were encountered. The mycorrhiza is most plentifully developed in moorland soils and least on sandy soils. The extent of the root systems showed wide variation and marked plasticity. On dry sandy soils the type tended to be " extensive " whilst on good soil it approaches the " intensive " and, in general, the area occupied decreases as the quality of the site improves, a feature which has already been shown to obtain in *Pinus sylvestris*. The actual areas observed ranged from 2 sq. m. to 500 sq. m. The depths of the horizontal roots were from 2·8 cm. to 31·1 cm. according to size of tree and soil type. On moraine soils the depths were greatest (av. 20 cm.) and on water-logged soils least (*ca.* 8 cm.). The vertical roots penetrated to depths of over 2 m., being deepest in clay.

MORPHOLOGY.—*Technical Bulletin*, No. 16, of Macdonald College, is devoted to the means of identification of pasture grasses by their vegetative characters. The authors, F. S. Nowosad, D. E. N. Swailes and W. G. Dore, have provided descriptions of the vegetative features of 39 species, each accompanied by illustrations of the ligular region of the leaf as seen from the front and side. Amongst the more striking types figured may be cited *Echinochloa crus-galli* which possesses no ligule, thus recalling the recessives of this type in the wheat. A striking example of the type of ligule consisting of hairs is afforded by *Panicum lannginosum v. implicatum*, whilst a similar ligule in *Danthonia spicata* is accompanied by long hairs at the margins of the collar, suggesting a similar relation to the pronounced auricles such as occur in *Agropyrum*, *Lolium* and *Elymus*.

From a study of the vascular supply of the flowers of the

Cistaceæ Miss Saunders concludes that the ground plan exhibits alternation of the whorls in relation to one another though there is some displacement of the members themselves which has led to the diverse interpretations that have been put forward. The andrœcium is composed of five groups alternating with the petals, and where five carpels are present these alternate with the stamen groups. The genera *Cistus*, *Fumana*, *Helianthemum* and *Halimium* show progressive stages in reduction of two calyx segments which in the extreme case of *Halimium* are completely fused with two of the unreduced sepals as indicated by the vascular supply (*New Phyt.*, XXXV, 47, 1936).

CYTOLOGY.—In a detailed study of *Allium schoenoprasum*, A. Levan (*Hereditas*, XXII, 1–128, 1936) reports that the majority of the numerous types he examined are diploid (Zn= 16). These diploid types include, *inter alia*, *A. typicum*, *A. sibiricum* and *A. alpinum*. An autotetraploid and an allotetraploid are also described. Giant pollen grains were found to occur quite frequently and are associated with omission of the second meiotic division. Breeding experiments between diploid and tetraploid types resulted in the production of individuals exhibiting a considerable range of chromosome numbers ; from a study of these it appeared that self-fertility increased with increase in the chromosome number of the individual.

In the same journal (p. 167) O. Heilborn concludes from studies of species of Carex that chromosomes of equal size in the haploid complement tend to be associated together both at the metaphase and prophase.

ENTOMOLOGY. By H. F. BARNES, M.A., Ph.D., Rothamsted Experimental Station, Harpenden.

GENERAL ENTOMOLOGY.—Population studies on insects can conveniently be divided into those in which the insects are investigated under controlled conditions in the laboratory and those in which the insects are under observation in the field. Among those of the first type, D. S. MacLagan and E. Dunn (*Proc. Roy. Soc. Edinburgh*, **55**, 1936, 126–39) have analysed the growth of a population of the weevil *Sitophilus oryzœ*. In this case the internal resistance to the population's growth was brought about by a lowering of the fecundity and fertility and by an increase in the mortality of the young larvæ and the eggs. The limitation of numbers resulted principally from thigmotropic influences operating automatically. The copulation-frequency increased up to a certain density and then decreased according to a definite biological law, such that log. Y = log. $a - b$ log. X, where Y is the copulation-frequency and X the

population density. Since natural populations exert an automatic check on their numerical increase, the organism itself imposes the ultimate limit to its own abundance in the absence of other factors of control.

Another study has been made by S. E. Flanders (*J. Econ. Ent.*, **28**, 1935, 898–900) on the effect of host density on parasitism. In this instance the effect has been measured by the progeny per parasite and the percentage of parasitism under two series of environmental conditions. In one series the host density and in the other the numbers of parasites varied, in other respects the two were identical. With increasing host (*Sitotroga cerealella*) densities the number of progeny per parasite increased until a maximum was reached and maintained. It became constant only after the number of hosts within the sphere of action of the parasites, in this case *Trichogramma minutum*, exceeded the total reproductive potential of the parasite. The rate of increase was inversely proportional to the number of parasites liberated, owing to superparasitism taking place. The percentage of parasitism is also discussed.

H. S. Smith (*J. Econ. Ent.*, **28**, 1935, 873–98), using the Verhulst-Peal Law and one of Volterra's equations, comes to the conclusion that the density-independant mortality factors alone cannot determine the equilibrium position. Climate is probably not always a density-independant mortality factor; but the characteristic density-dependant mortality factors are parasites, predators and disease. F. S. Bodenheimer previously (*Biol. Zbl.*, **48**, 1928, 714–39) assumed that the equilibrium of insect populations in nature is maintained almost entirely by abiotic factors, principally climate, and that biotic factors are of little importance.

Among recent studies of populations in the field, one on gall midges (Cecidomyidæ) by H. F. Barnes may be quoted. This investigator has recently discussed the results of five separate yet interlocking studies (*J. Animal Ecology*, **4**, 1935, 254–63). From the data accumulated it is quite evident that successful field population studies can be carried out on such insects as gall midges. Major fluctuations have been revealed and numerous hints as to the factors involved have been provided. It is claimed that the " weather " acting directly on the insects, or through their host plants, or through its effect on the insects' parasites seems to dominate over every other factor.

Another field population study is one by J. Ford (*J. Animal Ecology*, **4**, 1935, 195–207). He has shown that the total density of population in the soil on meadow land was 263,755,800 individuals per acre, of which over 90 per cent. were springtails (Collembola).

This figure is far in excess of what previous workers on soil populations, such as Morris, Cameron and Edwards, found. The density of population of all forms in the surface vegetation was 8,813,640 per acre. Only 132 species were taken from the soil and vegetation together. The majority of the organisms were found near the surface of the soil, few being found below a depth of 4½ inches and none below 8 inches. In the case of Collembola the average of samples taken in the winter months showed a rising population to December and a falling one from January to May.

The need for accurate sampling methods in all field population studies is becoming realised more and more. J. Marshall (*Ann. Appl. Biol.*, **23**, 1936, 133–52) has described statistical methods and the technique used in determining the distribution of egg laying by the American bollworm (*Heliothis obsoleta*) in the field.

Research on the activity and abundance of insects has been carried out at Rothamsted Experimental Station for some years. Some of the results have now been published. Such investigations have on the one hand entailed the use of various types of traps. The first was a light trap devised so that the time at which the insects are caught can be ascertained. C. B. Williams (*Trans. R. Ent. Soc. Lond.*, **83**, 1935, 523–55) has described the mechanism of this trap. The light trap naturally will only capture those insects which are phototropic, capable of flight and active at night. A second type of trap has also been recently described by C. B. Williams and P. S. Milne (*Bull. Ent. Res.*, **26**, 1935, 543–52). This consists of two nets, on a light framework about 12 feet in diameter, driven by electricity and so caused to rotate about five times a minute. This trap will catch flying insects by day or by night whether phototropic or not. W. M. Davies (*Bull. Ent. Res.*, **26**, 1935, 553–8) has ingeniously modified this trap so that the motive power is water.

On the other hand, having caught the insects it is necessary to analyse the data. As a matter of interest during the first year the light trap was used 109,344 insects were caught and 103,362 in the second. C. B. Williams (*loc. cit.*) has dealt with the times of activity of certain nocturnal insects, chiefly Lepidoptera. The method of analysing the figures are described. Among the results, it is demonstrated that taking all insects together the maximum number is at the beginning of the night with a steady fall towards dawn. The Diptera, which made up 76 per cent. of the total, show a maximum in the first period (the night was divided into eight equal periods), and the Lepidoptera (16 per cent. of the total) later in the second to fourth periods. Certain species of Lepidoptera are dealt with separately and they all show an individuality of distribution. One

species of Noctuid (*Trachea secalis*) is shown to have a strikingly different distribution in the two sexes, other moths having less marked differences. In all such cases the females appear in the trap earlier than the males. The Hemerobiidæ and Chrysopidæ (Neuroptera) caught in the light trap have been discussed in a separate paper by C. B. Williams and F. J. Killington (*Trans. Soc. Brit. Ent.*, **2**, 1935, 145–50).

The biological control of any insect depends for its success on so many and intricate factors that a vast amount of careful research has to be done in order to assess the progress made after the liberation of the insect's enemies. Such work has been carried out since 1929 by R. W. E. Tucker in Barbados on the control of *Diatrœa saccharalis* in sugar cane by frequent liberations of mass reared *Trichogramma minutum*. His results have been published from time to time in the *Barbados Agricultural Journal*. In a recent contribution (*loc. cit.*, **4**, 1935, 25–50) all the data obtained have been reviewed. The annual liberation of *Trichogramma* is considered to be a permanent necessity, but at the same time it is highly successful and eminently practicable on account of the ease, cheapness and economic effectiveness of routine mass rearing of *Trichogramma*.

The important rôle fungi and bacteria play in the nutrition of many insects is well known. D. B. Creager and F. J. Spruijt (*Ann. Ent. Soc. Amer.*, **28**, 1935, 425–37) have shown that the presence of the basal-rot *Fusarium* among other fungi is necessary for the normal development of the lesser bulb fly, *Eumerus tuberculatus*.

COLEOPTERA.—The morphology of the Staphylinidæ is the subject of a detailed study by R. E. Blackwelder (*Smithsonian Misc. Coll.*, **94**, No. 13, 1936, 102 pp.).

In a paper (*Trans. R. Ent. Soc. Lond.*, **83**, 1935, 449–82) on the biology of the Bostrychid beetle *Rhizopertha dominica* Fab., a pest of stored grain, C. Potter gives a list of the substances recorded as its food and describes the life history of specimens fed on synthetic whole-meal flour under controlled conditions of temperature and humidity. A list of the natural enemies is also given; they include fungi, predacious mites and hymenopterous parasites.

The biology and morphology of certain wireworms are the subject of a thesis presented to l'Ecole Polytechnique Fédérale, Zurich (Flawil, 1934, 133 pp.), by E. Guéniat. He was unable to establish the fact that the larvæ descend into the lower levels of the soil during the winter, neither were biological differences found between *A. obscurus*, *A. lineatus* and *A. sputator*. Nevertheless, there is a large amount of interesting data given.

A full account, well illustrated, of the beetle *Brachyderes incanus*

L. has been written by H. J. de Fluiter and P. A. Blijdorp (*Meded. LandbHoogesch., Wageningen*, **39**, 1935, 72 pp.). J. H. Fidler (*Ann. Appl. Biol.*, **23**, 1936, 114–32) has described and figured the immature stages of *Melolontha melolontha, Amphimallus solstitialis* and *Serica brunnea*.

H. C. Hallock (*J. N. Y. Ent. Soc.*, **43**, 1935, 413–25) has made an interesting study of the movements through the soil of larvæ of the oriental beetle (*Anomala orientalis*). The larvæ were found to move downwards to depths of 8–17 inches when the soil temperature at the 3-inch level reached 50° F. The return upward movement after hibernation starts at a soil temperature of about 43° F. and is completed at 50° F. Oviposition took place at depths of 1–11 inches. Evidence is given to show that the larvæ move horizontally away from plants they have destroyed. Application therefore of insecticides only on patches where the turf has been damaged will not kill the pest.

LEPIDOPTERA.—It has been well known that the maize ear caterpillar (*Heliothis obsoleta*) is cannibalistic under certain conditions. G. W. Barber (*U.S. Dept. Agric., Tech. Bull.*, No. 499, 1936, 18 pp.) has studied this habit. He has found that the extent to which cannibalism occurs in the ears of maize depends largely on the character of the husk, as this determines the points of entry of the larvæ into the ear and the feeding area within it. Ears which have long tightly wrapped husks suffer least damage from the caterpillars as in such cases the extent of cannibalism is greatest. Cannibalism can be increased by improving the nature of the husk protection to maize ears. This would result in fewer caterpillars reaching maturity and so act as a control on the insect numbers.

HEMIPTERA.—Blood-sucking insects regularly contain intracellular micro-organisms, but they were said to be absent in the Triatomidæ which contains the genus *Rhodnius* among others. V. B. Wigglesworth (*Parasitology*, **28**, 1936, 284–89) now describes a bacterial organism which occurs constantly in *Rhodnius prolixus*. R. P. Hobson (*Biochem. J.*, **27**, 1933, 1899–1909) found that sterile blood was a deficient diet for blowfly larvæ unless " vitamin B " was added. This same investigator (quoted by Wigglesworth, *loc. cit.*) has done a few experiments adding the bacteria from *Rhodnius* to sterile horse serum and then feeding *Lucilia* larva on the mixture. The results seem to show that when blood is so infected it becomes an adequate diet for insect growth. These observations support the view that symbiotic organisms in exclusively blood-sucking insects provide an endogenous source of vitamin.

The development of catalepsy or muscular rigidity, often termed

death feigning, among the Heteroptera has been studied and analysed by U. Godglück (*Zeits. wiss. Zool.*, **146**, 1935, 678–721).

The aphid *Myzus persicæ* is recognised as a vector of virus diseases of potatoes. Field observations by W. M. Davies and T. Whitehead (*Ann. Appl. Biol.*, **22**, 1935, 549–56) have shown that migrating alate forms are the main source of the initial infestation of *M. persicæ* on the potato crop. The proportion of migrating alatæ infected with virus diseases, in a district where the spread was rapid, proved to be particularly small. The authors conclude that the introduction of virus diseases into a healthy stock by migrating alatæ is slight, but this amount is subsequently spread by apterous forms moving within the crop.

HYMENOPTERA.—M. F. Bowen (*Ann. Ent. Soc. Amer.*, **29**, 1936, 119–25) has compared the productivity, longevity and developmental period of two structurally similar species of *Trichogramma* when both were subjected to similar environmental (experimental) conditions. Statistically the differences found were significant. This paper is yet further evidence that physiological races exist and that quantitative studies in certain types of taxonomic work are essential. The Massachusetts species studied exhibited greater variability than the Louisiana one. This fact might be taken to indicate that the former species would be adaptable to a wider range of environmental conditions than the Louisiana or less variable species.

An exceedingly interesting apparent correlation between the feeding habits of certain parasitic Pteromalids and the condition of their ovarian follicles has been demonstrated by S. E. Flanders (*Ann. Ent. Soc. Amer.*, **28**, 1935, 438–44). When the ovarian follicles reach a certain stage of development a change from a carbohydrate diet to a protein one (its host's juices) takes place. Later, when environmental conditions inhibit oviposition, the follicles and the eggs apparently disintegrate and are absorbed. The parasite then reverts to a purely carbohydrate diet. A period of " phasic castration " follows which may last as long as nine months at between 65° and 80° F. The end of this period is apparently synchronised with the appearance in the field of the susceptible stage of the host. Then the diet becomes protein, the ovarian follicles develop and oviposition takes place.

DIPTERA.—Roubaud and his co-workers have previously shown that in France there are definite biological races of *Culex pipiens*. They were distinguished as follows : (a) *C. pipiens pipiens* undergoes cyclic hibernation (asthenobiosis), does not oviposit without blood meals (anautogeny) and requires a large space for pairing (eurygamy);

(b) *C. pipiens autogenicus* does not hibernate but breeds continuously; oviposits fertile eggs without any food in the adult stage, the ova being matured at the expense of reserves accumulated in the life stages (autogeny); and mating takes place even in very small spaces (stenogamy). P. Tate and M. Vincent (*Parasitology*, 28, 1936, 115–45) have confirmed the presence of these two races in Europe but have come to the conclusion that eurygamy and stenogamy are more constant differentiating characters than autogeny and anautogeny. But all the characters must be taken into consideration. For instance, Roubaud described the autogenous race as an urban one, but the present workers among others have found this race in rural districts. In fact, it is highly probable that both races do sometimes occur in the same district. Crossmating experiments have shown that interbreeding can take place and this may account for De Boissezon's results which contradict those of most other workers. P. Tate and M. Vincent have also found that the English anautogenous race, unlike the French one, is not subject to obligatory cyclical hibernation and cannot store up fat-body at the expense of larval tissues without food in the adult stage. Blood meals are not necessary but carbohydrate food in the form of fruit juice is essential for the development of fat-body in adults of the English anautogenous race. This has recently been confirmed by De Buck (*Z. angew. Ent.*, 22, 1935, 242–52) working on Dutch races. Light also influences the activity of the females as regards gorging and perhaps also hibernation. One of the main points resulting from this investigation is that prolonged autogenous development extending over three years involving 45–49 generations does not entail loss of vigour. Roubaud claimed that spanogyny, or the gradual decrease in numbers of females produced in succeeding generations, followed prolonged autogeny.

The morphology and biology of another cambium miner (see SCIENCE PROGRESS, No. 111, 1934, 508) has been worked out by E. Kangas (*Communicationes instituti forestalis Fenniæ*, 22.1, 1935, 31 pp.). This new species, *Dendromyza betulæ*, lives on *Betula*.

An account of the morphology, development and changes following emergence, of the ptilinum of the blowfly, *C. erythrocephala*, is given by Joyce Laing (*Q.J.M.S.*, 77, 1935, 497–521). G. Fraenkel (*Proc. Zool. Soc. Lond.*, 1935, 1936, 893–904) has made observations and experiments on this blowfly during the first day after emergence. It is common knowledge that such flies, when they have emerged under ground, dig their way out of the soil by the action of the ptilinum. Fraenkel describes this digging in detail and finds that its direction is determined by light, not by gravity. The "walker"

or newly emerged fly then becomes inflated with air and the wings expand. These processes are described. During digging these reactions are inhibited and postponed. Air-pumping occurs independently of external and internal stimuli, starting automatically a certain time after emergence and stopping automatically after a certain period. Fraenkel has proved experimentally that Evan's hypothesis (*Bull. Ent. Res.*, **26**, 1935, 115–22) that the first filling of the air-sacs is due to negative pressure in the body arising from the diffusion of air out of the gut is correct. Colouring and hardening of the skin are inhibited and postponed during prolonged digging.

OTHER ORDERS.—A good detailed account of the genus *Aptinothrips* Haliday by E. R. Speyer has appeared (*Trans. R. Ent. Soc. Lond.*, **83**, 1935, 483–508).

A. E. Emerson (*Ann. Ent. Soc. Amer.*, **28**, 1935, 369–95) has demonstrated, by a study of termitophilous Staphylinids and a statistical study of variation in the termites, the presence of physiological species for the first time among the Termites. In this case there was no geographical or ecological isolation but probably the differentiation of the two species was based on reproductive isolation only.

ARCHÆOLOGY. By E. N. FALLAIZE, B.A.

PREHISTORY AND CHRONOLOGY.—Although a relative chronology based upon stratigraphy serves the purpose of the working archæologist in many ways with a greater efficiency and a broader significance than an absolute system, notwithstanding the greater and more assured precision of the latter, it is not without its limitations. These become strikingly apparent when a chronological terminology adapted to localised conditions is given a wider application on account of cultural resemblances, which do not afford evidence, one way or the other, of a chronological relation or parallel. This is one of the matters to which Professor Gordon Childe referred when he addressed the Prehistoric Society (formerly the Prehistoric Society of East Anglia) as its first president in its reorganised form (*Proc. Prehist. Society*, 1935). His address, one of the most important pronouncements on the general principles and methods of archæological studies which has been made in recent years, crystallised a number of archæological discontents and pointed the way along which, in his opinion, future advance should lie. It has, of course, long been apparent that the conventional divisions of prehistoric archæology, in so far as they represent stages of cultural advance, do not necessarily carry with them a dating. A Stone Age culture of palæolithic type, for example, does not necessarily

imply a quaternary dating—many modern savages have possessed a Stone Age culture at the time of their first European contacts—nor even where such a Stone Age culture represents an early phase or series of phases of cultural development, as in South Africa, does it necessarily follow in age and character the sequence which the traditional system of archæological studies has evolved from the evidence of western Europe. Yet until recently that assumption has been almost invariable in the archæological investigation of Stone Age cultures outside Europe. In the south-western United States an attempt even has been made to identify every one of the European palæolithic cultures in a Stone Age which is almost certainly purely " neolithic."

Professor Childe argued for certain fundamental changes of nomenclature, point of view, and method. Thus while he agrees that the old classifications of stone, bronze and iron are cultural rather than chronological, owing to their wide distribution in time as well as in space, he himself would be prepared to retain them as expressing phases in economic development, which have their place in a temporal relation. They thus become even more significant, when they are viewed in the light of what Professor Childe regards as the present aim of archæology, which centres not so much on the intrinsic interest of the objects recovered, as on the reconstruction of the cultures of which they are an indication. It follows, he went on to maintain, that in the worldwide view of prehistoric problems, which must now be taken, since the chronology of the sequence of cultures, based on archæological finds in France, breaks down, the chronological problem must be solved through the relation in space and time of the various distinguishable cultures *inter se.*

EARLY MAN IN INDIA.—An important investigation which had for its object the discovery of evidence bearing on the early history and culture of man in India has been carried out by an expedition in charge of Dr. Hellmut de Terra of Yale University, which in 1935 visited north-west India and studied the country extending from the Kashmir Valley to the Salt Range lying between the Indus and the Jhelum, a sector comprising the slope of the Himalayas and the adjoining plains of the Punjab, with their more recently uplifted ridges. General considerations, as well as the discovery of fossil apes in the Sivalik Hills, make this an exceedingly promising field of research, which Dr. de Terra has visited on two previous occasions. On this visit he was accompanied for part of the time by Dr. Teilhard de Chardin, the well-known authority on the geology and archæology of China, Dr. T. T. Paterson of

Cambridge, Dr. N. K. N. Aiyengar of the Geological Survey of India, D. Sen of Calcutta, and two temporary assistants. In a preliminary report (*Nature*, April 25, 1936, p. 686) Dr. de Terra records that the Pliocene formations yielded a number of fossils of the higher primates, which are still under examination. The Pleistocene succession has been worked out provisionally. So far as the evidence bears on the question of early man, it was found that the prehistoric cultures of the area range from the Early Palæolithic to Late Middle Palæolithic and Neolithic. The oldest is represented by worn flakes in the Boulder Conglomerate and by bifaces, cleavers and hand-axes associated with gravels perhaps of similar age. The Soan culture, mainly a flake culture named after the Soan River near Rawalpindi, may range from the Early to Late Middle or even Upper Palæolithic. Workshops are numerous below the Potwar loess-like silt. The deposits of the Upper Siwalik stages it was found could be linked with the Kashmir glacial cycle. Thus the worn flakes of the Boulder Conglomerate belong to the second ice advance in a fourfold Himalayan glaciation, the Lower Palæolithic Soan culture belongs to the long second interglacial, represented in the Upper Karewa beds erosion, the Soan culture in the lower gravel and silt belongs to the third ice advance, with a terminal moraine at 6,500 feet, and the doubtful Upper Palæolithic to the fourth ice advance, with a terminal ice moraine at 8,000 to 10,000 feet. Dainelli referred the first glaciation to the Mindel advance, but with this Dr. de Terra is inclined to disagree.

Excursions further afield produced some interesting results, which included Chellean and Acheulean tools from the Nerbadda valley with Middle Pleistocene fauna, as well as Soan artefacts; while field work in the Madras and Bombay Presidencies produced a widespread occurrence of Soan, a Microlithic industry, and a proto-neolithic culture. The Early Neolithic appears to be akin to that of northern Europe, with types similar to types of the latest Upper Palæolithic of North Africa.

ARCHÆOLOGY AND ABSOLUTE CHRONOLOGY—EUROPE.—In his recently published *The Mesolithic Settlement of Northern Europe*, Dr. J. G. D. Clark, in working out the chronology of his period, makes use of various auxiliary investigations, more especially in palæobotany and geology, in the latter relying on the investigation of the silt deposits according to the methods elaborated by de Geer and in the former on pollen analysis. In this way not only is he able to correlate cultural succession with changes in vegetation and climate, but he also suggests an absolute dating for the beginning and end of the Mesolithic Age.

An attempt to fix an exact chronology on a grander scale through the study of the effects of solar radiation is discussed in its bearing on the archæology and palæontology of Central Europe by Dr. Friedrich E. Zeuner in " The Pleistocene Chronology of Central Europe " (*Geological Mag.*, 72, 1935, 350–76). Briefly his argument is that during the last twenty years the investigations by geologists of the Pleistocene of Central Europe have established a stratigraphy which is generally applicable over a wide area. Most of this work comes from Germany. From these various observations it would appear that the North German area, the Alpine area and the periglacial area, *i.e.* Middle and South Germany, can be correlated in a system in which, using the Alpine terminology in a generalised sense and applying it to an extended area which includes from the Caucasus and North Russia to the Rhine at least, each of the four glaciations of the Alpine, Günz, Mindel, Riss and Würm, consisted of two cold phases, with the exception of the last named, the Würm, which had three, while for the Alps and a few river systems, Günz was preceded by several cold phases. Two cool phases of lesser intensity were intercalated between Mindel 2 and Riss 1.

Certain difficulties and obscurities which arise out of this correlation have been elucidated or resolved by the study of solar radiation. For the archæologist and the palæontologist the most significant result is that by the application of the radiation curve worked out by Milankovith it is possible to determine the divisions of the Pleistocene with certainty in terms of years, by their coincidence with minima of radiation occurring at an ascertained number of years previous to A.D. 1800. Hence it becomes possible to date in thousands of years horizons, fossils, and implements in various localities provided their exact stratigraphical position is known and determined in relation to the evidence of geology.

Applying this system to the various fossil remains of man and to the cultures from the areas to which it refers, Dr. Zeuner works out some interesting results. Thus he dates *Homo heidelbergensis*, the most ancient human fossil of Central Europe, which, it is said, cannot be later than the interglacial between the second Günz and the first Mindel, at about 500,000 years—this as all other dates to be read as before A.D. 1800. On the basis that a pre-Chellean culture is to be assigned to Heidelberg Man, the Chellean is dated at 430,000–226,000 years, the Acheulean at 183,000, the Mousterian at 140,000–105,000, Aurignacian 95,000–69,000, Solutrean 67,000, Magdalenian 65,000–18,000, the Mesolithic following after Würm 3 at 15,000–7,500 and the Neolithic of the Atlantic phase of the

post-glacial at 7,500–4,000. These figures, it is to be noted, apply only to the area under immediate consideration, and may need modification for western Europe and Britain.

The significance of the new stratigraphy and chronology for palæontology are discussed especially in relation to the succession and evolution of types and the disappearance of extinct forms, but must be passed over here.

ARCHÆOLOGY AND CHRONOLOGY IN AMERICA.—As a further, though in this instance indirect, outcome of the study of solar radiation, precision has been given to the dating of the ruins of the Pueblo Indians in the south-western United States. This has been accomplished through the tree ring system of chronology worked out by Professor A. E. Douglass. By observing the effect of climatic variation on the growth of the annual rings in trees, Professor Douglass was able to identify certain identical sequences of growth in trees which had been subjected to identical climatic conditions, and by the overlap between trees of different ages, subjected to these conditions, he was able to trace back an annual succession from trees of known age to timbers in historic buildings and from these to the timbers in prehistoric structures. Professor Douglass now has a complete series of tree ring and timber sections for each year from A.D. 1929 back to A.D. 698.

The material with which Professor Douglass first worked was drawn from various archæological sites in the south-west. Accounts of the method and some of the results achieved by its assistance have appeared from time to time since investigations first began at the invitation of Dr. Clark Wissler in 1918 ; but there is now available for the first time the results of an extended series of investigations on a single site, supplemented with comparative results from other adjacent sources in a report by Dr. Douglass on the material from the Pueblo Bonito ruin in the Chaco Canyon, New Mexico. (*National Geographical Society : Contributed Technical Papers. Pueblo Bonito Series No.* 1, *Dating Pueblo Bonito and other Ruins of the South-West. By A. E. Douglass. Washington D.C. 1935.*)

The exploration of Pueblo Bonito was undertaken by the National Geographical Society as the results of an archæological reconnaissance of the Chaco Canyon made by Dr. Niel M. Judd in 1920. This country is extremely rich in archæological remains, and of these Pueblo Bonito is the largest and the most important, the ruined structure containing more than a thousand rooms of stone and adobe. The exploration was carried out by annual expeditions under Dr. Judd from 1921 to 1927 ; and in 1922 the

co-operation of Dr. Douglass was invited. His first beam expedition visited the ruins in 1923 and second and third beam expeditions for the purpose of further examination of the timbers followed in 1928 and 1929. The results have proved to have a far wider bearing than on the chronological issue alone.

So far as the ruin of Pueblo Bonito itself is concerned, the tree ring chronology indicates that construction began in A.D. 919, but that the major constructional work was carried out in the decade 1060–70, with a period of lesser activity twenty years later. Comparing the results from other sites, of which over forty were examined in this area, the important site of Aztec is dated at 1110. Notwithstanding its great size, it was completed by 1121. The dates of construction on other sites range from A.D. 919 until 1417, except in two instances, dated at 1550 and 1770 respectively.

Even more interesting is it to find how the method gives precision to the periods into which archæologists have divided the Pueblo culture, and how they confirm the succession assigned to cultural features by archæological reasoning. Thus, for example, the cream-coloured Hopi ware begins in the early part of the fourteenth century. It is preceded by the polychrome ware of the thirteenth century ; red pottery was common in the twelfth century, and black on white was characteristic of the tenth and eleventh centuries. Of the cultural periods of Pueblo civilisation Pueblo III, the Golden Age, took its early form in Chaco Canyon about 919, reached its climax in the late eleventh century, and probably closed with the great drought which lasted from 1276 to 1299. Pueblo IV began in the early thirteen hundreds.

In view of the remarkable efflorescence of Pueblo culture and its no less remarkable decline even before the Spaniard had reached America, it is interesting to note that Professor Douglass is able to point to certain very marked periods of aridity which may be closely connected with the fluctuation in the density of population and the flourishing state of their culture, or rather with its decline. The grouping of sequences of rings of poor growth is interpreted as indicating a succession of droughts. These occur with extreme intensity at intervals of about 300 years. The most severe was in the closing years of the thirteenth century, from 1276 to 1299 ; another appears from 1573 to 1593 ; and still another in 1890, with a period of maximum intensity from 1896 until 1904. Earlier periods of drought can be traced as far back as A.D. 904.

ARCHÆOLOGY IN IRELAND.—It is possible to make a brief reference only to the results of the continuation of the scheme for the relief of unemployment in the Irish Free State, which again in

1935 made provision for excavation on a number of archæological sites.   A preliminary report by Mr. S. P. O'Riordan of the National Museum of Ireland appears in *Discovery* for April.   Among the results he records, perhaps the most important is the confirmation of the indication afforded by the excavations at Cush, Co. Limerick, in the previous year that ring forts with souterrains, a characteristic feature of Irish archæology, could be dated back to Late Bronze Age.   It now appears that the fort containing the cremation burials upon which this conclusion was based, was not the earliest of the series, but had been built later than that adjoining it.   Another interesting discovery was that of new house sites, some of wattle and daub, which have furnished the plan of a distinctive Irish house type.

NOTES

Some Recent Safety in Mines Research Board Publications [1]
(J. W. W.)

In these six papers we have further evidence of the excellent work done by the Safety in Mines Research Staff, under the direction of Dr. R. V. Wheeler, at Sheffield and Buxton. The publications are clear in type, and well illustrated ; the prices are low, yet the paper and covers leave little to be desired as regards durability and good quality. Indeed these research papers in their general style of production may well be taken as a model for work of this kind. If there must be one point for criticism, it is that insufficient use is made of heavy distinctive type for important observations and conclusions.

PAPER 88.—There is no doubt that in the past ignitions of firedamp-air mixtures in the mine have occurred through the use of explosives for blasting. Explosives, although capable of causing coal-dust explosions in the absence of methane, are particularly dangerous in " gassy " mines, where, as is well known, lamps have to be doubly guarded, whether they be flame lamps or electric. Explosives are liable to cause mine-explosions not only through the flames produced on firing and through the shock and pressure waves sent out, but also because of hot solid particles projected in a chemically active state. Payman and Woodhead give in this paper an account of their investigations (by means of Schlieren photography) into the differences between the behaviour of an explosive charge sufficient to ignite a firedamp-air mixture and the behaviour of an insufficient charge.

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[1] Paper 88.   The Pressure Wave sent out by an Explosive.   1s.
    ,,   89.   The Ignition of Fire-Damp by Broken Electric Lamp Bulbs. 1s.
    ,,   90.   The Ignition of Firedamp by Coal Mining Explosives.   9d.
    ,,   91.   A Recording Manometer having Low Inertia.   1s.
    ,,   92.   The Deterioration of Haulage Ropes in Service.   1s.
    ,,   94.   The Deterioration of Colliery Winding Ropes in Service.   2s.
Published by H.M. Stationery Office.

Permitted explosives, both of the ammonium nitrate and nitro-glycerine classes, were used, but the research as indicated by the title was confined largely to the registering of the differences in the pressure waves sent out.

Noticeable differences in the waves from the two types of charge —igniting and non-igniting—were obtained, but the most striking features of the records were certain disturbances, secondary to the main shock wave, and observable whether the charge was heavy or light.

The suggestions put forward to explain these disturbances are obviously tentative. Further investigations are clearly called for. The paper is illustrated with several excellent photographs.

PAPER 89.—Allsop and Wheeler record here the results of a study of the appearances of burnt-out electric lamp filaments. The object of the investigation was to see if there were any characteristic features when the filament had burnt out in a firedamp-air mixture, and if possible to establish whether an electric lamp filament had caused an explosion.

The report describes the appearance of tungsten filaments of 2-volt vacuum and 4-volt gas-filled bulbs, which have been broken when cold, or have burnt out under different conditions. The colour of the filament is apparently a useful guide. If a filament is destroyed by a blow when cold or by normal decay or by decay due to over-running, the colour is a bright and steely grey. If the filament is burnt out in air or in a firedamp-air mixture (which it has ignited), there is a certain amount of a greenish-yellow deposit of oxide of tungsten on the supports; and the authors conclude that it may sometimes be possible to judge from a broken lamp bulb whether or not the filament has burnt out in an explosive mixture. Among the illustrations there is a reproduction of a filament of a 2-volt vacuum bulb that has ignited a firedamp-air mixture without being burnt out.

PAPER 90.—*Paper* 90 by Naylor, Payman and Wheeler forms the sequel to *Paper* 69 which dealt with the dangers from the use of explosives caused by the position of the shot hole, the weight of the charge, the position of the detonator, and the amount of the stemming. *Paper* 90 deals with another safety aspect, namely the results of using a sheath for the explosive, the sheath containing some such quenching material as sodium bicarbonate. The research has in fact shown the eminent suitability of this material for the purpose. Explosives so sheathed have been proved to be equally effective in the mine in performing their work, and (what is all important) when stemmed in the usual way such explosives

have been proved incapable of igniting firedamp through a blown-out shot or an overcharged shot. This marks a definite step forward in the safe use of mine explosives.

PAPER 91 by Allsop and Lloyd is a useful report on the construction and operation of a recording manometer of low inertia. In the earlier work (*Paper* 16) use was made of the thermionic valve, in association with suitable oscillatory circuits, to show the displacement of a small diaphragm, subjected to the pressure to be measured and acting as a plate of a condenser. The circuit for the earlier method has now been abandoned. In its place two new types have been evolved and are here described with diagrams of the circuits and photographs of the assembled apparatus. The new manometers have been in everyday use for three years, and have been entirely satisfactory. The new circuits are said to be electrically stable and the sensitivity easily adjustable over a wide range. The sphere of action of a manometer of this type is clearly not restricted to work on explosions.

PAPERS 92 AND 94 deal with the important question of the deterioration of wire ropes in service. In *Paper* 92 Dixon and Hogan give the results of the examination of 23 haulage ropes, used for hauling men (not winding). Wear, corrosion and fatigue are said to be the three main factors (often acting conjointly) responsible for deterioration. In nine of the failures wear was the predominant factor ; in ten cases corrosion ; and in the remaining four, capel failures. Serious accidents were caused by the breakage of 7 of the 23 ropes.

The field covered in the paper is a large one. The authors have dealt with the strengths of ropes, " factors of safety," types of ropes, rope-construction, size of the wires, lubrication in manufacture, installation, storing, lubrication in use, periodical examination, re-capping and capels. In describing the actual failures, they have given individual reports on all the 23 cases, the notes being supplemented with excellent photographic illustrations. The circumstances attending the failures in each case are considered, and recommendations are given for the procedure necessary to prevent the recurrence of similar failures. The paper (price 1s.) should be in the hands of all colliery engineers. It will be of no little assistance to them in the proper selection, installation, maintenance and capping of underground haulage ropes.

A Preliminary report on *The Deterioration of Colliery Winding Ropes in Service* was published as *Paper* 50 in 1928, but is now out of print. *Paper* 94 incorporates the work described in *Paper* 50, and also includes valuable new matter.

The main cause of winding-rope breakages is stated here (by Dixon, Hogan and Robertson) to be corrosion-fatigue, a term used to describe " the phenomenon exhibited by materials subjected to the simultaneous action of corrosion and repeated stressing." This kind of deterioration may proceed with very few obvious signs of corrosion. Indeed, the amount of corrosion may be " insufficient to dull the appearance of the wires," and may be only " a slight pitting or roughening of the surface," visible only under a magnifying glass. On the other hand the corrosion effects may be quite marked with other specimens.

Before dealing with the specific cases of rope deterioration, the authors discuss the production and properties of the wire used in winding ropes, the manufacture of ropes, and the effects of different factors in the winding conditions on the endurance of ropes. Stress is laid on the question of lubrication during manufacture and in use, and the advantages of galvanising the wires are set forth so strongly as to leave no doubt concerning the favourable opinion of the authors on this treatment.

Coming to the actual ropes examined, we find a record of some 250 tests, with a classification of the types of deterioration found. The importance of regular examinations of the winding rope in service is emphasised, and sound recommendations are made for more frequent recapping.

The report requires a summary. There is such a wealth of detail in its 108 pages that the material is apt to become a source of mental exhaustion. Certain it is that most colliery engineers will not feel inclined to work their way through all the descriptions and records. The use of heavy distinctive type for important conclusions and recommendations would have been particularly helpful in this publication.

Tin (E. W. Y.)

The International Tin Research and Development Council, composed of delegates appointed by the Governments of the principal tin producing countries, has been established for the purpose of acquiring and disseminating scientific and technical knowledge relating to tin, its alloys and chemical compounds, the processes involved in the production of these materials, and their applications. The objects of the researches and other activities of the council are to discover and develop new industrial applications of tin, to improve the existing products and processes, and to assist tin consumers in overcoming technical difficulties and problems relating to tin.

The *Miscellaneous Publications* issued by the council are of interest to general readers and bear titles such as " Tin and its uses," " Tin and civilisation," which indicate their aim. They serve the useful purpose of directing attention to the present and possible future uses of the metal. The *Technical Papers* have either been directly published by the Council or are reprints of work done by their investigators which has been previously published in the transactions of various scientific societies.

It is not possible in this short notice to do more than indicate the scope of these papers. They deal with such subjects as the atmospheric corrosion of tin and tin alloys, the mechanical properties of tin-base alloys and the electrodeposition of tin and tin alloys from alkaline stannate baths and with the employment of bronze and bi-metallic anodes. There is also a paper describing the improvement of the quality of tinplate by superimposed electrodeposition of tin.

It is shown that pure tin proves slightly more resistant to atmospheric attack than tin-antimony and tin-copper alloys. Comparisons were made with brass, nickel, zinc and cadmium and with two exceptions the tin and tin alloys were corroded less than the comparison materials, copper proving slightly superior under Stevenson screen conditions and also as regards mechanical properties when exposed outdoors.

Satisfactory deposits of tin-nickel alloys containing up to about 25 per cent. nickel were obtained with an alkaline stannate bath containing in addition potassium nickelocyanide and potassium cyanide, nickel anodes being employed. Alloys containing tin and antimony in almost any proportions can be electrodeposited from an alkaline stannate-thioantimonate bath at 70°–75° C. by suitable choice of current density. Bronze can be electrodeposited, at a high rate of deposition, up to 0·8 mm. thickness or greater from alkaline solutions of sodium cuprocyanide and sodium stannate, the metal concentration of the solution being satisfactorily maintained by the use of annealed cast bronze anodes having a composition approximating to that of the bronze deposit. The deposits were found to be satisfactory as undercoats for chromium deposits.

There seem to be fundamental drawbacks to the use of oxalate baths when bi-metallic anodes of copper and tin are employed; the alkaline stannate-cuprocyanide bath appears, however, to show promise of being made suitable for industrial use.

It was found that Macnaughtan's process of electrodeposition of tin on tinplate reduced the porosity of tin coatings on tinplate

when small thicknesses were electrodeposited and eliminated them in the case of larger thicknesses.

The pamphlets issued by the Council are valuable records of work which has been done in connection with tin and should be closely studied by all interested in the subject. A full list of publications can be obtained on application to the Council. In their present form the pamphlets are of great value; it would, however, be an added advantage if their size were standardised.

Research in Australia

The *Annual Report* of the Council for Scientific and Industrial Research of the Commonwealth of Australia for the year ending June 30, 1935, contains a remarkable record of achievement and promise—the latter, indeed, somewhat restrained by the fact that the Government has of late found it inconvenient to provide the Council with a definite income for a definite period of years instead of a fluctuating annual grant.

The investigations sponsored by the Council are with few exceptions designed to increase the prosperity of agriculture in the Commonwealth. They are classified as being concerned with plants, entomology, animal health and nutrition, the soil, irrigation, forest products, cold storage and such miscellaneous items as radio research, fuel and fisheries.

Peach moth (*Cydia molesta*) is causing serious loss to growers— about £70,000 in 1933-4—and no entirely satisfactory method of destroying the larvæ has yet been found. Silverfish (*Ctenolepisma longicaudata*) have become a pest in houses and offices in Canberra ; but here a bait composed of a mixture of flour paste, sugar and barium fluosilicate spread on cords has given good results. Progress has been made in dealing with tick paralysis—a serum from immunised dogs providing a satisfactory curative agent. A vaccine against pleuro-pneumonia in cattle has been prepared and a method of diagnosis for carriers developed, so that it is hoped within a few years to eradicate the disease from Australian herds. An extensive study of the feed required to produce most wool from sheep has been started.

The export of chilled beef to Great Britain has passed into the commercial stage and from Queensland alone over 8000 tons were exported in the year under review. A few shipments arrived in poor condition and, when data concerning these were available, the failure could be traced to unsuitable preparation at the meat-works. Conditions necessary for the successful export of oranges were investigated ; in particular the condition of the fruit after

it had been out of cool store for a week and exposed to normal atmospheric conditions was examined—a notable recognition of the fact that it is the condition of the food when it reaches the consumers' table which really matters.

The *Cactoblastis* attack on the prickly pear has been remarkably successful, wide areas have been cleared and steps are now being taken to deal with similar pests of lesser importance. It does not appear, however, that any adequate method of dealing with the rabbit has yet been discovered. It is hoped that the virus disease known as myxomatosis, now being investigated in the Department of Pathology at Cambridge, will show promise as a method of destruction, and if so it will probably be tested on a large scale on some convenient island.

Parts of the Commonwealth are suffering from the effects of soil drift due in particular to destruction of pasture by rabbits, overstocking and drought. In recent years the trouble has become serious ; in South Australia, for example, hundreds of square miles, which formerly carried sheep and cattle, have had to be abandoned and are becoming a desert. The Australian Council of Agriculture has approved a proposal that the matter be investigated by the Council for Scientific and Industrial Research and arrangements for a survey of the problem have been made.

These and very many other matters are discussed in the *Report* and it is remarkable that the expenditure on the whole of the work in the year 1934–5 was only just over £90,000.

Mining Royalties and Rents

The amount and method of assessment of royalties are regarded as matters of considerable importance in the development and prosperity of a mining industry. The extent of these royalty charges may be illustrated by the fact that coal in Great Britain pays nearly £5,000,000 per annum ; in the Federated Malay States, tin pays nearly £1,000,000, whereas in the Union of South Africa the profits tax, excess profits duty, etc., on the gold industry produced over £13,000,000 per annum in 1934–5.

In view of the recent announcement of the British Government's intentions in regard to the unification of coal-mining royalties, it is particularly opportune that the Mineral Resources Department of the Imperial Institute should now issue an up-to-date and authoritative work on *Mining Royalties and Rents in the British Empire* (The Imperial Institute, London, S.W.7 ; 183 pp., 1936, 3s. 6d.), which has been compiled with the co-operation of over seventy members of overseas mining departments and some

hundreds of companies working mineral deposits in the United Kingdom.

Following an introduction dealing briefly with royalty systems in general, the main subject matter is divided into two parts. In Part 1 the statement for each country opens with a summary as regards the ownership and right to work minerals, and in this connection attention may be drawn to the useful statement regarding the rather complex conditions prevailing in the Union of South Africa. Next follow details of royalties and export duties leviable on minerals and metals, and the period, terms of renewal and "dead" or sub-surface rents of mining leases. In Part 2 the royalties and export duties detailed in Part 1 have been summarised for reference purposes, under eight groups, *i.e.* coal; petroleum and natural gas; bituminous and oil shales; ores of iron and other ferro-alloy metals; base metals; precious metals; precious stones; and miscellaneous minerals.

A glance through the document shows that royalties levied on certain minerals (*e.g.* coal) are roughly uniform in many parts of the Empire, but in the case of other minerals (*e.g.* gold) wide divergence occurs, both as regards the amount and basis of the charge. Coal in the United Kingdom pays an average royalty of about $5\frac{1}{2}d.$ per long ton, which is not widely different from the rates for Southern Rhodesia ($6\frac{3}{4}d.$), New South Wales ($6d.$), Nova Scotia ($6d.$), New Brunswick ($5d.$), Western Australia ($3d.$ to $6d.$), Federated Malay States ($7d.$), British Columbia ($5\frac{1}{2}d.$). In Victoria, Australia, the rate varies with the thickness of the seam worked, rising from $\frac{1}{4}d.$ per ton won from a 2 feet seam up to $6d.$ per ton from a seam of 3 feet or over. In Queensland, the distance of the deposit from a seaport or point of delivery affects the rate levied. South Africa and British India do not levy tonnage royalties, the former having a profits tax of $2s.$ $6d.$ in the pound, and the latter taking 5 per cent. *ad val.* with minimum of $2\frac{1}{4}d.$ per ton.

Gold affords an example of rather widely varying rates and systems. Gold and silver in Great Britain being "royal metals" belong to the Crown and the royalty levied is usually 2 to 4 per cent. of the value won. No royalty is charged on gold won from gold-mining leases in Queensland, Western Australia, or in New Zealand, but the last-named country levies an export duty. An *ad val.* tax is favoured by many countries; the Anglo-Egyptian Sudan levies 2 per cent., while 5 per cent. is specified in Kenya, Tanganyika, Uganda, North Borneo, New Guinea, and Fiji. Southern Rhodesia graduates the tax between $2\frac{1}{2}$ and 5 per cent. according to the recovery of gold per ton of ore. Nigeria charges

6 per cent. ; Sierra Leone, 9 per cent. ; and in South-West Africa and Sarawak the rate is 10 per cent. *ad val.* On the other hand, a tax on profits is adopted by certain countries ; thus the Union of South Africa takes 4*s.* in the pound ; most provinces in Canada, on gold obtained from reef workings, levy 3 per cent. upwards on profits exceeding $10,000 per annum. British India takes either $7\frac{1}{2}$ per cent. of the profits or $2\frac{1}{2}$ per cent. of the gross value. In Mysore, 5 per cent. *ad val.* is levied in those years when a profit is made, and Hyderabad Deccan reduces the standard royalty (5 per cent.) to one-half if the mining is carried on at a loss. The Union of South Africa and certain other countries levy a gold-premium tax in addition to the ordinary tax on production.

Miscellanea

The following is a list of the names of those elected this year to be Fellows of the Royal Society : Dr. A. C. Aitken, lecturer in statistics, University of Edinburgh ; Dr. J. D. Cockcroft, demonstrator in physics, University of Cambridge ; Prof. H. J. Fleure, professor of geography, University of Manchester ; Mr. C. Forster-Cooper, reader in zoology, University of Cambridge ; Sir Alexander Gibb, consulting engineer ; Mr. H. L. Guy, chief engineer, Mechanical Engineering Department, Metropolitan Vickers, Ltd. ; Prof. H. G. A. Hickling, professor of geology, Armstrong College ; Prof. L. Hogben, professor of social biology, London School of Economics ; Dr. J. Kenyon, head of the Chemistry Department, Battersea Polytechnic ; Prof. E. H. Kettle, director of the Department of Pathology, British Postgraduate Medical School ; Prof. N. F. Mott, professor of theoretical physics, University of Bristol ; Dr. R. G. W. Norrish, lecturer in physical chemistry, University of Cambridge ; Prof. H. H. Plaskett, Savilian professor of astronomy, University of Oxford ; Mr. E. F. Relf, superintendent, Aerodynamics Department, National Physical Laboratory ; Dr. F. J. W. Roughton, lecturer in physiology, University of Cambridge ; Prof. Birbal Sahni, professor of botany, University of Lucknow ; Prof. E. B. Verney, reader in pharmacology, University of Cambridge.

The Keith prize of the Royal Society of Edinburgh has been awarded to Prof. L. Hogben for his work in genetics.

The Linnean gold medal for 1936 has been awarded to Prof. J. S. Gardiner, professor of zoology and comparative anatomy in the University of Cambridge.

The Society of Chemical Industry has awarded the Messel medal to Sir Robert Mond.

Prof. A. C. Seward has been appointed a member of the Advisory

Council to the Committee of the Privy Council for Scientific and Industrial Research. He has announced his intention of resigning his chair at Cambridge at the end of the current session.

Dr. W. E. Harper, assistant director of the Dominion Astrophysical Observatory, Victoria, B.C., has been appointed to succeed Dr. J. S. Plaskett as director.

We have noted with regret announcements of the death of the following well-known scientific workers during the past quarter : Prof. F. B. Allan of the University of Toronto, organic chemist ; Mr. Oskar Barnack, inventor of the Leica camera ; Mr. H. Brown, of the Plant and Animal Products Department of the Imperial Institute ; Prof. A. C. Dixon, F.R.S., of the Queen's University, Belfast, mathematician ; Sir Archibald Garrod, F.R.S., formerly regius professor of medicine in the University of Oxford ; Prof. J. S. Haldane, F.R.S., physiologist ; Prof. F. B. Jevons of the University of Durham, philosopher ; Prof. P. F. Kendall, F.R.S., of the University of Leeds, geologist ; Dr. K. Lambrecht of Budapest, palæontologist ; Mr. A. Mason of the Yorkshire Naturalists Union ; Prof. C. Lloyd Morgan, F.R.S., of the University of Bristol,- psychologist ; Prof. J. M. Page of the University of Virginia, mathematician ; Prof. I. P. Pavlov, For.Mem.R.S., physiologist ; Prof. Karl Pearson ; Sir Joseph Petavel, director of the National Physical Laboratory ; Dr. G. T. Prior, F.R.S., of the British Museum, mineralogist ; Mr. J. Rice, associate professor of physics in the University of Liverpool ; Mr. J. H. Scarr of the U.S. Weather Bureau ; Mr. J. Whitehead, K.C. ; Mrs. Tessa V. Wheeler, archæologist.

The following list contains the names of presidents of scientific societies elected during the past quarter : Royal Astronomical Society—Mr. J. H. Reynolds ; Geological Society—Prof. O. T. Jones ; Institute of Metals—Mr. W. R. Barclay ; Ray Society— Sir Sidney Harmer ; Quekett Microscopical Club—Mr. C. D. Soar.

Pending the appointment of a successor to the late Sir Joseph Petavel, the office of Director of the National Physical Laboratory is being held by Sir Frank Smith, K.C.B., the Secretary of the Department of Scientific and Industrial Research.

Nature (March 28) contains an interesting article by Finch and Quarrell concerning the development of ideas of the nature of a polished metallic surface. In the *Micrographia* Hooke suggested

that the act of polishing covered a surface with very fine scratches and this view was generally accepted until, in 1901, Rayleigh pointed out that polishing powder will function only when it is pressed against the metal surface with a yielding support (such as pitch). During the years 1902–20 Beilby investigated the problem with the microscope and concluded that polish is due to the formation of an amorphorus layer over the surface. In 1930 G. P. Thomson applied the method of electron diffraction to investigate the nature of the surface layer, but neither he nor other investigators using this method were able to reach an agreed decision. In 1934, however, Finch and his collaborators discovered that thin crystalline films of foreign metals deposited on the metal surface are dissolved by it if it is polished but not if it is etched. In this respect therefore the polished surface behaves like a liquid metal and the surface layer is certainly amorphorus. Electron diffraction examination of the surfaces of engine cylinder liners before and after they have been " run in " has shown that a substantial Beilby layer is formed by the running in process.

Dr. F. W. Aston has published (*Nature*, **137**, 357, 1936) the following determinations of atomic masses of elements of small atomic number (relative to $^{16}O = 16$): $H = 1.00812 \pm 0.00004$; $D = 2.01471 \pm 0.00007$; $He = 4.00391 \pm 0.00016$; $C = 12.0035 \pm 0.0003$, which confirm the values he previously gave (*Nature*, **135**, 541, 1935). The values for hydrogen and helium are somewhat higher than the yet earlier values (*Proc. Roy. Soc.*, **115 A**, 487, 1927). M. L. Oliphant, who had believed the 1927 values to be too low, gives an extended list of atomic masses up to ^{19}F (*Nature*, **137**, 397, 1936). The value that he gives for the neutron ($n = 1.0091$) differs from estimates obtained by G. Ising and M. Helde (*Nature*, **137**, 273, 1936), namely 1.0084 and 1.0080. It is strange if the original ^1H value is in error, for that was the value which enabled R. T. Birge and D. H. Menzel to predict the existence of the hydrogen isotope ^2H (*Phys. Rev.*, **37**, 1669, 1931).

The April number of *Sands, Clays and Minerals* (A. L. Curtis, Westmoor Laboratory, Chatteris. 3s. 6d.) contains a most varied selection of interesting articles. P. Evans writes on methods of drilling in oilfields with special reference to rotary drilling fluids. C. W. Boden (Johnson Matthey & Co., Ltd.) describes the methods used for handling radium of which some 8–9 gm. are now being obtained each year from the deposits on the shores of the Great Bear Lake in Canada, deposits reported as being the richest in

the world. Dr. Hampton (Chance Brothers & Co., Ltd.) deals with the efficiency of heat absorbing glasses such as Chance's *Calorex*, a ferrous iron glass with a maximum transmission (70 per cent.) at 5000 A, a wide absorption band centering at 10,000 A and a second maximum at 18,000 A, which transmits directly only 20 per cent. of the solar energy incident on it. M. L. Van Moppes describes the properties and uses of industrial diamonds—*Carbons* (black Brazilian diamonds) used, *e.g.* for diamond drilling crowns, *Ballas* (often of spherical shape with a structure radiating from a central point) used for mineral drilling and truing emery wheels, *Melee* (small diamonds of less than $\frac{1}{4}$ carat) used, *e.g.* for glass cutting and *Boart* used for diamond powder. There is also a useful account of minerals used for colouring glass and enamels written by H. L. Laithwaite ; the last section of a reprint of Klaproth's pamphlet (1786) on the *Mineralogical and Chemical History of the Fossils of Cornwall*, with several other articles, details of new patents and book reviews together forming an admirable number of a technical journal of very general interest.

The Board of Trade has issued a pamphlet describing comparisons of the parliamentary copies of the imperial standards with the imperial standard yard and the imperial standard pound. (H.M. Stationery Office, 1s. net.) The report, written by Mr. J. E. Sears, Jnr., Superintendent of the Metrology Department of the N.P.L., describes in detail the methods adopted for the comparisons and the results obtained. These show in particular, that the British Standard Pounds are losing weight by comparison with the Kilogramme (50 parts in 10^8 between 1846 and 1883 and a further 19 parts in 10^8 between 1883 and 1933) and that the Kilogramme Standards themselves may be slightly unstable so that the construction of a new series of Pound weights is highly desirable. Further, the length of the yard, as determined by the present standard, cannot be regarded as definitely established to a precision better than about 1 part in 10^6 so that a new standard of length is required also.

The whole of the January number of the *Journal of Research* of the U.S. National Bureau of Standards is devoted to an account of an absolute determination of the ohm by Curtis, Moon and Sparks. Carefully constructed helical inductors of calculated inductance were measured in terms of capacitance and resistance with a Maxwell-Wien A–C bridge ; the capacitance was measured in terms of resistance and time with a Maxwell capacitance bridge

and the resistance compared with the National Bureau of Standards International Ohm with a Wheatstone bridge. Thus in the end the value of the International Ohm is found in terms of the mean solar second and the " absolute " henry. Needless to say the most elaborate precautions were taken and it is considered that the final result (1 N.B.S. International Ohm = 1·000450 " absolute " ohms) is correct to within 20 parts in a million. It is stated that the preliminary results from similar measurements now in progress at the National Physical Laboratory and the Electrotechnical Laboratory of Japan give 1 N.P.L. International Ohm = 1·00047 and 1·00049 absolute ohms (by different methods), while 1 E.T.L. International Ohm = 1·00046 absolute ohms. Direct comparisons of the three International Ohms indicate that they agree to within 3 parts in a million and the final values from the N.P.L. and Japan will therefore be of very great interest.

We would draw the attention of our readers to two papers on magnetism which have appeared recently in the *Bell System Technical Journal*. In January last Dr. R. M. Bozorth gave an account of " The Present Status of Ferromagnetism " in a slightly revised edition of a paper which appeared in the November 1935 issue of *Electrical Engineering*. In April Dr. K. K. Darrow contributed a more general account of " The Theory of Magnetism." Anyone desirous of understanding something of the present position in this obscure field could hardly do better than to read these two articles.

We regret that in our notice of the newly formed Society for the Study of Alchemy and Early Chemistry we described Prof. J. R. Partington as president of the Society. He has written to inform us that the Society has not yet elected its first president. Prof. Partington is acting as chairman during the period of organisation.

ESSAY REVIEWS

THE NEW COSMOLOGY. By G. TEMPLE, Ph.D., D.Sc., Professor
of Mathematics in the University of London, King's College. Being
a Review of **Relativity, Gravitation and World Structure,** by
E. A. MILNE, M.A., D.Sc., F.R.S. [Pp. x + 365, with 4 plates and 21
figures.] (Oxford: at the Clarendon Press; London: Humphrey Milford,
1935. 25s net.)

THE publication of Professor Milne's investigations into relativistic
cosmology marks an epoch in the development of this subject.
The problem envisaged is nothing less than the large-scale dis-
tribution of matter and motion in the Universe regarded as a whole.
The importance of Milne's researches is that he has completely
emancipated himself from the restrictions of view imposed by Ein-
stein's General Theory of Relativity and has propounded a new,
simple and powerful method of obtaining a theoretical solution of
this problem. It is also argued by the author that from the stand-
point of observational astronomy the new theoretical cosmology
which he has thus developed is to be preferred to those cosmologies
dictated by General Relativity and commonly described as theories
of the Expanding Universe.

It is well known that Milne's work has excited considerable
controversy and that opinion is sharply divided as to the physical
significance of the results which he has obtained. Certainly these
results are sufficiently surprising. Within the framework of the
Special Theory of Relativity the author paints a mathematical
picture of the Universe which represents in a direct and unforced
manner all the main features of the extra-galactic nebular system :—
the concentration of matter into nebular nuclei, the recession of
nebulæ with velocities proportional to distance from the observer,
the K-effect, the existence of cosmic dust-clouds, and the existence
of isotropic penetrating radiation ! Such a programme is stupendous
both in its range and detail, but perhaps the most astonishing feature
of Milne's cosmology is the entire absence of any theory of gravita-
tion or of interaction between material particles.

How is it possible to deduce so much whilst assuming so little ?

The answer is that, in reality, nothing whatever is deduced about the actual world. The deduction refers entirely to a mathematical model, which may or may not faithfully represent the actual world, but which is certainly the simplest and most fundamental standard of comparison for the actual world. Milne's method is a transformation of the methods of Relativity Theory, and may be crudely summarised by saying that it determines what the structure of the world would be if it appeared the same to all of a certain class of observers in uniform relative motion. There is no convincing reason to suppose that the actual structure of the world does satisfy this cosmological principle, but, clearly, in the absence of any evidence to the contrary, such an assumption is the most natural and obvious postulate of any cosmological theory. The results obtained have the same kind of validity as the results obtained by dimensional analysis. They are to a certain extent independent of the particular physical laws (*e.g.* the law of gravitation) controlling the phenomena in question, for any laws which conform to the cosmological principle must lead to the conclusions actually reached by a direct application of the principle. (This may partly answer the question raised by the author in § 456.)

The preceding paragraphs summarise the problem attacked, the results obtained and the method employed. It remains to consider in detail the author's achievement. The two main questions are those of relativistic kinematics and of relativistic cosmogony. In both cases the author offers a number of criticisms of the accepted, traditional solution and advances new and simple solutions of his own. Throughout, the author's attitude to these problems is dominated by his views as to the relations between geometry and dynamics. We proceed, therefore, to give an account of his treatment of these problems.

Milne's attitude towards the geometrisation of physics is that of Poincaré—he holds that the geometry of space-time can be chosen arbitrarily and that the laws of dynamics can be validly stated in whatever geometry is assumed. In actual fact he does not require to use such a sweeping assumption : his working hypothesis is that the geometry of space-time can be taken to be that of the Special Theory of Relativity and that the laws of astronomical dynamics can be validly expressed in this setting. This view agrees with that held by Whitehead, who distinguishes sharply between the metrical manifold (whose properties he holds are given intuitively) and the dynamical manifold (whose properties are inferred from observation and experiment). It is a view sharply contrasted with that of the General Theory of Relativity which identifies the metrical and

dynamical manifolds on *a priori* philosophical grounds apparently derivable from Kant.

Milne's criticism of Einsteinian relativity is . firstly, that it is not expressible in terms of observable quantities but is mathematically developed from a purely conceptual basis ; secondly, that the theory cannot distinguish between those transformations of co-ordinates which correspond to a change from one type of co-ordinate to another, the observer being the same (tautomerisms), and those which correspond to changes from one observer to another, the type of co-ordinate system being the same ; and, thirdly, that it is incompetent to express the results of observation in unambiguous mathematical form—at least, I take it that this is the significance of the oft-repeated statement that the stress tensor $T_{\mu\nu}$ depends not only on the matter in motion (supposed known from observation) but also on the potentials $g_{\mu\nu}$ (only known as solutions of the as yet unsolved field equations). The first and second criticisms are unanswerable, but I suggest that the third criticism may be no longer valid when explicit account is taken of the atomicity of matter, *i.e.* of the fact that the potentials $g_{\mu\nu}$ present only discrete singularities along the geodesics of the material particles. However, this is certainly a question which should be investigated.

The standpoint of the author is, therefore, that the geometrical structure of space-time and the distribution of matter in space-time are independent questions, and it is essential that this view should be sympathetically accepted by the reader. The physicist will perhaps be convinced by the pragmatic argument that Milne's cosmology is immensely simpler in mathematical form than Einstein's, while it is much more fruitful in yielding physically significant results.

The first main question is that of relativistic geometry or kinematics, and here the author proposes a new method of treatment. The traditional arguments for the Special Theory of Relativity require the use of rigid measuring rods and accurate clocks, but Milne's argument is based entirely on the use of observations of clocks which are not initially accurate or synchronised. The author shows how any observer O can assign spatial and temporal co-ordinates to any distant event E in terms of observations made on his own clock only. For example, the distance of E is taken to be $\frac{1}{2}c(t_2 - t_1)$ and the epoch of E is taken to be $\frac{1}{2}(t_2 + t_1)$, where t_1 and t_2 are the times at which O emits and receive light signals which arrive at and depart from E simultaneously, and where c is initially a conventional constant. Clearly the observations of any isolated arbitrary observer have no physical significance. The

only significant observations are those made by a class of observers who are in some way " equivalent " to one another. The Special Theory of Relativity takes a class of observers to be equivalent when they are in uniform relative motion of translation, but Milne's definition of equivalence is much more fundamental and more extensive. In his scheme two observers A and B are said to be equivalent if A and B can graduate their clocks so that A's description of B agrees with B's description of A. He then shows that, if each observer appears to be in uniform motion relative to the other, then the transformation connecting the co-ordinates of A and B is a Lorentz transformation.

The essential feature of this scheme is that each observer A, by convention, assigns co-ordinates in such a way that, in A's co-ordinates, the velocity of light is a constant c, the same for all observers. Hence, for observers in uniform relative motion Milne's scheme leads inevitably to the Lorentz transformation and the Special Theory of Relativity, and it seems to be in effect equivalent to the traditional argument.[1] It is an interesting question to determine the transformation scheme from observer A to observer B when their relative motion is not uniform. Since the velocity of light is invariant it would seem probable that the transformation in question must belong to the conformed group in four variables, which consists of Lorentz transformations and inversions in hyperspheres. This conjection receives support from a recent paper by Professor Page in which he determines the transformation schemes for observers with uniform relative acceleration.[2]

The second main question is that of relativistic cosmology, and it is here that Milne has made such revolutionary changes. The common basis of Milne's theory and of the theories of Einstein, de Sitter and their followers is some form of cosmological principle, by which it is assumed that the distribution of matter is homogeneous throughout the Universe when small-scale irregularities are ignored. The definition of homogeneity requires some care. In current relativistic cosmology homogeneity is taken to mean that the proper density is the same at all points and at all times, and it is customary to choose a metric which itself also exhibits spatial homogeneity. It then follows that each observer at rest with regard to the surrounding matter will form the same large scale picture of the Universe. In Milne's theory the whole of the mathematical analysis required by the Einstein-de Sitter theory and by its observational

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[1] *Cf.* " On the Principles Underlying Professor Milne's Cosmological Theory," H. Dingle, *Philosophy*, Jan. 1936.

[2] " A New Relativity," L. Page, *Phys. Rev.*, Feb. 1st, 1936.

interpretation is short circuited by the adoption of a new definition of homogeneity, *viz.* a system of particles is said to be homogeneous if any two observers moving with *equivalent* particles form the same picture of the Universe as a whole. This definition of homogeneity has the great advantage of being stated in terms of observations, whereas the current definition requires rather careful analysis before its physical significance is revealed.

From this point onwards the development of the new cosmology is a matter of mathematical analysis, and all critics of the new theory unite in their admiration of the simplicity, rigour and elegance with which this analysis is carried out. Firstly, the author obtains a special solution consisting of a homogeneous system of particles in uniform relative motion and shows that such particles would have velocities of recession proportional to their distance from the observer. Secondly, he shows that no constraints are needed to maintain the particles in uniform relative motion, *i.e.* the motion is entirely free and natural. Thirdly, he considers the trajectories of additional particles which do not form part of the system of fundamental equivalent particles in uniform relative motion. Lastly he deals with the general problem of a statistical assembly of freely moving particles. The most surprising result is obtained that the trajectories divide naturally into subsystems such that the particles which belong to any subsystem are strongly concentrated towards one of the fundamental equivalent particles. It thus appears that the subsystems represent nebulæ and the particles represent their constituent stars, so that the analysis gives in a most natural and unforced way the most striking characteristics of the nebular system. At this point there seem to be only two comments possible : " This is too good to be true ! " or " This is too good not to be true ! "

The choice between Milne's cosmology and the current relativistic schemes is not entirely a matter of personal taste, for it appears that there are two crucial points upon which the theories differ decisively. In Milne's theory the number of particles is necessarily infinite, while in the Einstein-de Sitter theory the number of particles is essentially finite. Again in the latter theories it appears that the total number of observable particles steadily increases with the time —there is no such pseudo-creation in Milne's theory. It need hardly be said that at present there is no means of deciding these issues by observation.

In writing this review I have attempted to give an objective picture of Milne's achievement, free from any personal prejudice, but I cannot close without sounding a few personal notes. Those who heard the author lecture at Aberystwyth or at St. Andrews will

never forget the persuasive eloquence of his delivery and the sudden, inspiring lights which he threw on this dark and difficult subject. The present volume is written in the same characteristic and lofty style which frequently rises to heights of pure poetry. To read and study it is to experience an unforgettable joy.

THE RHOPALOCERA OF ABYSSINIA. By HUGH SCOTT, Sc.D., F.L.S., British Museum (Natural History). Being a Review of **The Rhopalocera of Abyssinia : a faunistic study,** by G. D. HALE CARPENTER, M.B.E., D.M., F.L.S., F.Z.S. (*Trans. R. Ent. Soc. London,* lxxxiii, part 3, pp. 313–448, plates 8–12 and map, Dec. 1935. £1 13s.)

THIS paper owes its origin to the author's study of the fine collection made by Sir Arnold Hodson, K.C.M.G., during his tenure (1925–7) of office as British Consul in south-western Abyssinia. But Professor Hale Carpenter has also worked out the butterflies collected during the expedition conducted by the writer of this review in south-central Abyssinia in 1926–7, and certain smaller collections, and has attempted to bring together all published records, among which must be specially mentioned the recent work of H. Ungemach.[1]

For the purposes of this study " Abyssinia " means the parts of the country above the 1500-metre contour line (roughly above 5000 feet). Thus, parts of Eritrea are rightly included though they supply but few of the records. The material collected by Hodson was obtained in the extreme south-western corner of Abyssinia, largely in the promontory of high country which projects south-westwards into the much lower lands of the Sudan, and some specimens taken below the 1500-metre line are considered. The work of the reviewer's expedition was carried out at altitudes between 1500 and 4000 metres (roughly, 5000 and 13,000 feet). Taking the information from all sources, the author remarks (p. 315) that our knowledge is fairly good for the country south of lat. 10° N. (that is, nearly 70 miles north of the capital), but of the Rhopalocera in the area between that parallel and the frontier of Eritrea we know almost nothing.[2]

[1] H. Ungemach, Contribution à l'étude des Lépidoptères d'Abyssinie, 1re Partie : *Mém. Soc. Sci. nat. Maroc,* XXXII, pp. 1–122, 1932.

[2] Professor Hale Carpenter's map does not appear to have been traced entirely from the most recent editions of the War Office maps. Mt. Chillalo is shown a rather long distance south-east of Lake Zwai, but it (the westernmost outlier of the high Arussi mountains) is immediately east, about twenty miles from the lake. Debra Libanos is farther north of Addis Ababa than is stated on p. 318, being between 40 and 45 miles. Judging by the dates and context (p. 318) the " Hawash Bridge " visited by Mr. G. H. Bullock refers to the bridge carrying a caravan track over the Hawash a little south of Addis Ababa and west of Mt. Zukwala, not to the " Awash Bridge," *i.e.* the railway-bridge about 95 miles east of the capital, shown on the map.

The butterflies enumerated in the systematic section (pp. 319–411) total over 500 named forms, comprising some 400 species and a number of sub-specific grades. The chief point on which information was sought is the relationship between East and West Africa, and the author generously expresses the hope that his results will provide a step in arriving at conclusions regarding the origin of the complex Abyssinian insect-fauna as a whole (to which end the reviewer's work has been and will be directed for some years).

As to zoogeographical affinities, the relationships of the Rhopalocera are mainly East African, but in a surprisingly large number of cases they are western. The typical West African fauna is that of the forests, and Rhopalocera support the theory that, during the pluvial periods of Pleistocene time, the western forests extended eastwards along the rivers to what we now call the southern Sudan. While many western butterflies have reached this point, most have got no further. But Abyssinia has over seventy forms the recorded distribution of which is typically western, or the nearest allies of which are found in West Africa. Some of these haunt bush rather than forest, and suggest the former existence of a belt of thick bush extending in an east-and-west direction. The similarity of the flora of the Cameroon mountains and Fernando Po on the one hand, and of Abyssinia on the other—there is even identity of certain species of forest trees—is also considered in this connection.

In Section IV (pp. 411–18) the distribution of nearly 100 species and forms of particular interest occurring in Abyssinia is analysed. The forms with western affinities are placed under six heads, including (to mention only three) those which extend across the continent from West Africa to Kenya and even as far to the south-east as Rhodesia ; those which reach only to Uganda, Southern Sudan and Abyssinia (the largest category numerically) ; and those with a discontinuous distribution in West Africa and Abyssinia. Other categories, apart from those with western affinities, comprise (*inter alia*) forms with apparently discontinuous distribution in East Africa and Abyssinia ; a curious little link with the " Great Crater " country of Tanganyika Territory ; a small northern element, which the author considers an intrusion from the Palæarctic Region, Abyssinia being the only African part of the Ethiopian Region which these species have reached ; and a link with south-western Arabia, suggesting a possible route by which northern species may have entered Abyssinia. The presence of " intruders " in Abyssinia, but in no other part of Africa, and the absence of many species widely spread elsewhere in Africa, suggest that the formation of an arid belt round the highlands, after the last pluvial phase, has

imprisoned the earlier invaders and prevented others from entering. These conclusions at any rate stimulate thought. How far they fit various interpretations of evidence as to past climatic and geological changes in the African Continent, which have been put forward, cannot be fully discussed here.

So far as other groups of insects have yet been worked out, certain broad conclusions tally with those of Professor Hale Carpenter, namely the predominantly East African character of the fauna and the presence of a small Palæarctic element in some groups. Only a slight affinity with West Africa has yet come to light.

Considering the abrupt changes of climate, and of faunistic and floristic associations, dependent on the towering precipices, the mighty canyons and even the smaller steep-sided valleys of the Abyssinian highlands—so that the traveller finds distinct faunas in places scarcely separable on the map—the author wisely devotes a section to the " altitudes to which certain species are confined." All the forms with Palæarctic, several of those with East African, and a few of those with West African, affinities, are restricted to the high levels above 5000 feet.

REVIEWS

MATHEMATICS

An Introduction to Projective Geometry. By L. N. G. Filon, C.B.E., M.A., D.Sc., F.R.S. Fourth Edition. [Pp. xviii + 407, with 79 figures.] (London : Edward Arnold & Co., 1935. 16s. net.)

Prof. Filon's book was first published twenty-seven years ago and the issue of a new edition is a tribute to its continued popularity. No great alterations have been made in order and content. A chapter on inversion in the plane has been added to illustrate transformations which are not projective. Involution has rightly been given an earlier place in the development of the subject, but, contrary to modern usage, imaginary elements, and in particular the circular points at infinity, are not mentioned until after the chapters on foci and focal properties of the conic. The reason for this is the author's insistence on the value of graphical construction to the beginner. In the earlier part of the book little is said that cannot be reproduced on the drawing board, and many graphical exercises are given : the author has anticipated criticism of this approach to the subject by printing these exercises separately from the rest, so that they can be omitted by those who prefer to do so.

We think it is a fair criticism to say that the author's subject is the projective treatment of Euclidean geometry rather than projective geometry in its generality ; in this connection it is significant that although co-ordinates are introduced no mention is made of homogeneous co-ordinates. From a formal point of view we should prefer to begin the book with a more general discussion in which homography and involution could take their place—they come now as late as Chapters X and VII respectively— and then to show how a metric can be introduced with the help of absolute elements. This is perhaps a counsel of perfection and one more likely to appeal to the advanced geometer than to the beginner.

The last quarter of the book deals with three-dimensional geometry and includes some elementary differential geometry of curves and surfaces.

Anyone who works through this book carefully—and there are nearly 900 examples on which he can test his knowledge—can hardly fail to feel the fascination of his subject and to be stimulated to further reading.

<div align="right">D. W. B.</div>

An Introduction to the Theory of Functions of a Complex Variable. By E. T. Copson, M.A. (Oxon.), D.Sc. (Edin.), Professor of Mathematics at University College, Dundee, in the University of St. Andrews. [Pp. viii + 448, with 8 figures.] (Oxford : at the Clarendon Press ; London : Humphrey Milford, 1935. 25s. net.)

Compared with other text-books on functions of a complex variable, this one makes easy reading, a characteristic which will be appreciated particu-

larly by undergraduates. Apart from the general theory, it deals with the Gamma function, Legendre and Bessel functions, hypergeometric and elliptic functions, and briefly with integral functions. Proofs of some of the more difficult theorems are omitted, but ample references are given concerning them. The book is rich in examples, and there is a good index at the end.

The book is, on the whole, well written, but unfortunately no more accurate in details than other books of this kind that I have seen. Thus, on page 59, it is stated that, if $f(z)$ is regular in a domain D, then the integral of $f(z)$, taken along a path contained in D, depends only on the end-points of the path. This is true for simply connected domains only.

<div align="right">T. ESTERMANN.</div>

PHYSICS

Physics for College Students. By A. A. KNOWLTON, Ph.D., Professor of Physics, Reed College. Second edition. [Pp. xxi + 623, with 429 figures and 14 tables.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1935. 21s. net.)

THIS is the second edition of a text-book of physics which is designed mainly for a general reader who wishes to acquire a standard about that of the English Intermediate, and who has a tendency to specialise in arts rather than in science, a type which the American system of education raises in huge numbers. It is therefore written to satisfy American needs, but its free style and many excellent illustrations will be of interest to teachers who wish to amplify intermediate courses. The main feature of the work lies in its avoidance of stereotyped division of physics into different branches such as heat, light, etc., and, while this may not be an advantage to the average student, it may appeal very much to those who wish to study physics from a humanistic standpoint.

<div align="right">L. F. B.</div>

An Introduction to Physical Science. By CARL W. MILLER, Ph.D. Second edition. [Pp. xiv + 409, with coloured frontispiece, 8 plates and 186 diagrams.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1935. 15s. net.)

THE author points out that a study of physics is nowadays essential to a general education. The average student has, however, only time for a short course in the subject, and it is desirable that he shall study not only the fundamental parts but shall also become acquainted with some of the recent developments.

In 380 pages we are led (without the use of calculus) from the problem of falling bodies to Heisenberg's Uncertainty Principle, the deuton, neutron, and positron, ending with ten pages on Cosmic Physics. This edition differs from the first (1932) chiefly by the inclusion of 10 pages on " The Nucleus." The historical order of development is frequently departed from and the various branches of physics are much intermingled.

The treatment is clear, most of the diagrams are good, and a large number of interesting problems are suggested for solution. For those who wish to glide through physics (and it would appear that they are many) this book is excellent. But for the more serious student it will need much supplementing. A schoolboy should know more about " how to test Boyle's Law,"

" the structure of a moving-coil galvanometer," and many other topics, than is given in this book.

Nevertheless, a university student who wishes to understand physics may find this general survey stimulating, if he is content with little or no reference to subjects such as elasticity, surface tension, thermal conductivity and photometry. He will probably be surprised to discover a reasonable introduction to Relativity that does not mention eclipses, or the perihelion of Mercury.

<div style="text-align: right">W. N. BOND.</div>

A Source Book in Physics. By W. F. MAGIE, Emeritus Professor of Physics, Princeton University. [Pp. xiv + 620, with 111 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1935. 30s. net.)

HERE are extracts from many of the most important contributions to Experimental Physics made during the period A.D. 1600–1900. Grouped under subject headings and arranged mainly in chronological order they include the essential parts of a collection of papers selected with the utmost care and without bias of any kind. One complaint only is possible—that the title lacks the qualification Vol. I, for, if nothing unworthy has been admitted, much that is worthy has been left out. Since, however, it appears that the publication of the whole series of source books in the history of the sciences has been made possible only by a grant from the Carnegie Corporation of New York the physicist must be content with the by no means meagre share which Prof. Magie has provided for him.

Space does not permit a list of the whole contents of the book ; but a few papers not readily accessible (at least, in English) may be mentioned, *e.g.*, Leibniz on Quantity of Motion, Torricelli's letter to Ricci dealing with Atmospheric Pressure, Mariotte on the pressure-volume relation for air (why does Prof. Magie state that the law was " discovered " independently ?), Coulomb on the Force of Torsion, Sauveur on Harmonic Tones, Davy's paper in which he does *not* rub pieces of ice together *in vacuo* and that in which Joule stated that he did, Fourier on the Method of Dimensions, Helmholtz on the Conservation of Force ; Bartholinus on Double Refraction, Grimaldi on Diffraction, Balmer on the Hydrogen Spectrum, Oersted on the Magnetic Effects of Currents (the whole paper), Gauss on Magnetic Force, Roentgen on X-rays and, of course, ample selections from the work of the giants. A feast which makes it churlish to grumble at the absence of anything written by, shall we say, Halley, Euler, Wilcke, Lambert, Herschel, Wheatstone or Weber.

Prof. Magie has translated many of the papers himself and has prefaced the extracts by short biographies of their writers. It is necessary to search hard to find material for adverse criticism, *e.g.*, the omission of the picture of the candle and the all-important comment thereon in the extract from Huygens's Treatise on Light and of dots from the end of the *extract* from Ohm's paper on the conduction of electricity. In short, a most admirable piece of work which should find a place in the library of every physicist.

<div style="text-align: right">D. O. W.</div>

A Text-Book of Light. By L. R. MIDDLETON, M.A., B.Sc. [Pp. viii + 288, with 189 figures.] (London : G. Bell & Sons, Ltd., 1935. 6s.)

THIS book covers the syllabus for the various Higher Certificate and University Entrance Scholarship Examinations, and nothing likely to be asked in any of these examinations is omitted (as far as I can see), but the various parts of the book seem very unequal in clarity. Chapters XIII, XIV and XV (Theories of Light, Interference, Diffraction) seem to me very much inferior to the others. They are so mathematical and so fiercely condensed that I think they would be almost useless to anyone who did not already know so much that he would hardly need them. The last chapter, on Polarisation, is very much better. Chapters I, X, XI, XII (Rectilinear Propagation ; Photometry, The Invisible Radiations, The Rainbow ; Colour, The Velocity of Light) are very good, and one of them, XI (The Rainbow ; Colour) contains the best treatment I know. The remaining chapters cover the ground adequately.

The book as a whole seems to me to have two serious defects. Its English is uncomfortable, and it is so obviously aimed at examinations that it quite lacks the scientific temper of the great text-books. Newton, Huygens, Preston, and R. W. Wood seem to write books on Light because they are possessed by the spirit, and must do so. The fact that they are useful for examinations (and if you do not believe this of Newton and Huygens, read them again) is an accidental result of the remarkable thoroughness of their investigations of physical phenomena, and of their immense gusto. The awkwardness of Mr. Middleton's English I can best show by example. On p. 63 we find : " It will be noticed that this equation is opposite in sign to the corresponding one for mirrors. In the case of mirrors, however, the image is real when v is positive, and virtual for v negative, while for lenses the opposite holds, the image being real when v is negative, and virtual when v is positive. Thus with lenses a virtual image is an erect one since the magnification is positive, while a real image is always inverted."

Prof. Quiller-Couch might have had some fun with this if it had been written before he delivered his lectures on " The Art of Writing "—particularly for its use of the words " while " and " case." And equations as a whole don't have signs. Anyone who is not annoyed by the English of this extract should find the book extremely useful in preparation for examinations, for it probably covers the ground it undertakes to cover more comprehensively than any book of its length yet published, and it is cheap, well-printed, and well-bound.

<div align="right">H. W. HECKSTALL-SMITH.</div>

The General Principles of Quantum Theory. By G. TEMPLE, Ph.D., D.Sc. Methuen's Monographs on Physical Subjects. [Pp. viii + 120.] (London : Methuen & Co., Ltd., 1934. 3s. net.)

THE author claims as his object to give an introductory account of the general principles which form the physical basis of the quantum theory.

The book undoubtedly provides a sound exposition of the mathematical apparatus of the quantum theory and we do not question the correctness of the equations and formulæ. The actual physical principles, however, do not stand out as we should have wished.

Some of the statements might be criticised, although the author's meaning

is perhaps correct, *e.g.*, "macroscopic physics and microscopic physics differ widely in their formal objects." We venture to disagree with this statement. What is meant is that the mathematical expression of microphysical laws is very different from the older description of macrophysical laws but the older is really a limit approached by the newer. The author unfortunately conveys the impression that the operational calculus employed is something rather difficult—much more difficult than we feel it ought to be if it were expressed in the best way.

The book constitutes a sound and conscientious presentation but is rather better suited for those who have already some acquaintance with the quantum theory and its notation than for beginners. It is a good book but not a satisfactory introduction. One gratifying feature is the use of the English adjective " proper " instead of the German " eigen." This seems to indicate that the author knows more of the history of mathematical terminology than many writers, for the word " proper " in this sense was in use before the quantum theory was conceived.

We do not wish to be hypercritical; the book is undoubtedly to be recommended for all who wish to acquire a sound knowledge of the quantum theory.

W. W.

Atomic Physics. By MAX BORN, M.A. (Cantab.), Dr. phil. (Göttingen), (Hon.) Sc.D. (Bristol). Translated by JOHN DOUGALL, M.A., D.Sc., F.R.S.E. [Pp. xii + 352, with 103 figures, including 8 plates.] (London and Glasgow : Blackie & Son, Ltd., 1935. 17s. 6d. net.)

IN publishing this book Messrs. Blackie & Son have made a noteworthy addition to their list of translations of treatises on physics by living writers of the highest authority. At the same time Dr. Born must greatly have extended the circle of his indebted readers. For here is a book which all honours students of physics will do well to digest, whether their interests be primarily in the experimental or the theoretical aspects of modern physics. Indeed, it seems that the diverse needs of these two groups of students were kept well in mind when the divisions of the book were first planned, for the consecutive treatment of the subject (256 pages) is followed by 86 pages of appendixes (31 in number) wherein all the important derivations and proofs are gathered in a most convenient form. In this way the experimentally inclined student may follow the mathematics as far as his attainments allow—and, speaking from experience, I shall be surprised if he does not then wish that they permitted him to follow further—whilst the budding theorist, it may be hoped, will read through the first portion of the book before he discovers the appendixes !

It has been said that the book deals with modern atomic physics. Modern, here, is no misnomer. The first German edition was published in 1933 and the presence of the author in this country throughout the time necessary for translation has ensured that much more than a mere translation has resulted. Considerable additions have been made. In one direction these are almost ahead of their time—wherein no possible deprecation is intended —in so far as the new field theory of Born finds its natural place in the treatment. Originally the material of the book formed the basis for a series of lectures delivered in Berlin : this probably explains the lavish use of the first person plural throughout the presentation. The reader will soon

accustom himself to this. Moreover, he will no doubt appreciate the fact that the price of the book shows a trend in the right direction—be it ever so slight.

N. F.

The Diffraction of Light, X–Rays and Material Particles. By CHARLES F. MEYER, Associate Professor of Physics, University of Michigan. [Pp. xiv + 473, with 283 figures.] (U.S.A. : University of Chicago Press ; Great Britain and Ireland : Cambridge University Press, 1934. 22*s*. 6*d*. net.)

IT is very pleasant in these days to meet with a considerable monograph dealing in the main, as this does, with purely classical aspects of physics and yet involving no more than the simplest of mathematics. Prof. Meyer's book will no doubt make its chief appeal to the specialist but there is hardly a page in it beyond the comprehension of a first-year honours student.

About two-thirds of the book is devoted to light, one-sixth to the diffraction of X-rays, the remainder to the diffraction of electrons and material particles and a series of appendices mostly concerned with mathematical details such as the aberrations of the concave grating. The order of treatment of light diffraction is quite orthodox :—zones, Cornu's spiral, Fresnel and Fraunhofer diffraction phenomena, a very detailed and practical account of ruled gratings extending over 80 pages and finally diverse phenomena dependent on diffraction including resolving power, scattering and edge effects. One interesting result which emerges from the discussion of rectilinear propagation is that when an opaque obstacle of *any* shape lies wholly within a circular aperture the illumination on the axis of the aperture is very nearly the same as it is in the absence of the obstacle.

The account of X-ray diffraction is limited to the optical aspects of the phenomenon. It includes diffraction by ruled gratings and the correction of Bragg's law necessitated by the fact that the phase velocity of X-rays in matter is greater than *in vacuo* by about 1 in 10,000. The section on electron diffraction is chiefly concerned with the work of Davisson and Germer and of Thomson, while the last section contains an account of the methods originated by Stern and his collaborators for the investigation of the diffraction of gas streams.

It is perhaps a matter for regret that the author has omitted the whole of the electromagnetic theory of diffraction as well as all reference to the transmission and reflection echelons. As it is he has had ample space to deal with the details of the topics he has selected and the book is likely to remain for a long time the authoritative introduction to the study of diffraction.

D. O. W.

Probability and Random Errors. By W. N. BOND, M.A., D.Sc., F.Inst.P. [Pp. viii + 141, with 16 figures.] (London : Edward Arnold & Co., 1935. 10*s*. 6*d*. net.)

THE aim of this book is to assist students of physics and chemistry in interpreting observations. Prior to a discussion of the Theory of Errors proper, two introductory chapters are included in which the notions of probability basic to that theory are developed. Among the topics afterwards discussed may be mentioned the following : the consequences resulting from scale

readings being discrete, errors which follow the Normal and Median laws of distribution, the estimation of the parameters characterising such distributions, the probable errors of functions of several independent variables and curve fitting. The case of a linear relation between two variables is considered in some detail, both when one variable only is subject to error and also when both are. Finally, a short chapter is included on periodicity. Many practical examples are given throughout, illustrating the methods advocated.

The importance of making the research student aware that there exist tools which will enable him to improve the planning of his experiments and give precision to their interpretation cannot be exaggerated. Dr. Bond, in presenting a simple description of such tools, has done a useful piece of work. It does, however, seem a pity that he has followed so closely the lines of the classical error theory and has not made more attempts to bring the reader into contact with modern statistical theory. This latter theory has not only provided methods of solution to his problems sometimes simpler and more accurate than those which Dr. Bond has given, but has also, in some cases, developed new tools of much greater power. Such methods are available in statistical texts as R. A. Fisher's *Statistical Methods for Research Workers* or L. H. C. Tippett's *Methods of Statistics*, although the reader who turns to them from Dr. Bond's book will find that he must, to some extent, learn a new notation and terminology.

The term "probable error," which is used throughout the book in accordance with the classical theory, has been generally discarded in most fields of application of statistical method. The worker is not as a rule particularly interested in the limits outside which half of the observations are expected to lie, but rather in those beyond which the expectation is some small quantity such as 1 in 20 or 1 in 100. It is therefore as useful and much simpler to give only the standard error, thus avoiding the continual multiplication by the factor 0·6745. In the case of certain distributions it is misleading to use the standard error. The correlation coefficient, discussed on p. 102, is a case in point. Since this coefficient must always lie between the limits − 1 and + 1, it follows that when the true correlation is large the distribution curve must fall off more abruptly on one side of the mode than on the other. This is particularly so when the samples are small. The use of the probable (or standard) error, which involves giving equal limits on each side of the observed correlation, is thus clearly inappropriate. This criticism would apply to the example given on p. 102 where the observed correlation is as much as ·9695. A transformation, which makes the distribution of the correlation coefficient simpler to deal with, has been provided by R. A. Fisher.

A point which might be further clarified is the use of the table of factors given on p. 50. In obtaining the probable error of the mean of a small sample, these factors are to be applied to the estimate of the standard deviation instead of the usual factor 0·6745. However, it should be realised that doing this is not equivalent to using an improved estimate of the standard deviation, but rather to allowing for the uncertainty in that estimate due to the smallness of the sample. Thus when the means of two samples are being compared it is not appropriate to apply these factors separately to the estimates in each sample as is done on p. 70. Tests for the significance of differences among means exist, which take into account the liability to error of the estimates of variation, and these fall into a more general class

of problems which have been widely developed recently under the heading of Analysis of Variance.

Into this same class falls also the X^2 test of goodness of fit of curves fitted by the least square method. This test, forming an alternative to that developed on p. 117, has the added advantage that it takes into consideration the actual magnitude of the residuals.

B. L. W.

Mercury Arc Rectifier Practice. By F. C. ORCHARD, A.M.I.E.E. [Pp. xi + 224, with 106 figures, including 23 plates.] (London : Chapman & Hall, Ltd., 1935. 15s. net.)

SINCE mercury arc rectifiers have become recognised in the electrical industry as an important form of converting plant capable of competing in the open market with rotating machinery an urgent need has arisen for an authoritative account of their operating characteristics and constructional details. The author has promptly responded to that need, showing commendable skill in filling the gap so thoroughly and effectively as to satisfy even the most exacting critic. If there is any justifiable complaint it is that fundamental principles have not been explained in sufficient detail— the reader's opinion will depend on his previous knowledge and training— and in any case there are many other works available to supplement the information provided by this book.

After a discussion of the general principles on which the valve action of the rectifier depends, a description of transformer arrangements, smoothing systems and other accessory apparatus is given. The construction of rectifier plants including glass bulb and steel tank types is carefully explained and their use in sub-stations for power conversion is considered in detail. The installation of the plant, methods of testing, maintenance and operation are dealt with in turn, each from a thoroughly practical point of view. A chapter is devoted to the grid-controlled mercury arc rectifier, attention being drawn to its probable future development as an inverter rendering possible high voltage direct current power transmission on a commercial scale.

Finally an interesting comparison is made between arc rectifiers and rotating types of converting apparatus. The claims of those who advocate the use of rectifiers are amply vindicated and the book should do much to popularise plant of that kind.

H. M. BARLOW.

Elements of Practical Flying. By P. W. F. MILLS. [Pp. viii + 133, with 6 figures.] (London : The Technical Press, Ltd., 1935. 4s. 6d. net.)

THE lay-out of this little book is excellent ; the author's ideas are expressed in simple language, and the printed page is pleasing to the eye. It is a great pity that the surface attractions are not applied to a sound technical foundation.

The object of the book is to explain to the non-technical reader how and why an aeroplane flies, and any such explanation must have as its basis the fundamental laws of the branch of science which we commonly term mechanics. Unfortunately the author appears to have but a hazy grasp of these laws, with the result that the most difficult, and perhaps the most important, chapters dealing with Stability and Control, and Turning suffer

from many unsound expositions; the other early chapters are also found wanting in this respect, though to a lesser degree. It is only when the author escapes from the technical tangles to his last chapter on the Personal Element that he is on sure ground again.

G. T. R. H.

CHEMISTRY

The Structure and Properties of Matter. By H. T. BRISCOE. [Pp. vii + 420, with 139 figures.] (London and New York: McGraw-Hill Publishing Co., Ltd., 1935.   21s. net.)

PROFESSOR BRISCOE has sought to present the metamorphosis of our ideas relating to the structure of matter from the earliest speculations of Democritus and Plato to those of Dirac and Pauling.

On the last page we read: " Our greatest danger lies within ourselves—the danger that lies either in extreme, dogmatic conservatism or in rank radicalism." To be sure the author cannot be charged with that old-fashioned dogmatic conservatism which rejoiced in the accurate presentation of established facts. Indeed, the longest chapter, that on Valency, is remarkable for the number of misstatements it contains. For example, the formulation of carbon monoxide on page 318 is somewhat unexpected from a writer who quotes Professor Sidgwick as an authority, but perhaps this, and the truly remarkable formulæ on page 319, in which nitrogen in its oxides is shown once with five and never with more than six electrons, may be attributed to injudicious typography. No such excuse can, however, condone the statement on page 320, "like carbon . . . the atom of fluorine can exhibit a maximum covalency of four by sharing four pairs of electrons." The paragraph on page 320, "Odd molecules are unstable. Nitric oxide molecules combine with each other, for example, to form molecules of N_2O_2 . . . the two ' odd ' electrons of the molecules form another shared pair. This is the bond that unites the two single molecules," betrays a lamentable ignorance of the well-established facts of elementary chemistry.

In the first six chapters such subjects as the atomic-molecular hypothesis, the periodic system, radioactivity, the electron, proton and other positive particles are considered. The atomic nucleus and the Bohr atom, and the contributions of chemical properties and the evidence of critical potentials and spectra in deciding the distribution of electrons around the nucleus is then described.

A post-scholarship or first-year university student would find the scope and elementary treatment of this part of the book well adapted to his requirements were it not for the very large number of errors, particularly in mathematical formulæ and symbols which might confuse a reader new to the subject. Sufficient care has not always been devoted to the preparation of diagrams. The distribution of the electronic charge in the stable configuration of the hydrogen molecule (Fig. 125) is curiously asymmetric, whilst the theoretical Laue spot pattern for NaCl (Fig. 65) is entirely faulty.

The omission of any reference to ortho- and para-hydrogen in the section on wave-mechanics is noteworthy, whilst the single crystal oscillation X-ray photograph deserves mention in the chapter on crystal structure.

A reasonably catholic list of books suggested for further reading concludes each chapter.

H. I.

A Comprehensive Treatise on Inorganic and Theoretical
   Chemistry : Vol. XIV : Fe (Part 3), Co. By J. W.
   MELLOR, D.Sc., F.R.S. [Pp. viii + 892, with 277 figures.] (London :
   Longmans, Green & Co., 1935. 63s. net.)

WHEN a treatise of this kind reaches its 14th volume there is little left for
the reviewer to say except to express his wonder that the author maintains
the high standard of his earlier volumes. The present part concludes the
treatment of iron, dealing with the halides, sulphides, sulphates, carbonates,
nitrates and phosphates with the complex salts derived therefrom. The
rest of the volume is devoted to cobalt and is the usual mine of information.
The cobaltic amines are not treated exhaustively, a list is given but the
only ones for which descriptions and references are supplied are those derived
from the halides, sulphate, carbonate and nitrate.

O. L. B.

International Tables for the Determination of Crystal Structures :
   Vol. I : Tables on the Theory of Groups. [Pp. xii +
   452, with 464 figures.] Vol. II : Mathematical and Physical
   Tables. [Pp. viii + 240, with 83 figures.] (Berlin : Gebrüder
   Borntraeger, 1935. RM. 33—paper covers ; RM. 40—bound.)

THESE two important volumes are the outcome of a general desire, expressed
in 1929 at a conference of the Faraday Society attended by a number of
crystallographers and X-ray investigators of crystal structure, for an agreed
nomenclature and standard mode of description of each of the 230 space-
groups, to one of which every crystal must conform in structure, and the
knowledge of which we owe to the independent, simultaneous, and wonder-
fully mutually confirming work of Schoenflies, Fedorov, and Barlow. An
editorial committee was formed, consisting of M. von Laue (Berlin), Sir
William Bragg (London), and C. Hermann (Stuttgart) ; and among those
who have assisted in the production are W. L. Bragg (Manchester), W. T.
Astbury (Leeds), Mrs. Lonsdale (London), R. W. G. Wyckoff (New York),
P. Niggli (Zürich), Ch. Mauguin (Paris), L. Pauling (Pasadena), E. Schiebold
(Leipzig), P. P. Ewald (Stuttgart), and J. D. Bernal (Cambridge).
    Probably no science has been so handicapped by rival nomenclatures,
notations, classifications, and descriptive methods, as crystallography, and
the one fault that may be found with these international tables is that yet
another mode of description and labelling has been adopted. The familiar
method of Schoenflies, as so admirably simplified by Hilton, and which was
adopted by Astbury and Yardley (Mrs. Lonsdale) in their excellent memoir
in the Philosophical Transactions of the Royal Society for 1924, in which
the 230 space-groups were so clearly defined and diagrammatically illustrated,
might have been thought to have been adequate. Already, however, another
nomenclature had in 1922 been used by R. W. G. Wyckoff, and now we
have in these Tables yet a third notation, due originally to C. Hermann
but later modified by Ch. Mauguin. Its recommendation is that it not only
gives the full symmetry but indicates the corresponding " setting " of the
crystal, that is, the relation of the a, b, and c axes to the symmetry elements.
The Schoenflies symbols are also necessarily given, for identification purposes.
    The first, larger, volume contains the fundamentals of space-group
theory, notation, and description, essential for the determination and dis-
cussion of crystal structure, including two diagrams from different points

of view for each space-group ; and the second, smaller, volume contains all the remaining mathematical and physical data which have been found useful in X-ray analysis. The text is given triply, in German, English and French. Great trouble has obviously been taken in the very difficult proof-reading in order to avoid mistakes, and the printing is clear, the diagrams are excellent, and the type is pleasantly large.

A. E. H. Tutton.

Inorganic Colloid Chemistry. Vol. II : The Hydrous Oxides and Hydroxides. By H. B. Weiser. [Pp. viii + 429, with 70 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1935. 23s. 6d. net.)

This second volume of Weiser's comprehensive treatise on inorganic colloids is perhaps even better balanced and written with greater ease than the first which dealt with the colloid systems of elements. On many pages one may notice how the author knows his subject matter closely from practical experience. This makes the book more lucid and to the point.

Colloid hydrous oxides and hydroxides are on the whole more hydrophilic than those of the elements. In part owing to this we find a number of physical phenomena which belong to the most interesting in colloid science : syneresis, sol-gel-transformation, marked optical anisotropy, sensitivity of sols towards X-rays, glow phenomenon of highly disperse powders. Thanks to X-ray analysis and other methods one has learnt in many cases (for instance in " iron oxide " sols) to distinguish the different chemical compounds (oxides, hydroxides, basic salts) coming into play. It is quite in accordance with the aim of Weiser's book that the chemical side, compared to the physical, is treated distinctly more thoroughly.

H. F.

The Tenth and Eleventh Reports of the Committee on Contact Catalysis. Division of Chemistry and Chemical Technology. By Guy B. Taylor and Robert E. Burk. [Pp. 181, with 7 figures.] (Washington : National Research Council, 1935.)

The branch of chemical kinetics known as catalysis continues to attract considerable attention. The formidable task of reviewing the subject from both its theoretical and industrial aspects has been undertaken by Mr. R. E. Burk in the Eleventh Report of the Committee on Contact Catalysis.

Interim reports on scientific subjects are apt to be strange things, lacking, as they do, the continuity of monographs or the completeness of standard works. It is to Mr. Burk's credit that he has achieved a standard above the average. He has prefaced his work with theoretical insight gained from the study of some ten more or less well-known books on the subject, the authors of which were, moreover, consulted during its preparation. There are also fairly continuous quotations from some of these sources. Of a number of articles which have been contributed, those by Mulliken, Taylor, Wheland and Zener should prove helpful to those chemists who desire an elementary introduction to some of the recent theoretical developments. Academic chemists, on the other hand, will find items of interest in the well-written summaries of Calingaert and Lankelma on selected industrial topics.

In an effort to co-ordinate commercial interest with scientific research, Bolton (of Du Pont de Nemours) gives a list of problems, the solution of

which would be of direct value to industry. Such co-ordination is, of course, highly desirable ; but there are two parties to it. It is therefore a pity that the other party is not represented. I can think of many an academic chemist who would be equal to the task.

By producing a readable and well-documented report, which has to deal with such varied topics as the manufacture of perfume and the specification of molecular orbitals, Mr. Burk has displayed skilful editorship and is deserving of warm thanks.

The tenth report, consisting of 12 pages, contains nothing which could not have been conveniently embodied in the fuller one.

E. A. Moelwyn-Hughes.

Principles of Experimental and Theoretical Electrochemistry. By M. Dole, Ph.D. International Chemical Series. [Pp. xiii + 549, with 158 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1935. 30s. net.)

In the preface the author dismisses generally accepted definitions of electrochemistry as " too vague and too general for modern science," and substitutes one which is claimed to be more restrictive. As a result of this the author has apparently felt justified in including such subjects as the measurement of dielectric constant, the relation of dipole moment to molecular structure, the production and properties of molecular rays, and electrokinetic phenomena ; while omitting or severely restricting reference not only to matters such as theories of indicators, buffer solutions, colligative properties of solutions, etc., but also to many aspects of so-called irreversible electrode phenomena. The fact that the name of Foerster does not appear in the Author Index affords evidence of the almost complete neglect of the whole field of electrolysis and electrode reactions which, according to most definitions, constitutes the central theme of electrochemistry. The major part of the book is, in fact, concerned with the properties of electrolyte solutions and the thermodynamic treatment of the electromotive force of cells—subjects which have been dealt with extensively in many recent works. Dr. Dole and his collaborators have, however, made such important contributions to these fields, both in the development of experimental methods and in the elucidation of theory, that the publication of the present volume is fully justified. The author deals authoritatively and in an original manner with the various methods for the determination of transport numbers, with the use and theory of the glass electrode, with extensions of the interionic attraction theory, and with many features of general experimental technique. The chapters on phase-boundary and semipermeable-membrane potentials, oxidation-reduction cells, and electro-capillary phenomena, are also particularly welcome. The diagrams are clearly drawn and reproduced, and the printing and binding are thoroughly satisfactory.

H. J. T. E.

Industrial and Manufacturing Chemistry. Part II : Inorganic. A Practical Treatise. Vol. I. Edited by Geoffrey Martin, D.Sc., Ph.D., F.I.C. Fifth edition. [Pp. xix + 496, with 277 figures.] (London : The Technical Press, Ltd., 1935. 28s. net.)

This treatise is divided into two parts, Organic and Inorganic, the latter appearing in two volumes, the one under review being the fifth edition of

Volume I. The aim of the work, as stated in the preface of the first edition published in 1916, is to cover the whole range of subjects with which the industrial chemist and manufacturer are usually concerned, in a form to serve either as a text-book or as a book of reference to meet the requirements of business men interested in chemical processes, chemists, chemical engineers, students and others. The difficulties of the author and his collaborators in endeavouring to cover such a wide field in such a limited space, and in providing at the same time for such widely varying interests, can well be appreciated. These difficulties have been met to a large extent by the summarised method of presentation adopted, together with the very useful references to the literature included at the beginning of each section and in the text. Full use is also made of patent literature.

The present volume includes some fifty sections, the subjects dealt with including fuels and their applications, refrigeration, industrial gases, the technology of water, and the heavy chemical industry in general. There is inclined to be rather a lack of balance as regards the space allotted to the various sections, some of which are extremely short in relation to the importance of their subject. Thus, only six pages are devoted to synthetic ammonia and two pages to the manufacture of nitric acid from synthetic ammonia, considerably less space than that given to the older and less scientific retort process of manufacture. Admittedly, the most recent process and plant details are frequently impossible to make available for publication, but it is somewhat disconcerting to read in this recently revised edition, in connection with the Haber process, that "the Badische Anilin und Soda Fabrik have *now* erected works at Oppau, near Ludwigshafen, for the large scale preparation of synthetic ammonia by this process." Again, in this section as in others, one looks in vain for evidence of the remarkable industrial awakening in the field of applied chemistry which has taken place in Great Britain during the last twenty years, and it is to be hoped that any further revision will bring the book more up to date in this and similar respects. There are some rather surprising omissions from the lists of references, amongst which may be noted in particular that of Greenwood's *Industrial Gases*. It is suggested that many of the illustrations, some of which are of little value, might well be omitted, thus affording valuable space for the fuller treatment of some of the sections which their importance demands.

The book undoubtedly fulfils a useful purpose and is a valuable work of reference. Its value would be still greater if clearer indication were given as to the relative commercial importance of the many processes referred to in the text.

<div align="right">H. W. Cremer.</div>

Fuel: Solid, Liquid and Gaseous. By J. S. S. Brame, C.B.E., F.I.C., F.C.S., and J. G. King, Ph.D., A.R.T.C., F.I.C. Fourth edition. [Pp. xvi + 422, with 2 plates and 82 figures.] (London: Edward Arnold & Co., 1935. 25s. net.)

The progress in our knowledge of Fuel in the ten years since the last edition of Brame's well-known text-book has covered such a wide field that even the expert has found difficulty in keeping pace with it. The new edition (the fourth) has been brought up to date with the collaboration of Dr. J. G. King who from his wide experience as chief chemist to the Fuel Research Station at Greenwich has been in close touch with the latest developments both at home and abroad.

When we remember that coal is the source of nearly all gaseous and a growing proportion of liquid fuel it is clear that in spite of the extension in the use of oil coal is still our most important source of energy. This is reflected in the fact that in this book 205 pages are devoted to solid and gaseous and 108 to liquid fuels. The chapter on liquid fuels is a very useful summary and includes an account of the hydrogenation of coal and tars. Those upon coal treat of the methods of investigation, the scientific and commercial classification, the preparation and up-grading by cleaning and carbonisation and the utilisation of solid fuel.

Coal petrology is not neglected but the subject is changing so rapidly that it is difficult to give an account of it which will remain adequate for long. The same may be said of the classification of coal and the correlation between composition and properties. One must demur to the statements on p. 61, although they are often made elsewhere, that " classification on the elementary constituents of the pure coal substance cannot indicate such important characteristics as fusibility and coking properties, hardness and friability, gas-yielding and burning properties," or that only a general relation can be established between ultimate composition and volatile constituents. Recent work in many directions is showing the remarkably close correlation between elementary composition and both chemical and physical properties.

The chapter on calorific value is particularly good as might be expected from the work of Prof. Brame. The book may be confidently recommended to all interested in Fuel and is a mine of up-to-date information on the subject.

C. A. S.

Organic Syntheses. Vol. XV. CARL R. NOLLER, Editor-in-Chief. [Pp. vi + 104, with 2 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1935. 8s. 6d. net.)

THESE volumes are now so well known and appreciated by organic chemists that they call for little criticism. The present volume contains descriptions for the preparation of acetone cyanohydrin, diazomethane, 2 : 6-dibromo-4-nitrophenol 2 : 6-dibromoquinone-4-chlorimide, di-n-butylcarbinol, 5 : 5-dimethyl-1 : 3-cyclohexanedione, 2 : 4-dimethyl-3 : 5-dicarbethoxypyrrole, 2 : 4-dimethylpyrrole, 2 : 4-dinitroaniline, n-dodecyl bromide, n-hexadecane, n-hexadecyl iodide, homoveratric acid, hydrogen bromide, p-iodophenol, 1-methyl-2-pyridone, o-nitrophenylsulphur chloride, nitrosomethylurea, oleyl alcohol, orthanilic acid, phenylarsonic acid, phenylbenzoyl-diazomethane, γ-phenylbutyric acid, phenylglyoxal, phloroacetophenone, n-propyl sulphide, Reinecke salt, α-tetralone, trichlorethyl alcohol and veratronitrile.

An appendix gives later references to preparations in preceding volumes and additions and corrections. The only criticism that the reviewer would make is of the absence of any warning of the objectionable character of commercial 2 : 4-dinitrochlorobenzene under the preparation of 2 : 4-dinitroaniline. On one occasion a worker received a very serious burn from a solution of this material on the tender skin of the arm owing to the vessicant nature of the impurities nearly always present in the commercial article and dermatitis between the fingers is not uncommon after handling it. Immediate washing of the hands in acetone and then plenty of soap and water is necessary should any technical dinitrochlorobenzene come in contact with them.

O. L. B.

Forensic Chemistry and Scientific Criminal Investigation. By A. LUCAS, O.B.E., F.I.C. Third edition. [Pp. 376.] (London : Edward Arnold & Co., 1935. 18s. net.)

THE third edition of this widely known book is welcomed. Although the work is essentially devoted to chemical methods, the technique connected with the examination of exhibits by the microscope, by the use of X-ray, by ultra-violet light and infra-red light has been described. The present edition differs from its earlier one in several respects, notably the inclusion of substantial and valuable additions such as the matching of torn paper edges, the photography of burnt documents, tests for particles of metal on steel files, the examination of glass fragments and of imprints made by tools and weapons in addition to many other lines of scientific criminal investigation. Probably the greatest advance made in the subjects treated has been in the methods employed in the examination of firearms, cartridge cases and bullets regarding which the author has been conducting research over a long period of years. His observations therefore may be accepted as authoritative. Few books of this character contain such generous bibliographical references and these will be of great value to the research worker. The examination of documents for forensic purposes is fully treated, the subject matter occupying no fewer than seventy-five pages, while the examination of bloodstains is clearly and concisely described including the serological or precipitin test and blood-grouping. There is also a good chapter on poisons in which many tests are described in addition to methods for the extraction, separation and tests for alkaloids. The author is to be congratulated on the new edition of his work which deals with practically every phase of modern scientific criminal investigation. Although the work is too advanced for students in medicine and law, it will be of great value to members of the legal profession who deal with criminal cases since it illustrates the potentialities of existing methods in crime detection. To the medico-legist and all who are interested in forensic chemistry, it is a highly useful book and should be included in departmental libraries. The literary style of the writer renders his descriptions clear and concise, while the general lay-out of the book, with its profuse and excellent index, makes ready reference to any subject easy for the reader.

J. G.

Annual Review of Biochemistry. Vol. IV. Edited by JAMES MURRAY LUCK, Stanford University. [Pp. viii + 639.] (Stanford University P.O., California : Annual Review of Biochemistry, Ltd., 1935. $5.00.)

THE present review, the fourth of the series, continues on the lines of its predecessors, and, like them, includes papers covering a wide range of topics by authors drawn from many different countries. This strong international flavour has characterised these volumes from the beginning, and is fully in evidence in the current review. It is naturally impossible here to attempt detailed criticism of even a few of the twenty-eight articles. It must suffice to state that the work is worthy of the high reputation of the authors who have been invited to contribute these accounts of recent advances in subjects with which they are specially familiar. It is the policy of the editors to devote part of the space to the treatment, in various years, of special topics, as distinct from those major subjects such as the various aspects of metabol-

ism, enzymes, hormones, etc., which constitute the solid basis of Biochemistry. This year the special topics include reviews of choline and allied compounds by Prof. Gaddum, of the biochemistry of malignant disease by Mrs. Holmes, and of plant hormones by Prof. Thiman. This policy is to be strongly recommended, for these articles, in addition to containing much valuable information, tend to be amongst the most readable and interesting in the whole book. In the case of certain of the subjects, especially those in which large numbers of papers have been published, there is clearly sometimes a conflict between the attempt to summarise, or at least mention, as many papers as possible and the wish to make the treatment more discriminating and critical. Though to those actually at work on the subject, an encyclopædic treatment may have advantages, to the less specialised reader—and, after all, the majority of readers are not likely to be specialists on a particular subject—the critical article, which brings out the main advances achieved, is by far the most useful, and certainly the most interesting to read. It is therefore to be hoped that the editors will encourage the reviewers not to allow the wood to be hidden by the trees—even though many quite good trees have to be discarded to that end.

W. O. K.

Biochemical Laboratory Methods for Students of the Biological Sciences. By CLARENCE AUSTIN MORROW, Ph.D. Revised and re-written by WILLIAM MARTIN SANDSTROM, Ph.D. [Pp. xv + 319, with 38 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1935. 18s. 6d. net.)

PUBLISHED originally as an independent laboratory course for students of Agricultural Biochemistry and other branches of the biological sciences the present edition has undergone some minor changes in the order of experiments to make it conform to the text of Prof. Gortner's *Outlines of Biochemistry*. In revising and re-writing the book Prof. Sandstrom has managed to retain the characteristic features of the original in regard to the general mode of presentation, and though reducing the text by some 30 pages has brought it up-to-date by the introduction of a number of more recent methods, such as those of Hagedorn and Jensen and Fiske and Subbarow for estimation of sugar and phosphate respectively ; considerable improvement may be noted in the chapter on Proteins which includes the use of flavianic acid and Reinecke's acid. An illustrated account of the method of estimating uronic acids is likewise a new feature. The new edition is assured of well merited success, at least equal to that of the previous one.

P. H.

The Essentials of Qualitative Analysis. By A. HOLDERNESS, M.Sc., and JOHN LAMBERT, M.Sc. [Pp. vii + 72, with 6 figures.] (London : William Heinemann, Ltd., 1935. 1s. 6d.)

THIS little book gives the beginner an account of the theoretical basis of the analytical tables. All reactions are accompanied by equations, the precipitation of Groups II and III is explained on the ionic hypothesis, two precautionary chapters are included, and special efforts have been made to avoid confusing the beginner. As an elementary text-book it is excellent and may confidently be recommended to all who require a knowledge of the

theory and practice of qualitative analysis of inter-B.Sc. standard. The book is intended to be used in conjunction with Sherwood Taylor's "Tables for Qualitative Analysis."

J. N. S.

Tables for Qualitative Analysis. By F. SHERWOOD TAYLOR, Ph.D., M.A., B.Sc. (London : William Heinemann, Ltd., 1935. 1s. 3d.)

THESE tables are intended to give in a concise form, practical directions for the qualitative analysis of simple mixtures and the name Sherwood Taylor is sufficient guarantee that they will fulfil their purpose. The simplest and most direct methods have been chosen and tested by teaching experience. The tables are printed on seven highly glazed cards which will (presumably) resist the wear and tear of the working-bench, but the reviewer feels that only the most methodical student will keep them in order and be able rapidly to find the required section.

J. N. S.

GEOLOGY

An Introduction to Physical Geology. By W. J. MILLER. Third edition. [Pp. xii + 465, with frontispiece and 397 figures.] (New York : D. van Nostrand Co., Inc. ; London : Chapman & Hall, Ltd., 1935. 15s. net.)

THE first edition of this book, published in 1924, was reviewed in SCIENCE PROGRESS, July, 1926, p. 154. A second edition published in 1927 escaped review in this journal. In this third edition the book has been subjected to thorough revision. Its length has been increased by thirty pages, and the page itself is somewhat larger than in the earlier editions. Many block diagrams, including a number of original drawings, and numerous new photographs, have been added, while some of the less-effective former figures have been deleted. The result has been to raise the value of the book considerably, for good illustrations are absolutely essential to the proper presentation of physical geology. While the detailed descriptions of some common minerals have now been relegated to an Appendix, it still remains difficult, as pointed out in our earlier review, to see what purpose is served by the insertion of these somewhat scrappy notes, when the same end could be better attained by referring the reader to several excellent text-books on this subject. And is it not to be presumed that every student of physical geology has already had a thorough grounding in elementary mineralogy ?

Some changes have been made in the order of presentation of the subject which, in our opinion, enhance the value of the work. The various topics are treated fully, yet briefly and lucidly, with the aid of a very fine series of illustrations ; but the usual impossible diagram of a " graben " (Fig. 118D, p. 131) has been included. That the book has been written with special reference to North America does not lessen its value for extra-American students, as the American continent provides perhaps the world's grandest illustrations of physical geology. There is a very full index, but we think the author should consider the addition of a few of the more important bibliographic references in a future edition.

G. W. T.

A Comprehensive Treatise on Engineering Geology. By CYRIL
S. Fox, D.Sc., F.G.S. [Pp. xvi + 392, with 31 plates and 70 figures.]
(London : The Technical Press, Ltd., 1935.   35s. net.)

THIS book, almost the only one in existence dealing with its particular sub-
ject, is divided into three main sections, Building Materials, Field Opera-
tions and Water Supply.   It covers a wide scope, and is evidently intended
for the use of engineers possessing little or no knowledge of geology.

The section dealing with Building Materials suffers greatly from the
fact that few of the important researches in progress during the last ten
years are referred to in any way.   During this time, a large amount of
fundamental work has been carried out by the Building and Road Research
Stations in this country, and by leading workers in Germany and America,
and the effect of this omission is to render the section definitely inadequate
for the needs of civil engineers who require guidance in the choice of mate-
rials.   The second section, dealing with Field Operations, is extremely inter-
esting, although the discussion of earth dams might have been expanded
with advantage ; while the matter given is based largely on Indian practice,
it is applicable to many civil engineering problems in this country.   The
section dealing with Water Supply is also usefully treated.

Passing to more detailed criticism, it is questionable whether theoretical
considerations as to the age of the earth are worthy of inclusion.   The term
diabase is used, although its disuse has been recommended by an influential
committee of petrologists, while confusion exists as between Abrasion and
Attrition tests, both of which have a distinct meaning in this country.   The
data relating to optical mineralogy might perhaps have been abridged with
greater comfort to the engineering reader, while the reference to springs
on p. 245 would have been improved by the addition of a descriptive figure.

It is to be regretted that so many spelling errors occur, such as Auston
Quarry in place of the celebrated Anston.   The photomicrographs are excel-
lent both in choice and reproduction, but would gain with fuller description
and identifying letters.   The diagrams are clear and explanatory, while the
text itself is very readable in style.   Altogether, a book to be recommended
to the reader requiring a general introduction to the subject.

B. H. KNIGHT.

Report of the Committee on Sedimentation, 1932–1934.   Pre-
pared under the auspices of the Division of Geology and Geography,
National Research Council, U.S.A.   *Bull. N.R.C.*, No. 98.   [Pp. 246.]
(Washington, D.C. :   National Research Council of the National
Academy of Sciences, 1935. $1.)

THIS report, the fifth of a series prepared by the Committee on Sedimenta-
tion, records, like the others, an immense amount of excellent work by a
band of enthusiastic researchers.   In an Introduction, the Chairman, A. C.
Trowbridge, states that in addition to original contributions, the report
includes about 1000 references to current literature on sedimentation.   E.
Antevs writes on " Recent European Studies on Stratification," including
Lundqvist's work on lake sediments and Brinkmann's on stratification and
its conditions.   He also contributes a short note on varved sediments.   H.
Becker reports on German contributions to work on sediments from 1930
to 1933.   This paper is very comprehensive, including as it does 513 refer-
ences which are covered by grouped annotations.   Studies in glacial sedi-

ments from 1932 to 1933 are dealt with by M. M. Leighton and E. Townley, with a very full annotation of the literature. A similar summary of progress in research on sediments by British scientists during 1931–34 has been made by H. B. Milner. F. J. Pettijohn's review of the mineralogy of the sedimentary rocks has been largely anticipated by Prof. P. G. H. Boswell's recent book, but he annotates 35 new papers. Hydrological and hydrographic investigations ,bearing on sedimentation are dealt with by A. M. Piper, and chemical papers by G. Steiger. P. D. Trask compiles a current bibliography of recent sediments and source beds of petroleum with 169 references. W. H. Twenhofel provides notes on various types of sediments and T. W. Vaughan writes on marine bottom deposits studied in the Scripps Institute of Oceanography. The volume closes with an important review of the terminology of coarse sediments by C. K. Wentworth. Petrologists who specialise on sedimentary rocks will find this book a rich field to browse in, and inspiration for continued work.

<div style="text-align: right">G. W. T.</div>

The Dorset Coast : a Geological Guide. By G. M. DAVIES, M.Sc., F.G.S. [Pp. vii + 126, with 8 plates and 33 figures.] (London : Thomas Murby & Co., 1935. 6s. net.)

NEXT to the Isle of Wight, there is no region in the South of England so instructive, geologically and physiographically, as the Dorset Coast, and it is surprising that no small and popular book on the district has appeared since the second edition (1884) of Damon's *Geology of Weymouth*, long out of print and date.

The present work is on very different lines from Damon's. It is arranged as a guide to actual excursions, to be made from three centres. There is therefore some slight and justifiable repetition in the general descriptions of the three sections which precede the detailed excursion accounts. The whole bears evidence of personal familiarity with the ground, and in at least one case we believe it contains the first published account of a geological exposure—that of Kellaways Clay at Bothenhampton, a more westerly exposure than has hitherto been mapped. Very little that is worth visiting has been missed : there is a slight gap, in the area between Swyre and Abbotsbury, where good exposures of Upper Greensand and " Chloritic " Marl at no great distance inland have been overlooked. But in a description of the Dorset Coast, the exclusion of inland exposures must take a somewhat arbitrary character. The sixteen photographic views are very clear and well-chosen, and the same may be said of the text-figures. There is a useful tide-table which takes Summer time into account. The book is of convenient pocket size, and altogether can be heartily recommended to the geological tourist.

<div style="text-align: right">A. M. D.</div>

Geology of Natural Gas. A Symposium. Edited by HENRY A. LEY. [Pp. xii + 1227, with 259 figures, including 7 plates.] (Tulsa, U.S.A. : The American Association of Petroleum Geologists ; London : Thomas Murby & Co., 1935. $6 or 26s.)

THE thirty-eight papers of this volume, only two of which have been published previously, are the outcome of a suggestion made by the late Sydney Powers and Frederick H. Lahee. They deal only with the gas-producing

fields of the North American continent, and have been prepared by authors having a close acquaintance with the areas they describe.

The work is mainly factual, providing an immense collection of data. In addition to the geology, information is given about the history of development of the fields, drilling methods, production statistics and regulations, pipe-lines, associated industries, etc., and for those desiring further data on these or many other points, there are numerous references. Indeed, one is occasionally referred elsewhere for the geology. Much of the value of the book lies in its being a key to additional information.

In spite of the large number of facts there is, at times, a feeling of dissatisfaction. The authors' opinions on many points, and a more critical analysis of the data presented, would have been very welcome.

It is impossible to refer to many of the papers in a brief space. In many fields the oil geology also is necessarily given. Some of the most interesting speculations are prompted by the commercially less important fields or by those which appear to be of exceptional types, e.g. Washington, Idaho, Eastern Oregon and Northern Utah. Repeatedly it is shown that the carbon ratio of coals is far from being a reliable guide to the distribution of gas and oil. Lenticularity, critical permeability below which gas only is produced, the necessity of determining the type of reservoir control, and the importance of buried ridges are amongst the many points stressed. There are numerous tables summarising geological and other facts, and some of the most interesting tables deal with the salt dome area of the Gulf Coast.

A paper is devoted to the natural gases rich in helium, nitrogen, carbon dioxide and sulphuretted hydrogen ; another to the valuation of natural gas properties, and a third to the estimation of gas reserves. Since a very considerable amount of gas is produced along with oil, the usefulness of this last paper would have been increased by the inclusion of details of the estimation of gas reserves in fields yielding oil and gas from a single reservoir.

Occasionally the phraseology is peculiar, but usually it is understandable. There is an abundance of good diagrams, though in a few instances some of the lettering is uncomfortably small. The accessibility of the facts is facilitated by an extensive index of seventy-seven pages, and the book is a valuable addition to the series of volumes published by the A.A.P.G.

G. D. H.

Les Ressources minérales de la France d'outre-mer. Part III :
Le Zinc, le Plomb, l'Argent, le Cuivre, l'Or, les Minerais
radio-actifs, le Mica, les pierres précieuses, substances
diverses. [Pp. iv + 394, with 27 figures and 5 plates.] **Part**
IV : Le Phosphate. [Pp. vi + 207, with 20 figures and 3 plates.]
(Paris : Société d'Éditions Géographiques, Maritimes et Coloniales,
1935. Frs.40 and Frs.20 respectively.)

THE energetic *Bureau d'Études géologiques et minières coloniales*, instituted in Paris a few years ago, has published two more volumes of its detailed review of the mineral resources of French overseas possessions. The first two volumes, on Coal and Iron respectively, were reviewed in SCIENCE PROGRESS, Jan. 1935, p. 565. Vol. III deals with certain metalliferous ores, radio-active minerals, mica, precious stones and miscellaneous substances ; Vol. IV with phosphate deposits. As in previous volumes we have in these a number of separate but well-co-ordinated essays. Lead,

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zinc and silver are dealt with by F. Blondel, copper by P. Lion, gold by P. Delaitre, radio-active minerals by H. Buttgenbach, mica by P. Chomette, precious stones by A. Lacroix, and remaining substances of only secondary importance by F. Blondel. Not the least valuable feature of these essays is the full bibliography of often unfamiliar items which is appended to each of them. Several sections contain matter which is of more than local value.

Vol. IV, on Phosphates, contains much material which will be found of general value in sedimentary petrology. Prof. L. Cayeux opens with an essay on the origin and modes of formation of the phosphates of sedimentary limestones. This is followed by descriptions of the phosphate deposits of Morocco, Algeria and Tunis ; and a chapter on marketing by different authors. The volume closes with a valuable paper by Prof. A. Lacroix on the remaining types of phosphates found in caves, in fissures, and in the superficial deposits of tropical islands. Both these volumes maintain the high standard of previous works in this series.

G. W. T.

BOTANY

Plant Life : A Textbook of Botany. By D. B. SWINGLE, Professor of Botany, Montana State College. [Pp. xv + 441, with frontispiece and 290 figures.] (New York : D. van Nostrand Co., Inc.; London : Chapman & Hall, Ltd., 1935. 15s. net.)

THE underlying idea here used is not to follow the usual logical sequence but to try at the beginning to arouse interest in the student by directing his attention to the plant as a living organism and to its reactions to its environment. The details of morphological and anatomical study are therefore touched upon rather later than is usual in a book of this type. The scheme used by the author seems to work quite well. Care has been taken with the text and with the drawings and there seem to be few obvious errors. The omission of the ligule in the description and figures of Selaginella and of the calyptra of Marchantia is to be regretted. A glossary of technical terms is provided and at the end of each chapter there are a number of questions. The latter will probably prove useful to the lazy student. The book should prove a useful introduction to the subject.

E. M. C.

Plant Physiology. By MEIRION THOMAS, M.A. [Pp. xii + 404, with 57 figures.] (London : J. & A. Churchill, Ltd., 1935. 15s. net.)

THE writer of this book has been faced with the extremely difficult task of producing a volume of medium size which deals with practically the whole of plant physiology, together with certain related aspects of physics and chemistry. The average reader, it is to be feared, will be given the sensation of being hurried from fact to fact, and from theory to theory along a devious and confusing path which is strewn with interruptions in the form of innumerable cross-references and footnotes. The result of this is that some students may find the book tedious to read and may not easily be able to assimilate the large amount of valuable information that it certainly contains.

As a work of reference of moderate pretensions the volume is of considerable value, its use in this direction being furthered by the inclusion of an up-to-date bibliography.

W. L.

Biological Processes in Tropical Soils, with special reference to Malaysia. By A. STEVEN CORBET, B.Sc., Ph.D., F.I.C. [Pp. xiv + 156, with 16 plates and 10 figures.] (Cambridge : W. Heffer & Sons, Ltd., 1935. 7s. 6d. net.)

THE conditions under which the soils, with which the author deals, have developed are comparatively uniform, namely a rainfall of about 100 inches and a temperature of about 80° F. Weathering is mainly chemical and results in lateritic soils, except in the mountains where podsolisation occurs. The natural vegetation is rain forest or coastal mangrove swamps, but the former has, owing to felling, given place to secondary communities in which *Imperata arundinacea* plays the dominant rôle as a pioneer.

The author briefly deals with the soil flora and fauna : the one is characterised by a high proportion of fungi and the other by a paucity of earthworms, but abundant termites and ants. In the mangrove swamps burrowing crustacea play the rôle of earthworms.

In the forests the nitrogen conditions are regarded as being in a state of equilibrium, but in the felled areas the higher insolation is accompanied by a lower nitrogen level. The view is advanced that when the conditions of humidity are uniform the nitrogen and organic content of the soil decreases with increase of temperature and insolation. The normal temperature of the forest soils, namely 25° C., is held to be a critical one above which organic decomposition surpasses the rate of accumulation. The last chapter is devoted to practical aspects such as the consequences of burning and the value of natural cover.

The book as a whole should prove useful to those who require a simple introduction to the nature of tropical soils.

E. J. S.

How to Identify Trees and Shrubs from Leaves or Twigs. By C. T. PRIME, M.A., F.L.S., and R. J. DEACOCK, B.Sc. [Pp. 39, with 57 figures.] (Cambridge : W. Heffer & Sons, Ltd., 1935. 1s. net.)

THIS is a dichotomous key for the identification of common trees and shrubs of southern England both native and cultivated. It is in two parts of which the first is for identification in summer and the second for the winter condition. The figures, though somewhat sketchy, should help with the inevitable difficulties in the use of an artificial key. The aim of the book is good, but would have been more generally useful if vegetative characters had alone been employed. The selection of literature for further reading should certainly have included Marshal Ward's work somewhat on these lines.

E. J. S.

AGRICULTURE AND FORESTRY

Colloids in Agriculture. By C. E. MARSHALL, M.Sc., Ph.D. [Pp. viii + 184, with 14 figures.] (London: Edward Arnold & Co., 1935. 5s. net.)

THIS book has been written for two classes of readers : " —all those who are engaged in scientific agriculture and who are interested in the application of our latest knowledge to their field work and their teaching " on the one hand, and " our agricultural students " on the other.

The latter, in the author's experience, find colloid chemistry difficult,

owing "largely to our haphazard methods of teaching" which apparently consist in brief excursions into colloid chemistry when they become unavoidable, as when studying silicic acid, clay or proteins. This manner of presenting the subject is not confined to the chemistry courses for agricultural students and produces the same unsatisfactory results elsewhere. There are two remedies for this state of things : one is the provision of proper tuition in colloid chemistry, including practical training in its special methods. Failing this, the alternative is the provision of text-books which in a reasonable compass give the student a general survey of the principles of colloid chemistry and show their application to the subjects with which he is more immediately concerned.

Dr. Marshall's book presents a very successful attempt to do so for the students of agriculture. The first part, entitled "The Realm of Colloids" states the general principles in the compass of 68 pages in clear and simple language, the importance of the large interface and the peculiar character of surface molecules being insisted on throughout. Even at this stage the author, where it is possible, draws illustrations from phenomena of interest to the reader ; thus the behaviour of bound water in gels serves to explain the winter hardiness of plants.

The second part is entitled "Colloids in the Soil" and deals with the subject under four headings : the mineral colloids of the soil, the organic colloids of the soil, colloids in soil formation processes, and colloids and soil texture. The present reviewer has derived from it much information on "clay"—which in spite of its somewhat indefinite nature has been used in a number of theoretical investigations, both on the effect of electrolytes and on anomalous flow—as well as on humus. The important subject of base exchange is treated fully and lucidly.

The third part is devoted to "Colloids in Plant and Animal Life," and the first two chapters " Some Colloidal Materials present in Living Organisms " and " Colloidal Architecture in Biological Structures " have to cover a very large amount of ground and do so successfully ; the brief accounts of the proteins and of the natural fibres deserve special commendation. The third chapter discusses milk as an emulsion of fat in casein sol, as well as the technical aspects of butter and cheese making. The final chapter " Smoke Damage and Plant Protection " under the latter heading defines very clearly the physical properties required in insecticide and fungicide sprays to make them efficacious.

The book is written in a singularly attractive style and can be unhesitatingly recommended to the class of readers for whom it is intended. Printing and proof reading leave nothing to be desired, and the price is very reasonable.

E. H.

The Use and Misuse of Land. By R. MacLagan Gorrie, D.Sc., F.R.S.E. Oxford Forestry Memoirs, No. 19. [Pp. 80, with 6 plates and 3 figures.] (Oxford : at the Clarendon Press ; London : Humphrey Milford, 1935. 6s. net.)

As Cobbett and Arthur Young more than 100 years ago toured the countryside here and recorded what they saw and thought, so has Dr. Gorrie, at a more recent date, travelled the length and breadth of the United States, and given us his impressions. But whereas Cobbett and Young were interested in agriculture as a whole, Dr. Gorrie has concentrated his study on

" The Use and Misuse of Land " and has approached it from the physical rather than from the economic or sociological standpoint—though neither of these is entirely neglected in his work.

The publication owes its origin to the award of a Leverhulme Research Fellowship to the author for the purpose of studying " The Correlation of Erosion Damage and Grazing on Forest Lands," and the material was collected during a four months' tour of the United States. In this time Dr. Gorrie travelled 17,500 miles—a spirited attempt to beat the American tourist at his own game in his own country—and he has had perforce to record his impressions in a general way rather than deal in a great degree of detail with the problems of any particular area. In fact, the almost complete absence of statistics is as refreshing as it may be unexpected in such a technical study, but Dr. Gorrie's previous experience of erosion problems in India and elsewhere gives him a greater confidence in making his points than would any number of figures without experience.

The student of land management problems can scarcely fail to benefit from the detailed account that Dr. Gorrie gives of the work that is already being done and that remains to be done to remedy the enormous annual waste that occurs through erosion : to the lay reader (if the term may be allowed) the vast differences in the problems that beset the U.S.A or India compared to this country suggest a train of thought that would otherwise scarcely have been conceived.

The monograph is well arranged, well written, and has some helpful illustrations, but one is inclined to wonder whether the few facetious remarks that occur from time to time, besides detracting from its dignity, do not also lessen the degree of balance on which so much of the satisfaction of reading an essay depends.

W. H. L.

Theory and Practice of Silviculture. By Frederick S. Baker, F.E. American Forestry Series. [Pp. xiv + 502, with 87 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1934. 30s. net.)

The recognition that silviculture is a branch of ecology has enriched both sciences. The practice of silviculture built up over a century or two on an empirical basis has much to contribute in the use of methods of precision while the study of the younger and broader science of ecology focuses upon the forest the light emanating from the study of plant communities in general. It is not surprising therefore that authors of recent text-books on silviculture attempt to correlate practice and theory, not only to expound the art of silviculture but to treat of principles underlying the science. They clearly recognise that within the framework of general principle local factors and circumstances dictate practice but they maintain that an adequate knowledge of theory puts the forester in the way of maximum use of his land.

To this end an exposition of ecological principles has served as the indispensable introduction to applied silviculture. Baker seeks to dig deeper foundations by devoting Part I to Plant Physiology, incorporating much that another would, with equal propriety, include under ecology. Part II, Forest Ecology, is thus emasculated, although the subject of Tolerance is dealt with fully. In Part III, Systematised Silvicultural Experience, silvicultural systems and thinning practice are analysed in the light of the physiological and ecological principles enunciated. Part IV, The Forest itself as a Source

of Silvicultural Knowledge, may be regarded as an extension of Part III, while in Part V there is given a guide to the scattered American literature on applied silviculture.

Efforts to explain practice and natural phenomena are all to the good, but as one reads this book one becomes conscious of shifting from the scientific foot on to the foot of practice and a drawing away of one from the other. This is perhaps inevitable because practice is only partly determined by science and empirical knowledge has not yet been fully interpreted in scientific terms. This inadequate " coupling " of theory and practice is partly due to the physiological approach whereby the tree is the unit and not the forest. Thus there is no treatment of soils, no account of the interactions between trees and the subsidiary vegetation and no suggestions are made how subsidiary vegetation may be used as a guide to practice. But the book is well worth reading and some of the chapters are exceedingly good.

<div style="text-align:right">A. S. W.</div>

Bacteria in Relation to the Milk Supply. By C. H. Chalmers, B.Sc., N.D.A. [Pp. xii + 192, with 31 figures and 4 plates.] (London : Edward Arnold & Co., 1935. 6s. net.)

This book undoubtedly supplies a long-felt want. It condenses into a reasonably compact space a great deal of useful information on the technique of routine examination of milk and water samples, the isolation and identification of bacteria commonly met with in market milk, the various species of micro-organisms responsible for well-known defects, the bacteriological control of the dairy plant on the farm and at the factory, and the routine to be observed in the media kitchen. The chapters all bear the impress of an author with personal experience both on the practical side and in the laboratory. They should provide an excellent guide to those commencing a course in Dairy Bacteriology in Colleges and Universities as well as to commercial bacteriologists whose task it is to investigate and control the quality of milk used for consumption and manufacture.

The subject is divided by the author into two parts—Part I dealing with the methods of bacteriological control and Part II with general bacteriological technique. There are appendices on the composition of culture media, stains and chemical reagents, on the construction and adjustment of the microscope and on the tests prescribed by the Ministry of Health for Graded Milk.

Instruction is facilitated throughout by some very clear diagrams and in the chapter on isolation and identification of organisms there is a series of 16 photomicrographs depicting either morphology or colony formation of those micro-organisms with which every dairy bacteriologist should be familiar.

Mr. Chalmers is to be congratulated on achieving the somewhat difficult object of combining comprehensiveness with the exclusion of unnecessary detail.

<div style="text-align:right">L. A. Allen.</div>

ZOOLOGY

Introduction to Vertebrate Embryology. By W. Shumway, Ph.D. Third edition. [Pp. xii + 390, with 239 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. 20*s.* net.)

In this third edition of his text-book, Prof. Shumway has re-written half of it so as to bring it up to date with the recent advances, especially in the experimental field. The method of treatment of the subject is interesting and novel. After an historical introduction, chapters deal with gametogenesis and genetics and the processes of cleavage, gastrulation, germ-layer formation and embryonic membranes are treated comparatively. The essential principles of causal embryology are then illustrated with reference to amphibia. This is followed by chapters on organogeny and on the anatomy of frog and chick embryos.

While much of this book is to be commended, it may be suggested that better figures might be used to illustrate the development of the skull, and it is to be hoped that the next edition, which this book deserves, will avoid the old error of referring to the vertebrate excretory organ as a " nephridium," since it is certainly *not* homologous with the structure of that name in Annelida.

G. R. de B.

The Invertebrata : A Manual for the Use of Students. By L. A. Borradaile and F. A. Potts, with Chapters by L. E. S. Eastham and J. T. Saunders. Second edition. [Pp. xvi + 725, with 483 figures.] (Cambridge : at the University Press, 1935. 25*s.* net.)

This second edition of the Cambridge work on Invertebrates, intended for the use of University honours students, may be strongly recommended. It is clearly and simply written, with an easy, uncomplicated classification and no unnecessary terminology. Every now and then one is reminded (as, for example, by a reference to *Alice in Wonderland*) that even teachers of zoology are human beings. The book is profusely illustrated, and one of its great recommendations is that many of the figures are not only new but good.

With so many good things to enjoy, one regrets all the more the poor introductory remarks about the Protozoa. The first paragraph informs us that the fundamental thing about them is that " no nucleus ever has charge solely of a specialised part of the cytoplasm." If one reflects on the limited experimental evidence on which the words " has charge " are based, and on their extraordinary vagueness, one cannot accept them in a definition. The author condescends, almost deprecatingly, to give us a good definition of the group (" animals which consist of one cell or of several cells which are all alike, save sometimes the reproductive cells "), but hastens to counter with a really bad one. He says, " If, on the other hand, we give the term ' cell ' its earlier extension, applying it only to the specialised units of nucleus and cytoplasm which together compose the bodies of the higher animals and plants, we shall define the Metazoa as ' cellular animals ' and the Protozoa as ' non-cellular.' " Quite true, but in exactly the same way one might say, " If we give the term ' money ' its earlier extension, applying it only to the wealth of people who are now dead, we shall define the ancients as ' moneyed people ' and present-day men and women as ' non-moneyed people.' " It

would be as reasonable to refuse ever to use the words " nucleus " and " chromosome " when referring to Protozoa, as to refuse to use the word cell on these grounds. This is a subject on which there has been much casuistical writing, which would genuinely shock a chemist or physicist, and also, the reviewer believes, most botanists.

There is little else to criticise in the book. One would like to have seen the Endoprocta definitely, instead of tentatively, separated from the Ectoprocta, and one would have welcomed at least a hint that the Hydrocorallinæ are probably an unnatural group. Again, if it is worth while to mention the nephridium of Nephthys, it is worth while to get it right. Such matters as these are not of great importance, and the reviewer will have no hesitation in telling his students to buy the book.

JOHN R. BAKER.

Pacemakers in Relation to Aspects of Behavior. By HUDSON HOAGLAND. Experimental Biology Series. [Pp. x + 138, with 34 figures, including 9 plates.] (New York and London : Macmillan & Co., Ltd., 1935. 12s. 6d. net.)

FOR countless ages it has been known that many of our vital processes as well as our common actions depend upon rhythmic sequences.

Pulse, respiration, the ovarian cycle and the alternation of limbs in walking and jaws in chewing are only some of the more obvious rhythmic activities. But within recent years the advance in electrical recording has revealed that the activity of the central nervous system is largely manifested in the rhythmic response of component nerve fibres. Behaviour, therefore, is intimately connected with these nervous rhythms, and with the factors which control them —the Pacemakers.

Of the eight chapters of Professor Hoagland's book, the first four are concerned with the general principles which are believed to underlie the activity of irritable tissues. These are the kinetics of the steady state, based upon the work of Osterhout, the conception of relaxation oscillators, illustrated by Lillie's iron wire, and the concept of a " master reaction " following Crozier.

The remaining and by far the larger section of the book is chiefly devoted to observations on certain sensory end-organs.

The description and experimental analysis of the records are well presented, but it is doubtful whether the attempt to " explain " these sensory rhythms in terms of hypothetical and unspecified " master reactions " advances our understanding of them.

The chapter on " The Chemistry of the Time Sense " records the single relevant fact that subjects with pyrexia think that the time is passing slowly, whence it is concluded that " this is consistent with the hypothesis of a chemical master reaction in the nervous system furnishing its possessor with a subjective time scale."

The book would have been excellent had it been restricted to the experimental analysis of sensory end organs, but the attempt to give it wider interest has resulted in a lack of proportion and the inclusion of too much speculative matter.

W. A. H. RUSHTON.

The Locust Outbreak in Africa and Western Asia in 1934. Survey prepared by B. P. UVAROV. Economic Advisory Council Committee on Locust Control. [Pp. 65, with 11 maps.] (London : H.M. Stationery Office, 1935. 3s. net.)

UNTIL recently it was thought that true migrations, in which both an outward and a return journey is made, were confined to birds and fishes. The locust surveys of the present series have proved that at least three African species show comparable migrations, differing only in that breeding takes place at both ends of the journey and sometimes also on the way. These migrations effect such vast areas simultaneously that it is now clear that large-scale seasonal meteorological changes must ultimately control them. Perhaps because they are cold-blooded and more susceptible to local changes than are birds, locusts are somewhat less regular in their migrations. Thus, in the Red Locust, there appear to be minor migrations of the young winged forms into the hills and of the mature ones back to the valleys, these journeys being determined by purely local conditions. The relationship between the individuals making up the swarms may also vary. Sometimes they are all members of one generation, but in the Tropical Migratory Locust in West Africa adults belonging to two successive generations may fly together. This again appears to be determined by the influence of local conditions on breeding and rate of development. Locusts are much more suitable animals for experimental study than are most migratory birds and there appears some hope that in them the migration-problem may eventually be understood in some detail.

An important practical conclusion of the present survey is that Africa in one part or another may always be suffering from locust invasion. The ordinary idea of severe but definitely intermittent outbreaks appears difficult to maintain after the experience of the last ten years.

O. W. R.

Principles of Insect Morphology. By R. E. SNODGRASS, of the U.S. Department of Agriculture, Bureau of Entomology and Plant Quarantine. McGraw-Hill Publications in the Zoological Sciences. [Pp. ix + 667, with 319 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1935. 36s. net.)

THE author established his reputation as a first-class authority upon insect structure by such theses as " The Morphology and Mechanism of the Insect Thorax," 1927, " The Morphology and Evolution of the Insect Head," 1928, " The Morphology of the Insect Abdomen," 1931, all published in the Smithsonian Miscellaneous Collections. The present work contains all the essentials of these and other theses and a great deal of additional material, arranged in 19 chapters and covering 667 pages. The book deals with the most minute detail of structure and almost every detail is given a name (often, as it seems to me, unnecessarily), indicating an extremely precise and accurate mental outlook. Otherwise I should not have referred to the startling first sentence of the Introduction where the author states that " Morphology, in the biological sense, is the science of form in living organisms." Morphology is the science of form and, in the biological sense, it " deals with the form of animals and plants and the structures, homologies and metamorphoses which govern or influence that form." It is certainly not concerned with the difference between living and dead organisms and the author's slip seems to be due to

the fact that he is leading up to the intimate association between structure and function.

The book is well illustrated, mostly from drawings made by the author for previous papers. Almost everything upon which an entomologist could possibly seek information is referred to and there is a full bibliography at the end. Each chapter is followed by a glossary of terms used in it and an English or Latin name is not only defined but the German equivalent is often added. Only those who have struggled through German descriptions and vainly searched dictionaries for many of the names of parts will realise the value of this assistance.

Some of the views expressed as to the origin or identity of structures may be open to criticism but disagreement with a theory will often stimulate thought when a few bare facts without any attempt at explanation will be of little use.

FRANK BALFOUR BROWNE.

Applied Entomology. By H. T. FERNALD, Ph.D. Third edition. McGraw-Hill Publications in the Zoological Sciences. [Pp. x + 405, with 384 figures.] (London and New York : McGraw-Hill Publishing Co., Ltd., 1935. 21s. net.)

THIS text-book of Applied Entomology in the United States follows conventional lines. Two chapters on morphology, one on metamorphosis and five on control methods are followed by a systematic treatment of the different orders with examples almost entirely chosen from insects of Economic Importance. Although the insects dealt with are classified into families, structural characters are not normally given for any groups smaller than orders.

The fact that the book has reached a third edition shows that it supplies a distinct need in North America. Its interest to workers in Great Britain is chiefly due to the fact that many of the insects dealt with are also of importance on this side of the Atlantic. The outlook of the author is, however, very definitely limited geographically ; thus in dealing with the Colorado beetle no mention is made of its occurrence in Europe ; in discussing locusts no mention is made of Uvarov's phase theory which has revolutionised our idea on these insects in the past fifteen years ; in discussing the greenhouse white fly no mention is made of control by parasites which has been so successful in England ; and the author alludes to the life cycle of the pear-thrips as "quite unlike anything known for any other Thysanoptera" when it is exactly similar to that of the injurious pea-thrips of Europe.

These are, however, errors of omission rather than commission and the book is, on the whole, a valuable summary of the knowledge and practice of control of insect pests in the United States at the present day.

C. B. WILLIAMS.

Limnology. By PAUL S. WELCH, Ph.D. [Pp. xiv + 471, with 46 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1935. 30s. net.)

THE author is Professor of Zoology in the University of Michigan, and this admirable text-book is the outcome of a course in general limnology which he has given for many years. The word is used in its widest sense to cover

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all inland waters, not only lakes. The book presents a well-documented
and critical summary of the main facts relating to the physical and chemical
conditions in fresh waters, and their relations to the animals and plants
that populate them. Details of technical methods of research are held
over for a subsequent publication. There is a most useful classified biblio-
graphy running to over 50 pages. It is naturally most complete as regards
American work, and some recent publications in this country and in the
Irish Free State have apparently escaped the author's notice, but it will
be of great service to the student.

It is typical of the present state of research in this field that more is
known of the physics and chemistry of the environment than of the reactions
and behaviour of the organisms inhabiting it. This is perhaps due to the
fact that methods and technique are more highly developed in the physical
than in the biological sciences. Prof. Welch maintains, however, a biological
standpoint, and some of his most valuable chapters deal with the biological
relations existing between fresh-water organisms and their milieu. He pays
considerable attention also to the important question of the productivity
of inland waters, but little or none to the serious problems of pollution.
Rivers and streams are dealt with, but in much less detail than ponds and
lakes, for the reason that much less research has been devoted to them.

It is natural that much more interest is taken in inland waters in the
United States than in this country, where the fresh-water fisheries are of
minor importance compared with the abundant harvest of the sea. But
interest in fresh-water biology is growing steadily, and excellent work is
now coming from the Windermere laboratory of the recently founded Fresh
Water Biological Association. There is still much to be done—our know-
ledge of the fresh-water fauna and flora is still surprisingly incomplete.
This book by Prof. Welch will be of the greatest value to the increasing
number of British biologists who are interesting themselves in fresh-water
problems.

<div align="right">E. S. R.</div>

The Sea Trout of Mull. By G. HERBERT NALL, M.A., F.R.M.S.
Fishery Board for Scotland, Salmon Fisheries, 1935, No. I. [Pp. 44,
with 4 plates.] (Edinburgh : H.M. Stationery Office, 1935. 2s. 6d.
net.)

MR. NALL has written at least a dozen papers about the sea trout of different
Scottish localities, and his treatment of the subject is thoroughly systematised.
In districts where netting is carried on, collections of scale samples can be
large and representative, but, as in the case of these Mull records, when
the collection depends on the sport of the angler, numbers are apt to be
smaller, but Mr. Nall deals with 1098 sets of Mull scales, and other 37 from
the River Aline in Morven.

The Mull sea trout smolt is found to be less in length than the average
smolt of the mainland or of the Outer Hebrides. Also while the majority
migrate at two years, the percentage is lower than elsewhere, being about
59 per cent. of the total. In one stream—the Uisg which flows from the
loch of the same name and enters the sea in Loch Buie—the percentage is
as low as 31, and from this stream also, two fish were obtained which had
migrated when five years old. Most of the samples seem to contain four-
year-old sea trout smolts, and we recollect that in the case of the migrating

salmon, this condition is found in the cold and much frozen north of Norway. The author considers that the smallness of the streams and their rocky nature may account for the condition found. However, in the case of the Ewe, a river of considerable size and flowing from a large loch, Loch Maree, in west Ross-shire, Mr. Nall has found sea trout of specially poor growth, and we recall that the climate of the neighbourhood is as mild as any in Britain. Sea growth, in the case of the Mull fish, is also poor except in the case of a few examples which have probably wandered more widely than the others. The heaviest fish is from the Ba and scaled 14¾ lb., while the oldest fish is found to be 10½ years. The oldest sea trout yet recorded by Mr. Nall's scale reading was a Ewe fish of 19 years, which weighed 12¼ lb. and had spawned no fewer than eleven times. The oldest fish from the Ba had spawned seven times.

W. L. C.

The Flow of Water through the Straits of Dover. Part II. By J. N. CARRUTHERS, D.Sc. Ministry of Agriculture and Fisheries, Fishery Investigations, Series II, Vol. XIV, No. 4. [Pp. 67.] (London : H.M. Stationery Office, 1935. 3s. net.)

ONE of the primary needs of Physical Oceanography is a direct observational knowledge of the movements of the waters of the seas and oceans. Besides the intrinsic interest of such a knowledge it is also a valuable aid to researches on fishery and other problems of Marine Biology. In British waters one of the key-positions for a study of circulation is the Strait of Dover.

These considerations led Dr. J. N. Carruthers of the Government Fisheries Laboratory at Lowestoft to design and develop a current-meter of a type which can be operated over a long period by the crew of a light-vessel. One of his instruments has been in practically continuous activity at the Varne Light-Vessel in the Strait of Dover since June 1926, and the memoir under review is the second report on the results obtained.

The nature of the instrument is such that it gives the resultant flow during the period between its being lowered into the water and its being raised again ; at Varne it has always been suspended at a depth of six fathoms. Each period has been an exact number (usually three) of lunar days, so that most of the alternating tidal motion has been eliminated from each record. These records have been treated so as to give the resultant flow during the individual months, seasons and years. The results show that the normal flow is from the English Channel to the North Sea, but there have been many periods during which the flow has been in the opposite direction. The direction of the resultant yearly flow has varied from N. 40° E. in 1927 to N. 8° W. in 1933 and on the whole this change in direction has been a progressive one. The speeds of the resultant yearly flow have varied from 2·9 to 3·9 sea-miles per lunar day. The flow towards the North Sea is found to be at its strongest in November and at its weakest in February, while the arrangement of seasons in order of decreasing flow in the same direction is autumn, winter, summer, spring.

It is shown that there is a high correlation between the flow of water and the direction and strength of the wind, but that there are certainly other factors at work. The author also examines the variations of current with depth and in particular, gives the results of a special investigation of this matter, extending over about a month in 1931.

J. P.

REVIEWS **Zoological Technique for Teachers and Senior Students.** By
T. L. GREEN, B.Sc., F.R.E.S. [Pp. 118, with 3 plates and 12 figures.]
(London : Allman & Son, Ltd., 1935. 5s. net.)

THIS is a good book for the rather limited number of people for whom it is
intended, namely, those who are starting work as zoology masters at schools.
It tells them a lot of things that they will probably not have been told
when they were themselves students, about how to obtain material, how
to keep it alive, and how to prepare it. There are some useful remarks on
subjects which are usually overlooked in books on methods, e.g. humidity
control in vivaria. It is a pity that the author did not recommend Mayer's
acid hæmalum (the formula with citric acid and chloral hydrate) instead
of Delafield's hæmatoxylin (which is unstable) and Ehrlich's (which is slow).
Mayer could advantageously replace all other hæmatoxylins and hæmateins
for routine purposes. One is surprised, also, that the original formula for
Flemming's fluid (surely one of the most valuable fixatives for many pur-
poses) should be excluded, while the formula without acetic acid is given.
The book is perhaps rather highly priced, for it is actually very small. Its
apparent size is due to interleaving with blank sheets, which will be appre-
ciated by those (the reviewer not among them) who like to write in printed
books. The young zoology master should buy this book.

JOHN R. BAKER.

Living Things. By RICHARD PALMER. [Pp. 400, with 182 figures.]
(London : George Allen & Unwin, Ltd., 1935. 7s. 6d.)

IN this book, which is intended to cover the School Certificate and (Sub-
sidiary) Higher Certificate Examination syllabuses in General Biology, the
object of the author has been to present a knowledge of biology as part of
a broad human culture rather than as the basis of a special training.

Mr. Palmer is to be congratulated on breaking away from the usual run
of School Certificate text-books and dealing with the subject (which he
rightly treats as a whole, and not as two separate sciences) first from the
physiological side. In doing so he has succeeded in producing a really good
book, a book enjoyable to read and full of up-to-date information.

In the second part of the book the author, with the object of giving some
idea of the diversity of living things, deals, from an evolutionary standpoint,
with a variety of animals and plants. The evolution of the various systems
of organs and of the seed is particularly well done. Unfortunately, in this
section, the ideal objective is hardly attained, for rather more attention is
paid to animals than to plants.

Although it is most desirable to give the student a wide view and lead
him on to higher things, one doubts the advisability of trying to produce
a book to cover both lower and (subsidiary) higher-stage examination sylla-
buses. In this instance the result has been to produce a text, at times rather
too diffuse and insufficiently clean-cut for the average school-certificate
candidate. Pupils of this age must be supplied with precise information.
In other words, this is an excellent text-book for a school-certificate class
with a good teacher behind it, ready to assist in making the more difficult
parts of the text more assimilable. Older pupils could readily tackle the
text and profit from reading it ; indeed, it is a book well worth supplying,
for vacation reading, to post-school-certificate pupils prior to their entering
upon Advanced Course work. The wealth of ideas it suggests and its broad

outlook are very commendable features. So also is the section on the Methods of Science which, although a little heavy for the younger pupil, will have achieved its purpose if it makes the older pupil stop to think.

It may be supposed that every teacher has his own ideas on the technique of teaching his own particular subject, but the author falls into a common error in failing to emphasise that for the particular experiment he recommends on photosynthesis (Exp. 5, p. 55) a dicotyledonous plant must be chosen. Again, the conclusion so clearly drawn appears to be contradicted by the results of Exp. 10, p. 58.

For the sake of consistency it would have been better to avoid the constant use of " CO_2 " for " carbon dioxide." " Elators," on p. 334, requires correction. "Paramœcium" (pp. 222–3) should read *paramecium*.

Generally speaking it would not be unfair to say that the zoological parts had been more ably dealt with than the botanical. But, in a book so good, it savours of carping criticism to enumerate minor errors which the author will, no doubt, take good care to correct for the second edition which will surely be demanded.

H. Alan Peacock.

MEDICINE

Handbook of Physiology. By the late W. D. Halliburton, M.D., LL.D., F.R.C.P., F.R.S., and R. J. S. McDowall, M.B., D.Sc., F.R.C.P. (Edin.). Thirty-fourth edition. [Pp. xii + 971, with 4 coloured plates and 382 figures.] (London : John Murray, 1935. 18*s*. net.)

The 34th edition of Halliburton's *Handbook of Physiology*, by Prof. R. J. S. McDowall, forms an important step in the interesting history of this publication. At a time when the requirements of the medical curriculum are being re-examined as regards the teaching of preclinical and clinical subjects, the production of a text-book on physiology must be a source of considerable concern to the author. Prof. McDowall has succeeded admirably, however, in his aim to present clearly the essentials of his far-flung subject, and, at the same time, to contain within reasonable bounds the material necessary for the education of the student in physiology. This has involved a highly critical selection of material and the exclusion of much that is largely of historical interest. While the decision to part from old familiar experiments and illustrations is always a matter for regret, there can be no doubt but that the author is abundantly justified in so doing.

The *Handbook* has now reached a stage in its evolution towards the perfect text-book which should allow it to remain in its present form for some time to come. It is clearly written and illustrated, the headings and other important points noted in dark type, and thus the salient points in nearly all the chapters are quickly evident to the student, who, in addition, has not to wade through a mass of information which is often suitable for the advanced student of physiology. The idea of keeping the amount of small print down to a minimum is a good one in a book of this nature, which aims essentially at presenting as concisely as possible an adequate course of instruction in physiology to students in medicine. Further, the insertion of blank pages permits the students to annotate or to add notes to the material contained in the chapter which precedes. In view of the importance of endocrinology to medicine, it may be necessary to expand the scope of the

chapter on the ductless glands, but, as this subject is at present under a process of intensive review, the author has probably decided wisely to defer this to a subsequent edition.

<div align="right">C. R.</div>

**The Schoolboy; A Study of his Nutrition, Physical Develop-
ment and Health.** By G. E. FRIEND, M.R.C.S., L.R.C.P. [Pp.
xii + 128, with 31 figures and 36 tables.] (Cambridge : W. Heffer
& Sons, Ltd., 1935. 7s. 6d. net.)

THIS book is the record of a long series of observations dating from the war years to the present day on the health, and rate of growth in height, weight and chest measurement of the boys of Christ's Hospital in relation to their diet.

Christ's Hospital was the first public school and still is practically the only public school which has anything like a scientific outlook upon and control of the diet of its boys. It is largely to the energy and organising power of Dr. Friend that this is due and everyone will welcome the publication of the results. The facts set forth are important to parents, to schoolmasters, to boards of governors of schools and to all medical men and students of nutrition.

The historical introduction gives us a glimpse into the bad old days of school dieting. How the boys survived it is a mystery—presumably holidays accounted for what measure of health they achieved. Scurvy we are almost sure was common. Accurate records of dietetic intake did not begin until the war years, and we discover that the restricted quantity and quality of the rationing period, 1917–19, made its mark on the boys entering the school, which it took some eight years or more to obliterate. It is pleasant to record that the boys now leaving Christ's Hospital are 1½–2 in. taller and 7–10 lb. heavier than those leaving in 1917. This is largely due to the better diet, qualitatively and quantitatively, which the boys now receive during their school life.

Dr. Friend and the governors of Christ's Hospital are to be congratulated on their pioneer work and the student of nutrition in their country owes them a deep debt of gratitude.

<div align="right">V. H. M.</div>

The Chemical Control of Conception. By JOHN R. BAKER, M.A.,
D.Phil. With a chapter by H. M. CARLETON, M.A., D.Phil. [Pp.
x + 173, with 8 figures, including 3 plates.] (London : Chapman &
Hall, Ltd., 1935. 15s. net.)

THE widespread use of contraceptives, both mechanical and chemical, without any scientific knowledge as to their efficacy or safety emphasises the need for such a study as the one under review. Neither is it necessary to admit the validity of the arguments in favour of birth control as a means of limiting the growth of population, or of altering the proportions of the contributions to the population by different social classes ; for even the opponents of such restrictive measures must admit that medical indications for the control of conception are of frequent occurrence and that, in the absence of a safe and efficient contraceptive, the only alternative is a surgical sterilising operation that, in so far as the female is concerned (and the operation usually falls to the lot of the female partner), is a long way from being free from danger

to life. Dr. Baker's book is a valuable addition to the literature of con-
traception, and indeed with that of Dr. Voge, on *The Chemistry and Physics
of Contraception* published two years ago, forms the only comprehensive
scientific contribution to our knowledge of the subject. Emanating from
the Department of Zoology and Comparative Anatomy of the University
of Oxford, the work, the results of which are epitomised in this book, was
carried out with the support of the Birth Control Investigation Committee.
Commencing with a short but useful historical introduction (short because
" the history of contraception has been the history of customs rather than
the history of science ") the book goes on to deal with the methods of testing
spermicidal power, with the modes of action of spermicides and with the
results of such scientific tests, when applied to various chemical substances
used, or that might be used as contraceptives. It is interesting to note that
some of the most widely used appliances come out of the tests rather badly.
An important chapter is that by Dr. Carleton on the pathology of contra-
ception. As the author points out, contraceptive substances and proprietary
compounds are tried out, and sometimes permanently adopted as routine
methods by birth control clinics in the absence of adequate experimental
tests, and in many cases it is thus not known whether a product is harmful
or not. The interesting suggestion is made that the inclusion of a paste
or jelly between the *os uteri* and a cervical cap is to be regarded as " a poten-
tially dangerous procedure," in that it may cause irritation and eventually
lead to cancer. The dangers of the Grafenberg ring are pointed out, espec-
ially its possible carcinogenic effect on the fundus uteri and the risk of frag-
mentation consequent on the change of the ring into silver sulphide. Finally,
the net result is that the combination of a really potent contraceptive which
is also devoid of any pathological effect has yet to be found. The book
forms an important landmark in the progress of our knowledge of scientific
contraception, and should be carefully studied by all who are interested in
the subject, and especially by all who are in charge of birth control clinics.

<div align="right">F. J. B.</div>

A Pocket Medical Dictionary. Compiled by LOIS OAKES, S.R.N., D.N.
    Assisted by T. B. DAVIE, B.A., M.D., M.R.C.P. Second edition.
    [Pp. xx + 366, with illustrations.] (Edinburgh : E. & S. Livingstone,
    1935. 3s. net.)

THIS small medical dictionary claims to be suitable as a book of reference
for medical students, but it is probable that nurses and health visitors, whose
medical training is not so thorough as that of the doctor, would find the book
more valuable. That a new edition has been required within two years of
the publication of the first is an indication that the convenient size and the
material contained in the book have appealed to many readers.

This edition contains a number of new words and several additions to
the Appendices, which by many will be found to be the most useful part of
the book. Also the dosage of the various drugs has been revised according
to the latest edition of the *British Pharmacopœia*.

In compiling this book great difficulty must have been experienced in
deciding what to omit, and it is surprising that so much space has been
occupied by diagrams—so small as to be indistinct—of such common pieces
of apparatus as are seen daily by those likely to use the dictionary.

<div align="right">T. C.</div>

Laboratory Manual of Physiological Chemistry. By M. BODAN-SKY and M. FAY. Third edition. [Pp. viii + 274, with 9 figures.] New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1935. 10s. net.)

EVEN in its first edition this book was outstandingly good though it was open to the criticism of laying disproportionate stress on quantitative methods, and of showing a suspicion of nationalism in its choice of these. The medical student does not need a training as an analyst, and the justification for teaching him quantitative methods must be sought in the fundamental principles which the chosen techniques illustrate, the opportunity afforded him to become aware of the relative importance of different kinds of error, and in the training in logic which is involved in the calculation of results. In their original edition the authors gave formulæ for the calculation of results, thereby removing one of the three justifications, but this practice was dropped in the second.

The present edition has been re-arranged somewhat. Most of the experiments on the properties of colloids have been omitted ; there is now no chapter on colloids : the few experiments remaining are put into other sections and the general ratio of qualitative to quantitative exercises has sunk lower. Chapter VIII, dealing with the blood, has been more particularly revised and now includes experiments in the mechanism of clotting of blood. Two methods of blood sugar estimation are given and two for non-protein nitrogen in blood ; for urea in blood, three methods. This chapter errs in fact on the side of thoroughness even more than was the case in the second edition, and one can only hope that the different methods are to be regarded as alternatives. Those who liked the earlier editions of this book will probably consider this edition even better than its predecessors.

P. EGGLETON.

HISTORY OF SCIENCE

A History of Science, Technology and Philosophy in the Sixteenth and Seventeenth Centuries. By A. WOLF. [Pp. xxvii + 692, with 316 illustrations, including 67 plates.] (London : George Allen & Unwin, Ltd., 1935. 25s. net.)

PROF. WOLF has great facilities for the compilation of a history of science, being Head of the Department of History and Methods of Science of University College. Moreover, he has—as he states—enjoyed the support of a large number of colleagues who have expert knowledge, and further he has had the co-operation of Dr. Dannemann, who has, apparently, given permission for full use to be made of his four-volume history of science.

Encouraged, no doubt, by this support and these opportunities, Prof. Wolf has embarked on a plan which is ambitious, being no less than to give a history of all branches of pure and applied science, both physical and biological, and of technology, in the sixteenth and seventeenth centuries. It speaks much for Prof. Wolf's courage, and little for his discretion, that he has not let the task daunt him, in spite of the fact that most of his professional life has been spent as a servant of abstract philosophy. He tackles the history of mining with the same appreciation as he does the philosophy of Descartes, or perhaps even with more. For one of the things that has most surprised the reviewer is the little stress laid on Descartes' theory of vortices, which

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played such a part in pre-Newtonian scientific thought, and continued to exercise an influence into the eighteenth century. Except for a few words of passing reference here and there, all that we have is part of a page about the application of the theory to magnetism, until we come to the last chapter, headed " Philosophy," where the Cartesian cosmogony comes in for mention, but is soon dismissed. Unfortunately this is typical of the book, where, great as is the amount of information offered, proportion and arrangement are not as prominent as might be wished. The book is well balanced in that the author shows equal familiarity with all the branches of science of which he treats, but in no other sense.

One of the chief troubles is the way in which the subject has been grouped into chapters such as *Scientific Instruments, Mechanics, Technology* and the like. This means that Leeuwenhoek's microscope is figured in one place and his work with it described in another (incidentally Dobell's great and delightful work on Leeuwenhoek is not quoted as a reference) ; that Hooke's work is scattered here and there, without any estimate of his position as a man of science ; that the work of Boyle and of the Accademia del Cimento are treated quite separately, while Boyle's air pump is described over a hundred pages away from his work on " pneumatics," as it is called. There is no picture of the spirit of the period or of the growth of ideas—each chapter seems written as a separate essay, and, on the whole, consists of a string of facts. This is not to suggest, of course, that there are not shrewd remarks, as when Prof. Wolf says, " We cannot be certain whether Copernicus really derived his ideas, in the first instance, from the writers whom he quotes, or whether he introduces their names merely for the sake of the impression which these would produce on the readers of his day." But they are few and far between.

The book contains parts of individual interest, such as the account of what are called the Social Sciences (bills of mortality, early economics and the like), some good pictures of early micrometers (source not acknowledged) and a good chapter on Chemistry. There is, however, no sign that the material has ever been assimilated, nor that original authorities have been consulted. Neither the great personalities nor the great achievements stand out. The work is rather a vast scrap-book, where matter taken from various sources is set down, apparently as it came to hand. For instance, Chapter XXI, on *Building Problems*, is, it would appear, taken from the thesis of Mr. Hamilton on the subject—that is, all the figures and all the information are contained in Mr. Hamilton's work, and many passages are transported with only slight verbal changes. There is no great harm in this, although many improvements might have been made in the exposition and the thesis might have been set down among the other references given at the end of the chapter. What does matter is that Galileo's work, in the *Discourses on Two New Sciences,* described in this chapter, is not connected up in any way with the chapter, 400 pages earlier, devoted entirely to Galileo, which is taken mainly from Dannemann ; in the pages devoted to the *Discourses* in the earlier chapter, in fact, no mention is made of the Dialogue on the Resistance of Solids, to which the " Second Day " of the *Discourses* is largely devoted, although this occupies so large a place in *Building Problems.* Actually the experiment on the tensile strength of water is separated from its context and given in the earlier chapter under Pneumatics, without any explanation of its real or supposed significance. The diagram is labelled by Prof. Wolf " The resistance of a vacuum." It is such complete lack of continuity, the wrenching of a single book apart without even cross-references or, apparently, any

appreciation of the original writer's thought, that completely destroys the historical picture, and, though it make the unskilful marvel, cannot but make the judicious grieve.

A book may have many imperfections and still be of great use, and at first sight it would appear that so large a work must serve an important purpose. It is, however, a little difficult to see whom the book is to aid adequately. It is not suited for the young student, who requires above all a treatise with a sense of proportion, historical continuity and perspective and, when it comes to mechanics, one that makes clear the mechanical principles and mode of action of the engines (using the word in the seventeenth-century sense) described. On the other hand the maturer scholar, who desires to locate himself on some particular subject, will not find it of much use, because of the extraordinary lack of references either to the original sources, or, in many cases, to essential secondary sources. This is nowhere more remarkable than in the illustrations. These are not in most, if in any, cases reproduced from photographs of the actual originals, but are taken, apparently, from copies drawn with the pen by a hand skilled enough, but not, perhaps, always clear as to what the details of the original represent ; or from reproductions in other books. In some cases they appear to be from photographs of half-tone reproductions. In the case of the copies we are never told the source—whether they are made from an original, from copies of the originals in other books, or from illustrations that are neither one nor the other. There are occasional references to original sources in the text, but in many places when the present writer wished to refer to the original books he was unable to find any guide but his own memory.

One cannot but admire the energy with which the author and his collaborators have made from collected fragments this mighty mass of synthetic stone, and heave a genuine sigh of regret that the statue that lies concealed within the bulk has not been liberated by the chisel.

<div align="right">E. N. DA C. A.</div>

MISCELLANEOUS

An Historical Geography of Europe. By W. GORDON EAST. [Pp. xx + 480, with 58 figures.] (London : Methuen & Co., Ltd., 1935. 15s. net.)

IN writing this book Mr. East has placed all geographers and historians in his debt. He has collected together for the first time within two covers a vast amount of widely scattered material. His work is scholarly, erudite and well-documented—a considerable storehouse of information rather than an easily digested story. In this marriage of geography and history it is quite definitely history " *qui porte les culottes.*" The viewpoint is that of the historian ; the written word of man rather than the unwritten law of nature is sacrosanct.

The book is divided into three roughly equal parts. In the first, entitled the " Geography of Settlement in Europe," we are shown successively the Roman Empire, the frontier regions of the Roman Empire, the Barbarian invasions and then settlement, urban and rural, in the Middle Ages. Part II, the " Political Geography of Europe," deals in turn with the rise of the major political entities from before Byzantium to the German Empire. Part III, the " Economic Geography of Europe," presents us with a series of detailed pictures of set periods from the Middle Ages to the Railway Age.

Thus the unit for treatment throughout is either the political unit or the period and the author's philosophy is shortly expressed in regard to France (p. 226) : " The geographical frameworks which states come to occupy are certainly never predestined, as it were, by conditions of physical geography. Nevertheless, beneath the surface of historical events which mark the progress towards political unification, geographical factors, though largely masked from view, are in no small degree operative." So the author, in tracing territorial changes, uncovers from time to time the masked geographical factors. But the spectre of determinism haunts him, he will not pursue his uncovering sufficiently far for fear of meeting the spectre. The result is one that is tantalising to the geographer, who, if honest, is only too anxious that the correlations he so frequently makes between the activities of man and the environment should be either corroborated or refuted by the historian. Mr. East has the necessary knowledge ; the reader has the feeling of wishing to ask him a continuous succession of questions. For example, was not the position of the fertile Syrian crescent the dominant factor in determining the Mediterranean–Euphrates route-ways ? (p. 24). At the other end of the book (p. 438) is it not true to say that the political units of Central Europe to-day are closely related to the natural regions, that their frontier problems are essentially the problems inseparable from the determination of the true limits of natural regions and that the downfall of the Austro-Hungarian Empire was consequent upon that colossus having sprawled across all natural boundaries ? How far is the distinction between nucleated and scattered settlements purely a question of water supply (Chap. IV) ? How far is the simple classification of villages into " wet-point," " dry-point " and similar types justified historically ?

This is a valuable book with a long life of usefulness before it. Is it too much to ask that Mr. East envisages for the second edition one with a full series of maps on which every place mentioned in the text is indicated, where possible against the physical background ? The present borrowed maps are both inadequate and confusing. Fig. 5 affords a glaring example, there seems no connection between the places mentioned in the text and those shown on the map.

<div align="right">L. D. S.</div>

Habitat, Economy and Society : A Geographical Introduction to Ethnology. By C. DARYLL FORDE. [Pp. xiv + 500, with frontispiece and 108 figures.] (London : Methuen & Co., Ltd., 1934. 15s. net.)

THERE are indications that in the academic world, as opposed to that of research, the thought of the anthropologist recently has been moving in the direction of the need for restatement in the synthetic study of the history of cultural development. Comparatively little that was written on this topic in the twenty years following the publication of Tylor's *Anthropology* has withstood the test of time, and much water has flowed beneath the bridge since 1885. In America a reaction against the abstraction and generalisation of the usual type of text-book in this subject has produced some interesting work in the form of detailed descriptions of specific peoples, who are labelled as typifying grades of culture. This method has its advantages, especially in the avoidance of abstraction, and in giving a close grip of reality ; but it loses in breadth of view. In effect Prof. Forde's geographical method

is a compromise, in which, dealing only with the simpler cultures, observations of specific peoples in a given environment are made the basis of inferences of broader application in the study of cultural development, due warning being given lest generalisation become too broad. Thus hunting and collecting, though primary means of subsistence, are shown to cover diverse modes of life, the comparison and contrasts being worked out in examples taken from the New World and the Old. Similar treatment is given to the cultivators of Africa, South India, the Pacific and America, while pastoral nomads are treated as exemplified in the contrasting conditions of the Masai, and the Ruwala Bedawin, the horse and sheep nomads of Central Asia and the reindeer herders of Siberia. A valuable chapter on the general problems involved in the history of cultural development completes a scholarly, original and suggestive study.

<div align="right">E. N. F.</div>

Sex and Temperament in Three Primitive Societies. By MAR-GARET MEAD. [Pp. xxiii + 335.] (London : George Routledge & Sons, Ltd., 1935. 10s. 6d. net.)

DR. MARGARET MEAD stands out among field-workers in anthropology to-day not only on account of her qualities as an accurate observer, but also in virtue of a breadth of view in applying the sociological principles which may be deduced from behaviour in the simpler societies to human conduct generally, and more particularly to the problems of modern civilisation. The latter characteristic, which was markedly notable in her two earlier works on children and adolescents in New Guinea and Samoa respectively, is more than ever to the fore in her present study of the relation of sex and temperament in primitive societies in New Guinea. Indeed, it would be difficult to say whether this book will appeal with greater force to the anthropologist, who will value it as a piece of careful ethnographical observation, or to the sociologist, who will be concerned with the author's interpretation of her data in their bearing on the position of women and of certain abnormal types in modern society.

The crux of the problem with which Dr. Mead is concerned is "the feminine temperament." From the popular point of view—which carries weight, even if it is ill-founded—and in scientific theory, with its reservations, certain mental attitudes and capacities are assigned respectively to the sexes. These, it is held, condition their functions and activities. Whether this dichotomy is well founded is the question which the author sets out to answer in the light afforded by data collected in the more readily analysed circumstances of three communities on the Sepik River in New Guinea, which she visited for the purpose in 1931–33.

Dr. Mead finds some very remarkable differences in temperament and outlook as well as in mentality in these three communities. These she brings out very clearly in a lucid record and interpretation of custom and institution. So far as these three communities are concerned she makes out a convincing case that qualities of temperament which it is customary to regard as sex-linked are socially determined. Even here, too, the social " misfit " is encountered as in our own civilisation.

<div align="right">E. N. F.</div>

The Medicine–Man of the American Indian and his Cultural Background. By WILLIAM THOMAS CORLETT, M.D., L.R.C.P. (Lond.). [Pp. x + 369, with 14 plates and 10 figures.] (Baltimore : Charles C. Thomas ; London : Baillière, Tindall & Cox, 1935. 22*s.* 6*d.* net.)

DR. CORLETT has compiled a most entertaining book on a little-known subject and establishes a number of facts about these widely distributed races of Indians, their pre-Europeanised modes of life, religions or superstitions, and especially the cult of their medicine-men and their methods of treating disease. It is apparently certain that these Indians came to America from Asia—to Alaska—in successive waves of immigration, and dispersing at different rates and guided by their various cultures spread eventually throughout the continent of north, central, and south America. They seem to have varied widely in habits and language, some being nomads and pastoral, some hunters and agriculturals, others segregating in towns and devoted to their special arts and manufactures, showing various evolutional deviations from the most primitive to a high state of comparative civilisation. There is, however, a more or less common link uniting these people in their priests and medicine-men, religion and disease being closely associated.

Illness is caused by (*a*) " soul-loss "—by witchcraft, by some enemy, by some evil spirit, by the non-observance of individual or tribal taboo, etc. ; (*b*) by the introduction of some " disease-making " object into the body ; (*c*) by eclipse of sun or moon. Treatment is accompanied by elaborate ritual, songs and dancing, rattling of gourds, tobacco incense and the extraordinary vestments of the medicine-man who is usually under the influence of some drug (coco leaves) and is generally more or less inebriated ; it is often a lengthy *séance* and is performed at night in the dark. The treatment commences with a confession often after the administration of emetics and purges and sometimes bleeding.

If the illness is due to some " disease making " object, this is sucked out by the medicine-man who has previously provided some object such as a spider, worm, stone, etc., which he produces from his mouth at the end of the operation. If the doctor considers the disease incurable the patient may be strangled by the relatives ; sometimes if the treatment fails the medicine-man is put to death.

" Medical training " or initiation to the mysteries of medical art is interestingly described. It is a long process, taking about seven years, till the body of the novice is hardened and enured to pain, and the mind controls the body. But the " science " is much mixed up with conjuring tricks, sleight of hand and humbug. Specialism is very common ; there are different doctors for different diseases. The book ends with an interesting account of Indian midwifery and a list of their *materia medica*.

<div align="right">P. J.</div>

The Tressé Iron–Age Megalithic Monument (Sir Robert Mond's Excavation) : Its Quadruple Sculptured Breasts and their Relation to the Mother–Goddess Cosmic Cult. By V. C. C. COLLUM. [Pp. xii + 123, with 35 plates and 14 figures.] (London : Oxford University Press, 1935. 10*s.* 6*d.* net.)

IN 1929 Sir Robert Mond was invited to undertake the exploration of the *allée couverte* at Tressé, Ille-et-Vilaine, by the Baron Robert Surcouf, its

owner. The offer was accepted and the work of excavation entrusted to Miss Collum. She carried out her task in April 1931, and has now published a report on her results in a finely produced and amply illustrated volume, which is issued at a price made possible only by Sir Robert's well-known generosity to archæological studies. Miss Collum has been able not only to record the course of her excavation step by step in considerable detail without regard to limits of space, but also has taken the opportunity to discuss at length the character and affinities of the cult of which this monument was a material expression, making a survey of the ontological concepts of eastern mysticism, *i.e.* of the gnostic and cognate systems round and about the beginning of the Christian era.

It will be gathered from this orientation of Miss Collum's study of the religious ideas which underlie the Tressé monument that she regards the material with which she is dealing as late in the sense of the prehistorian. Although reported and scheduled as an ancient monument so long ago as 1880, this *allée couverte* had never been explored. Notwithstanding a certain superficial resemblance to some Scottish monuments—probably due, however, to the character of the stone from which it is made—it appears to be indubitably a member in the chain of Brittany megaliths. The ulterior object of this investigation, therefore, was to test the generally accepted view that such monuments were utilised and disturbed in a period long after the time of their erection, thus accounting for the peculiar and anomalous character of their associated relics. In the search for collateral evidence a small mound in the neighbourhood was examined, and a search was made for traces of settlement. The latter met with indifferent success.

A further quest upon which Miss Collum engaged was, as already indicated, to trace the source, meaning and path of migration to Brittany and Great Britain of the cult of the Mother-Goddess or Female Principle. A patent indication of the connection of the *allée* with this cult is afforded by the double pairs of human breasts, two pairs side by side on each of two stones. It is not without interest to note that of these breasts two pairs are of two virgins, while the remaining two are of one and the same matron.

Miss Collum's excavations were conducted with meticulous care, and were recorded with no less careful attention to detail. Special heed was given to stratification, which the author, judging from her wide knowledge of the literature of the megalith, thinks has been insufficiently observed hitherto. From her record she infers that the *allée couverte* of Tressé is the burial-place of a Gallic chieftain, dating from the first century of our era. Fragments of skeletal remains, unburnt, associated with a funerary furniture —pottery, hand- and wheel-made, flint and chert implements of an indeterminate character, and suggestions of personal ornament in the form of steatite beads and what may have been parts of an iron fibula—point to funerary offerings to the Mother-Goddess, whose cult is indicated by the sculptured breasts.

Considerations of space preclude detailed reference to the theological and ontological concepts of eastern mysticism to which Miss Collum relates the cult of the Mother-Goddess in western Europe. It must suffice to point out that her view as to the route by which such doctrines might have come to the West is in substantial agreement with the results of archæological analysis of the material evidence.

In view of the character of Miss Collum's conclusions archæology has

been laid under lasting obligation to Sir Robert Mond for his presentation of what will be a standing work of reference.

E. N. F.

Ability and Knowledge : The Standpoint of the London School. By FRANK C. THOMAS, M.A., B.Sc. [Pp. xx + 338, with 49 figures.] (London : Macmillan & Co., Ltd., 1935. 15s. net.)

THE standpoint and theories of the " London School of Psychology," by which the theories of Spearman are meant, are very usefully and adequately presented in this book. As a perfervid follower of Spearman, the author is perhaps inclined to be at times somewhat dogmatic and rather unsympathetic towards any other standpoint or theory, and the title of the book is also just a trifle misleading to anyone who does not belong to the " school " in question. Nevertheless, if read critically, the book can be heartily recommended.

The psychology of the " London School," as developed by Spearman and expounded by Thomas, is by no means a single, complete, self-consistent body of doctrine. Some psychologists indeed would assert that it is only partly psychology, the remainder being epistemology rather than psychology. In any case there are really two distinct bodies of doctrine very artificially joined together. The first of these develops from the search for a general or central mental factor or function. Spearman since 1904 has maintained the view that mental operations are dependent upon two factors, a general factor *g* entering into all mental operations, and a specific factor *s* specific to the particular operation with which we may be dealing. This conclusion he has arrived at on the basis of the observed correlations between the results of different mental tests, and mathematical deductions therefrom. Any criticism of this body of doctrine must be mathematical, rather than psychological.

It is clear, however, that the nature of this general factor or function *g* is not mathematically deducible. Mathematical formulæ can yield nothing but quantitative relations. This is a psychological enquiry, and to be pursued only by psychological methods. The psychological enquiry leads to the second body of doctrine characteristic of the " school." First of all an attempt is made to formulate the principles or laws of cognition, resulting in the formulation of the three so-called " noegenetic " laws. Of these the first, as formulated by Spearman and by Thomas, hardly seems to belong with the others, if we accept their formulation. This first law is the " law of the apprehension of experience," and is stated : " Any lived experience tends to be followed by an immediate awareness of its characters and of the experiencer." Surely the first principle of cognition is reference to an object, and what is meant by a " lived experience " without " awareness " ? The other two laws express different aspects of that " relating activity," which may be said to be the fundamental characteristic of mind. Many psychologists prefer the account given long ago by Lotze, or that given at the present day by the *Gestalt* psychologists.

Chapter VIII in the present work is devoted to a discussion of a particularly weak part of Spearman's psychology—the " constancy of cognitive output," which attains the dignity of a law, and which later plays an important part in Spearman's attempt to connect up the two bodies of doctrine. What precisely " cognitive output " means is far from clear. Still more

obscure is the meaning of " constancy " in this connection. Finally, there is no evidence whatever to suggest that there is such constancy of output.

These are only a few of the criticisms that may be directed against the psychology of the " London School." For the book itself as a clear and well-written presentation of this psychology one can have nothing but praise.

JAMES DREVER.

This English. By SIR RICHARD PAGET, Bart. [Pp. xii + 118.] (London : Kegan Paul, Trench, Trubner & Co., Ltd., 1935. 4s. 6d. net.)

IN this little book, the author expounds his well-known theory of speech as a series of pantomimic gestures in a fashion that any layman can understand. We mean by this that no special knowledge of any of the sciences which underlie the study of voice, viz. philology, acoustics, physiology and psychology, is required of the reader. Having explained to us the nature of the processes involved in producing and appreciating speech and its relation to gesture and sign language, the author takes a series of short English words ending in ip and eep from a rhyming dictionary and uses them to illustrate the dependence of the mouth and tongue gestures used in producing them on their associated meanings. In this list of some thirty words he certainly states a good case for his theory, though in a number of instances this support is obtained by choosing dialectal or uncommon forms of Old English words, e.g. diop in place of the standard deop—which would have told against him—as the precursor of Modern English, deep. One is tempted to enquire further and ask how the theory fits other words, not cited, of allied meaning. We read that sleep " begins with the backsliding tongue gesture sl-. To sleep is to slide back into the condition of eep in which the tongue and lips mimic a closing eye, as in the words peep and weep." But what of doze, dormir and sova (Swed.) which have a similar meaning ?

Nevertheless, the author has made a good case for a correlation between meaning and gesture in most of the words examined, particularly where consonantal sounds are concerned, even those derived from proper names such as wellington and boycott, which he holds to be survivals of the fittest approximation in gesture to the concept they denote. One wonders how this idea would work with place-names. Certainly some insignificant places have acquired names involving quite a feat of labial acrobatics, e.g. Perranzabuloe in Cornwall, while important cities like York have colourless names.

Sir Richard's views are not orthodox, as he takes care to point out, but a reader who will compare this little book with a treatise by an " orthodox " philologist will find much in it to stimulate interest. He will probably conclude that etymology is not yet an exact science, but that attempts to put it on a scientific basis, such as this, deserve every encouragement.

H. O. AND E. G. R.

BOOKS RECEIVED

Introduction to the Theory of Linear Differential Equations. By E. G. C. Poole, Fellow of New College, Oxford. Oxford : at the Clarendon Press ; London : Humphrey Milford, 1936. (Pp. viii + 202, with 5 figures.) 17s. 6d. net.

Rockets Through Space or the Dawn of Interplanetary Travel. By P. E. Cleator, President of the British Interplanetary Society. With an Introduction by Professor A. M. Low. London : George Allen & Unwin, Ltd., 1936. (Pp. 243, with 22 plates and 21 figures.) 7s. 6d. net.

Foundations of Physics. By Robert Bruce Lindsay, Ph.D., Associate Professor of Theoretical Physics, Brown University, and Henry Margenau, Ph.D., Assistant Professor of Physics, Yale University. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1936. (Pp. xvi + 537, with 29 figures.) 22s. 6d. net.

The Quantum Theory of Radiation. By W. Heitler. The International Series of Monographs on Physics. Oxford : at the Clarendon Press ; London : Humphrey Milford, 1936. (Pp. xii + 252, with 26 figures and 9 tables.) 17s. 6d. net.

Report by the Board of Trade on the Comparisons of the Parliamentary Copies of the Imperial Standards with the Imperial Standard Yard and the Imperial Standard Pound and with each other. London : H.M. Stationery Office, 1936. (Pp. 56, with 6 figures and 37 tables.) 1s. net.

Magnetism. By Edmund C. Stoner, Ph.D., Reader in Physics at the University of Leeds. Second edition. Methuen's Monographs on Physical Subjects. London : Methuen & Co., Ltd., 1936. (Pp. viii + 136, with 20 figures.) 3s. net.

A Class Book of Magnetism and Electricity. By H. E. Hadley, B.Sc.(Lond.), A.R.C.Sc., Principal of the School of Science, Kidderminster. London : Macmillan & Co., Ltd., 1936. (Pp. x + 512, with 432 figures.) 6s. 6d.

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Electrical Engineering in Radiology. A Treatise on the Nature and Function of Electrical Equipment for X-Ray Work in Medicine and Industry. By L. G. H. Sarsfield, M.Sc., M.I.E.E., A.Inst.P., Research Department,

Woolwich. With a Foreword by V. E. Pullin, C.B.E. London: Chapman & Hall, Ltd., 1936. (Pp. xiv + 284, with 199 figures, including 59 plates.) 25s. net.

Hydrostatics. A Text-Book for the use of First Year Students at the Universities and for the Higher Divisions in Schools. By A. S. Ramsey, M.A., President of Magdalene College, Cambridge. Cambridge: at the University Press, 1936. (Pp. viii + 169, with 107 figures.) 7s. 6d.

An Introduction to the Theory of Elasticity for Engineers and Physicists. By R. V. Southwell, M.A., F.R.S., M.I.Mech.E., F.R.Aë.S., Fellow of Brasenose College, and Professor of Engineering Science in the University of Oxford. The Oxford Engineering Science Series. Oxford: at the Clarendon Press; London: Humphrey Milford, 1936. (Pp. x + 509, with 120 figures, including 2 plates.) 30s. net.

Graphical Solutions. By Charles O. Mackey, M.E., Assistant Professor of Heat Power Engineering, Cornell University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1936. (Pp. viii + 130, with 53 figures, and chart in pocket.) 12s. 6d. net.

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Salts and Their Reactions. A Class-Book of Practical Chemistry. By Leonard Dobbin, Ph.D., Reader-Emeritus in Chemistry, and John E. Mackenzie, D.Sc., Reader in Chemistry, University of Edinburgh. Sixth edition. Edinburgh: E. & S. Livingstone, 1936. (Pp. x + 246, with 1 figure.) 6s. net.

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Solutions of Electrolytes. With Particular Application to Qualitative Analysis. By Louis P. Hammett, Ph.D., Associate Professor of Chemistry in Columbia University. International Chemical Series. (Pp. x + 211, with 11 figures and 5 tables.) 12s. 6d. net.

Recent Advances in Organic Chemistry. Vol. II. By Alfred W. Stewart, D.Sc., Professor of Chemistry in the Queen's University of Belfast. Sixth edition. With the addition of Part II, by Hugh Graham, D.Sc., Reader in Organic Chemistry in the Queen's University of Belfast. London, New York, Toronto: Longmans, Green & Co., 1936. (Pp. xiv + 519, with 3 plates and 4 figures.) 21s. net.

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Thorpe's Dictionary of Applied Chemistry. Supplement, Vol. III: Glossary and Index. By Jocelyn Field Thorpe, C.B.E., D.Sc., F.R.S., F.I.C., Professor of Organic Chemistry and Director of Organic Laboratories, Imperial College of Science and Technology, and M. A. Whiteley, O.B.E., D.Sc., F.I.C., formerly Assistant Professor of Organic Chemistry, Imperial College of Science and Technology. Assisted by eminent contributors. London, New York, Toronto: Longmans, Green & Co., 1936. (Pp. viii + 166.) 21s. net.

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Perfumes, Cosmetics and Soaps. With especial reference to Synthetics. Vol. I: A Dictionary of Raw Materials together with an Account of the Nomenclature of Synthetics. By William A. Poucher, Ph.C. Fourth edition. London: Chapman & Hall, Ltd., 1936. (Pp. x + 439, with 36 plates and 3 figures.) 25s. net.

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Soils. Their Origin, Constitution, and Classification. An Introduction to Pedology. By Gilbert Wooding Robinson, M.A., Professor of Agricultural Chemistry in the University College of North Wales, Bangor. Second edition. London : Thomas Murby & Co., 1936. (Pp. xviii + 442, with 5 plates and 17 figures.) 20s. net.

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The Identification of Trees and Shrubs. By F. K. Makins, F.L.S., Diploma of Forestry of the University of Oxford. London : J. M. Dent & Sons, Ltd., 1936. (Pp. 326, with 128 figures.) 15s. net.

Reports on the work of Agricultural Research Institutes and on certain other agricultural investigations in the United Kingdom, 1933–34. London : H.M. Stationery Office, 1936. (Pp. 351.) 5s. net.

Parade of the Animal Kingdom. By Robert Hegner, Ph.D., Professor of Protozoology in the Johns Hopkins University. Assisted by Jane Z. Hegner, formerly Instructor in the University of Missouri. New York and London : Macmillan & Co., Ltd., 1935. (Pp. viii + 675, with 743 figures.) 21s. net.

The Variations of Animals in Nature. By G. C. Robson, M.A., Deputy Keeper of Zoology, British Museum (Natural History), and O. W. Richards, M.A., D.Sc., Lecturer in Entomology, Imperial College of Science and Technology. London, New York, Toronto : Longmans, Green & Co., 1936. (Pp. xvi + 425, with 2 coloured plates and 30 figures.) 21s. net.

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Mechanistic Biology and Animal Behaviour. By Theodore H. Savory, M.A. London : C. A. Watts & Co., Ltd., 1936. (Pp. xvi + 182, with 7 figures and 5 tables.) 7s. 6d. net.

A Monographic Revision of the Genus Ceuthophilus (Orthoptera, Gryllacrididæ, Rhaphidophorinæ). By Theodore Huntington Hubbell.

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Report on Investigations into the Condition of certain of the Oyster Beds in the South Wales Sea Fisheries District (June, 1934). By F. S. Wright, Fisheries Laboratory, Lowestoft. Ministry of Agriculture and Fisheries, Fishery Investigations, Series II, Vol. XIV, No. 5, 1935. London : H.M. Stationery Office, 1936. (Pp. 29, with 7 figures and 10 tables.) 2s. net.

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The White Fish Industry. Second Report of the Sea-Fish Commission for the United Kingdom. London : H.M. Stationery Office, 1936. (Pp. 113, with 1 chart and 39 tables.) 2s. net.

Sea Trout and Trout. By W. J. M. Menzies, F.R.S.E., Inspector of Salmon Fisheries of Scotland. With a Foreword by G. Herbert Nall, M.A., F.R.M.S. London : Edward Arnold & Co., 1936. (Pp. 230, with 16 plates and 3 figures.) 10s. 6d. net.

Lemon Soles (Pleuronectes microcephalus). Marking Experiments in Scottish Waters during the period 1919-1931. By Alexander Bowman, D.Sc., F.R.S.E., with the assistance of Bennet B. Rao, M.A., B.Sc. Fishery Board for Scotland, Scientific Investigations, 1935, No. I. Edinburgh : H.M. Stationery Office, 1936. (Pp. 42, with 6 charts, 1 graph and 13 tables.) 2s. net.

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Tiergeographie des Meeres. Von Sven Ekman, Professor der Zoologie an der Universität Uppsala. Leipzig : Akademische Verlagsgesellschaft m.b.H., 1935. (Pp. xii + 542, with 244 figures.) RM.30.—, paper covers ; RM.32.—, bound.

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Evolution and Heredity. Theories and Problems. By Charles Edward Walker, D.Sc., M.R.C.S., L.R.C.P., Associate Professor of Cytology in the University of Liverpool. London : A. & C. Black, Ltd., 1936. (Pp. x + 222, with 20 figures.) 6s. net.

Psycho-Analysis and Social Psychology. By William McDougall, M.B., F.R.S., Professor of Psychology in Duke University. London : Methuen & Co., Ltd., 1936. (Pp. x + 207.) 7s. 6d. net.

The Natural History of Mind. By A. D. Ritchie, M.A., Lecturer in Chemical Physiology in the Victoria University of Manchester. Tarner Lectures delivered in Trinity College, Cambridge, 1935. London, New York, Toronto : Longmans, Green & Co., 1936. (Pp. viii + 286.) 15s. net.

An Enquiry into Prognosis in the Neuroses. By T. A. Ross, M.D., F.R.C.P., sometime Medical Director, Cassel Hospital for Functional Nervous Disorders. Cambridge : at the University Press, 1936. (Pp. viii + 194, with 1 plate.) 10s. 6d. net.

Twentieth Century Psychiatry. Its Contribution to Man's Knowledge of Himself. By William A. White, M.D., A.M., Sc.D. London : Chapman & Hall, Ltd., 1936. (Pp. 198.) 10s. 6d. net.

Narrative of an Investigation concerning an Ancient Medicinal Remedy and its Modern Utilities. The *Symphytum officinale* and its Contained Allantoin. By Charles J. Macalister, M.D., F.R.C.P. With an Account of the Chemical Constitution of Allantoin. By A. W. Titherley, D.Sc., Ph.D. London : John Bale, Sons & Danielsson, Ltd., 1936. (Pp. vi + 60, with 4 plates.) 2s. 6d. net.

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The Eskimos. By Kaj Birket-Smith, Ph.D., National Museum, Copenhagen. Translated from the Danish by W. E. Calvert. The Translation revised by Professor C. Daryll Forde. With a Foreword by Diamond Jenness,

M.A.(Oxon.), F.R.A.I. London : Methuen & Co., Ltd., 1936. (Pp.
   xiv + 250, with 32 plates.) 15s. net.

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The Mesolithic Settlement of Northern Europe. A Study of the Food-
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Man Makes Himself. By V. Gordon Childe. The Library of Science and
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How to Live Long and Keep Young. By F. Attfield Fawkes. With a
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SCIENCE PROGRESS

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By J. E. SEARS, JNR., C.B.E., M.A., M.INST.MECH.E., F.INST.P.

Superintendent of the Metrology Department, The National Physical Laboratory

THE adoption and use of a uniform system of weights and measures for purposes of trade has been recognised from the earliest times, and by all races which have attained any degree of civilised organisation, as highly desirable, and a proper subject for government action directed to the establishment of suitable standards. Repeated attempts in many countries have shown that in practice this ideal is very difficult of attainment. At the present time, however, it may fairly be said that there are only two basic systems of weight and measure of importance in the whole civilised world —the British Imperial System, and the International Metric System. Where other systems are in use, as for example in India and in China, the units of these systems are now defined, not independently, but by reference either to the British or to the Metric units. The need for uniform standards of measurement for scientific purposes is a more recent development; but the degree of precision required in this connection is a great deal higher than is ordinarily necessary for purposes of trade—*i.e.* for the buying and selling of goods—though the accuracy of measurement demanded for certain purposes by industry is such as to tax the metrologist to the limit even of his present-day resources.

The process of measuring any quantity consists essentially in ascertaining the ratio of that quantity to another known quantity *of the same kind*, which is known as the *unit* of that kind of quantity. Theoretically the reference unit for each different kind of quantity might be fixed entirely independently of those for every other kind. But it would obviously be very inconvenient in practice to have a whole series of disconnected units, and we therefore endeavour to make use of as few independent units as possible, deriving all other units in terms of these, which we describe

as " fundamental " units. In this way we find that all the units of the system of " mechanical " quantities can be expressed in terms of three fundamental units, the particular units chosen, for convenience, to fill this rôle being those of length, mass and time. One great advantage of using a co-ordinated system of units of this kind is that it enables us to apply the test of " dimensional " consistency to all our calculations.

There are four different ways in which we can define the fundamental units of any system, by reference either to arbitrary material standards, to the properties of specified substances, to natural phenomena, or to physical constants. All these have been employed at different times for various units. The present units of length and mass in both the British and Metric Systems are defined by reference to arbitrary material standards, but the unit of time is defined by reference to a natural phenomenon, the time taken by the earth to rotate upon its axis. The Kilogramme was originally defined as the mass of a cubic decimetre of pure distilled water at its temperature of maximum density, but this definition was subsequently abandoned in favour of the arbitrary material standard (the Kilogramme des Archives) which was constructed in the first instance merely to serve as the physical embodiment of the mass so defined.

In addition to the purely mechanical quantities there are, of course, many others for which units are required. The most important of these are the electrical units. These are at present based on properties of materials, the international ohm being defined in terms of the resistance at 0° C. of a mercury column of specified length and mass, and the international ampere in terms of the rate of deposition of silver in a silver voltameter under prescribed conditions. The remaining electrical units are derived from these two, together with the three fundamental mechanical units. The units thus defined are the so-called " practical " units, intended originally as physical realisations approximating closely to the theoretical c.g.s.m. " absolute " units multiplied by suitable powers of 10. It is now known, however, that these approximations are in error by amounts substantially exceeding the experimental errors associated with direct determinations of the corresponding absolute units at the present day. The ohm is too large by 5 parts in 10^4, and the ampere too small by 1 part in 10^4, with the result that the unit of electrical power, the watt, is greater than the corresponding mechanical unit by 3 parts in 10^4. In consequence the International Committee of Weights and Measures, who now have been given responsibility for electrical as well as mechanical units,

decided at their meetings in October 1935 [1] to revert to the system of directly realised " practical absolute " units, and decreed that the change should become effective on January 1, 1940.

It is now generally recognised that the distinctive properties of the electrical quantities cannot be derived from—or rather that these quantities cannot be given adequate dimensional expression in terms of—any combination of the purely mechanical units. Some additional characteristic is required. In the establishment of electrical units on an absolute basis by means of measurements which apparently involve reference only to the three fundamental mechanical units, this characteristic enters unseen into the system in the form of the electro-magnetic permeability of space which, from this point of view, may be regarded as taking the place of a fourth fundamental (electrical) unit which is required to complete the basic structure of the whole electro-mechanical system. The fourth fundamental unit is thus, effectively, referred to a fixed physical constant which involves us in no anxiety as to its permanence. What of the other units which we now see to constitute the essential basis on which all accurate measurement depends ?

The unit of time, as already mentioned, is referred to a natural phenomenon, the speed of rotation of the earth upon its axis. For the practical realisation of this unit we are dependent entirely on astronomical observations, and although we know it is not absolutely constant, the rate at which it is changing is not sufficient to cause us any immediate anxiety. It may, at some future time, become necessary to take account of the lengthening of the second, but the rate of change is so small that up to the present it has not been possible to determine it with any certainty although our measurements of time reach a very high level of accuracy. The principal difficulty with regard to the measurement of time is concerned not with the basic astronomical observations, but with the means (various forms of time-piece) employed to subdivide uniformly the intervals between successive observations which, in bad weather, may be separated by comparatively long intervals of time. The very fact, however, that the astronomical observations cover very long periods of time means that errors in these observations can lead to no ultimate variation in the mean value of the second. Considerable improvements have been, and are still being, made in the accuracy of time-pieces—one of the most recent being the development of the quartz crystal clock—whereby the precision of time measurements, even over quite short intervals, has been brought to a level comparable with those of length and mass. The best criterion of the accuracy with which

the unit of time is at present realised may perhaps be found in the fact that recently standard frequencies of 1000 per second transmitted by wireless have been received and compared at a number of laboratories in various countries, with recorded agreement of the order of 1 part in 10^7. We turn then to our remaining units—those of length and mass—which in both recognised systems are at present referred to arbitrary material standards for their definitions. With what degree of accuracy are these units at present established, what do we know as to their probable constancy, and what prospect is there of future improvement ?

The earliest definite forerunner of the present British standard Yard was the Iron Yard of Edward I and the length of the yard has probably not suffered any substantial change since 1284. The Standard Yard of Edward I is, unfortunately, no longer in existence, the earliest standard we still have being that of Henry VII, which differs by only $\frac{1}{32}$ inch from the present Imperial Standard. A slightly later standard is that of Elizabeth which, though broken and roughly mended, remained the legal standard of the country right up to 1824, and which differs from the present Imperial Standard Yard by only 0·01 inch [2]. These two standards are now included in a collection of weights and measures on exhibition at the Science Museum, South Kensington. Both of them are " end-standards "—that is to say the length which defines the unit is the distance between the terminal faces of the bars, which are nominally flat and parallel to each other, though in these early standards no very high degree of accuracy was attained (or at any rate has been preserved) in this respect.

The modern history of the British standards begins with the Reports of Lord Carysfort's Committee in 1758 [3] and 1759 [4]. This Committee had constructed three Troy pounds in brass, and one of these, together with a brass bar made by Bird in 1760, on which the length of the Yard was defined by the distance between two dots marked on inserted gold plugs, were placed in the custody of the Clerk to the House of Commons. A bill was introduced in 1760 with the intention of giving these two standards the status of primary legal standards, but in the confusion caused by the death of George II and the accession of George III was never passed into law, and the old standards of Elizabeth remained legal until the Weights and Measures Act of 1824 was passed which at last gave effect to this intention. The Act of 1824 [5] prescribed that if the Imperial Standard Yard were ever lost or destroyed it should be restored by reference to a " natural " standard—namely the length of the simple pendulum beating seconds in London. Unfor-

tunately the catastrophe contemplated by this provision occurred all too soon, the fire in the Houses of Parliament in 1834 resulting in the destruction of the Standard Yard, and the complete loss of the Standard Pound. The Committee appointed in 1838 to deal with the problem of replacing the lost standards soon reported that the accuracy with which the length of the seconds pendulum could be determined was far inferior to that which it might be expected to realise in comparisons with other standards in existence whose lengths were known to have been compared with that of the lost Yard.

The method of measurement employed with the Bird Yard was to transfer the distance between the dots to other measures by means of trammels, a proceeding which can hardly be supposed to have been accurate to better than, say, 0·001 inch. The inadequacy of the old Elizabethan standard for precise measurement had long been recognised, and even before 1824 graduated scales, with lines suitable for measurement in comparators by means of micrometer microscopes, had been made for the use of private individuals, Scientific Societies, and the Ordnance Survey.[1] It is somewhat remarkable therefore that a standard of this kind was not adopted for the purposes of the 1824 Act. The Committee decided that the new standard should take this form. Experiments were made with various metals, and eventually a bronze alloy suggested by Mr. F. Baily, containing 16 parts of copper, 2½ parts of tin and 1 part of zinc, was selected as having the most suitable mechanical properties for use in its construction. Following a remark made by Captain Kater in 1826, it was decided that, in order to minimise the effect upon the distance between the defining lines of any variation in the flexure of the bar under its own weight, due to possible variations in the positions of its supports, these lines should be ruled on a portion of surface, specially exposed to receive them, in the neutral plane of the bar. Sir G. B. Airy, the Astronomer Royal, further gave the formula for the distributing of the supports in such a manner as to make the overall length of the bar a maximum, and therefore independent of small variations in position of the supports.

The final form given to the new standard was that of a square bar of 1 inch side, 38 inches in length, with the neutral surface exposed at the bottoms of two small circular pits, ½ inch deep, placed 1 inch from either end. At the centres of these pits two polished gold plugs were inserted, on which the defining lines were

[1] The first scale of this kind would appear to be that made for Sir George Schuckburgh in 1798, which is still in the possession of the Royal Society.

ruled. The standard is supported on eight equally spaced rollers, by means of a system of levers so arranged that each roller takes an equal share of the weight (Plate I, Fig. 1). Both by calculation and by test, however, it has been proved that the distance between the defining lines is sensibly independent of the manner of support.

The construction and verification of the new standard and of the many copies of it that were made and distributed all over the world occupied a period of about ten years, and it was not until 1856 that it was eventually legalised [6]. The main part of the work was carried out by the Rev. W. Sheepshanks, who unfortunately died just as it had been brought to its conclusion. From the account prepared from his papers by Sir G. B. Airy [7], it appears that considerable difficulty was experienced in finding reliable yard measures which had been directly compared with the lost standard with sufficiently well-authenticated results to be used as a basis for establishing the new standard. Eventually reference was made to only three such measures, the brass bar made by Kater for the Royal Society in 1831, and two iron bars belonging to the Ordnance Survey. The values derived from these three bars were in agreement within 0·00025 inch, and it may be concluded that the new standard represented the best current " scientific " standard of the time within this limit, and also reproduced the lost legal standard within the limits set by the uncertainty inevitably associated with any comparisons that could have been made with the latter.

Sheepshanks determined the "probable errors" of his comparisons between different bars of the new series, and concluded that the average probable error of such a comparison was of the order of 0·000004 inch. This estimate, as is a not uncommon experience in metrological work, was certainly unduly optimistic. Subsequent repetitions of the comparisons have shown that they are liable to errors of the order of $\pm 0 \cdot 00002$ inch or approximately 1 part in 2×10^6 Nevertheless, this marks a tenfold advance in accuracy as compared with anything that had previously been attained. For the first time standards of length, and means for comparing them, were established whereby measurements could be made with such a degree of accuracy that it becomes of interest to study, apart from mere questions of reproduction or replacement in the possible event of loss or damage, how far the standards themselves could be relied on to preserve a constant value of the unit.

In this connection it is necessary to remark, in the first place, that when the 1838 Committee commenced its labours the science, or art, of thermometry was still in a very backward state. One

PLATE I

Fig. 1.—Imperial Standard Yard, on roller supports, in coffer, with casket containing the Imperial Standard Pound

Fig. 2.—British National Copy of International Metre, and Case

(Figs 1 and 2 are reproduced by permission of the Controller, Standards Department, Board of Trade)

of the principal difficulties encountered by the Committee in ascertaining the actual values of the various existing measures placed at its disposal for the purpose of re-establishing the lost Standard was due to the great variations—amounting to as much as 1° F.—found between the readings of the thermometers associated with the different measures. Sheepshanks accordingly devoted much attention to the development of improved thermometers and achieved a notable advance in this direction. He was, however, entirely dependent on the relative expansion of mercury in glass for the establishment of his thermometric scale, and had not at his disposal the special thermometric glasses which have since been developed. His thermometers are no longer available and, even if they were, it would no longer be possible, owing to the ageing of the glass, to reproduce his temperatures with certainty. To-day we are able to establish temperatures on the absolute scale correct to the order of 0·001° C. between 0° C. and 100° C. But it is quite possible that the temperature indicated by Sheepshanks' thermometers as 62° F. was as much as, say, ¼° F. in error, which means that, even if the Standard Yard itself had remained invariable, we could not be sure that our measurement of it at 62° F. would agree with Sheepshanks' measurement at the same nominal temperature to better than 0·0001 inch.

Let us now see what evidence we have as to the probable stability of the standard itself. The 1856 Act established four copies, known as " Parliamentary Copies," of each of the new Imperial Standards, and provided that if either standard should ever be lost or damaged it should be replaced by reference to the corresponding copies, or by the adoption of one of them. The Copies P.C.4 of both the Yard and the Pound were immured in the Palace of Westminster, and the remaining three pairs were distributed for custody to the Royal Society, The Royal Observatory and The Royal Mint. The Act further prescribed that these latter Copies should be compared every ten years with each other, and every twenty years with the Imperial Standards themselves, which are in the custody of the Board of Trade. The 1878 Act [8] authorised the construction of an additional pair of Parliamentary Copies, also to be kept in the custody of the Board of Trade. Let us glance for a moment at the results of the successive intercomparisons of these standards which are set out in Table I.[1]

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[1] Tables I, III and IV are reproduced by permission of the Controller, H.M.S.O., from the Report by the Board of Trade on the Comparisons of the Parliamentary Copies of the Imperial Standards, 1936 (H.M. Stationery Office, price 1s. net) [9].

TABLE I

Comparison.	Differences in Millionths of an Inch.							
	1852.	1876.	1886.	1892.	1902	1912.	1922.	1932.
P.C.2 –I	+ 21	+ 36	——	+ 6	——	– 23	– 19	– 39
P.C.3 –I	– 33	+ 57	——	+ 55	——	– 49	– 61	– 111
P.C.5 –I	– 55	– 33	——	+ 70	——	– 43	– 23	– 47
P.C.VI–I	——	——	– 3	——	– 192	– 215	– 217	– 234

In this Table, I represents the Imperial Standard Yard, P.C.2, 3 and 5, the original Parliamentary Copies, and P.C.VI the additional Parliamentary Copy constructed under the authority of the 1878 Act. In considering the table it is at once obvious that the observations on P.C.3 in 1876, and those on P.C. 3 and P.C.5 in 1892 are suspect. It is probable that the comparisons carried out in those years were not as thorough either as the original comparisons of 1852, or as those from 1912 onwards. If we except these three results we see that all the results for the first three bars, made at the same time as I, lie within ranges of $\pm$ 0·00004 inch. Those for P.C.5 lie within a range of $\pm$ 0·00002 inch. These four contemporary bars have thus remained in agreement with each other within $\pm$ 1 part in 10^6 throughout the eighty years of their existence. Though there is some slight suggestion that P.C.2 and P.C.5 may have shortened slightly (within these limits) relative to I, the apparent variations are probably for the most part due simply to the experimental errors of the comparisons, and afford a practical indication of the precision of measurement actually attainable with the present Imperial Standard Yard which, judging by the results of the last three comparisons, may be assessed at about $\pm$0·00002 inch.

In this connection it has to be remarked that the lines on the gold plugs, judged by modern standards, are comparatively roughly ruled, appearing under the microscopes as broad furrows with irregular edges, so that different observers, or even the same observer on different occasions, or using microscopes of different magnifying power, are liable to vary in their interpretation of the " centres of gravity " of the lines. This no doubt largely accounts for the variations shown in the Table being so much in excess of Sheepshanks' probable errors, which were calculated from continuous series of observations by the same observers working under constant conditions.

The case is quite different, however, when we come to consider the new Parliamentary Copy, P.C.VI, constructed as nearly as

possible of the same material, and in the same manner, as the original series, but forty years later, in 1889. Two other indirect determinations of the length of this bar intermediate between 1886 and 1902 completely confirm the change shown between these years, the whole series of determinations, when plotted, falling with remarkable accuracy on a smooth curve of exponential form, with the asymptotic value—228×10^{-6} inch which, it will be seen, has now effectively been reached.

There is here definite evidence that P.C.VI shortened, relative to the earlier bars, by a little over 0·0002 inch during the first forty years of its existence, but by that time had reached a stable state. If we are entitled to assume that the earlier bars, which are made of the same alloy as P.C.VI, behaved similarly to it, then we must conclude that all these bars, including the Imperial Standard Yard itself, are now shorter by about 0·0002 inch than when they were first made, but that the length of the Yard has undergone no further measurable change for the last fifty years. It cannot be said that a change in the length of the Imperial Standard Yard during its earlier years has been definitely established. The Report on the Decennial Comparisons of 1932 mentions a recomparison with the Imperial Standard of two other bars which were also among those included in the original comparisons made by Sheepshanks. Of these, one of the regular series of bronze bars, originally known as the " Generator," since it was used by Sheepshanks as the intermediary against which the values of all the others were established in terms of the lost Standard, shows a slight decrease relative to I, of the same order as that of P.C.2 and P.C.3. The other, a bar of Low Moor iron, but otherwise similar to the rest of the series, also shows an increase, but only of 0·000036 inch. We have therefore either to assume that the bar of Low Moor iron has decreased in length by roughly the same amount as the bronze bars—a possibility, but one involving a rather unlikely coincidence—or else we must admit that the presumed shortening of the bronze bars is " not proven."

Let us turn now to the Metre. The history of this unit is in many respects parallel to that of the Yard. Up to 1824 the Standard Yard was an end-measure. The 1824 Act prescribed that the new standard, if lost, should be replaced by reference to a natural standard—the seconds pendulum. This was found impossible, and recourse was once more had instead to an arbitrary material standard—this time a line-measure. When the Metric System was first set up, the Metre was defined as the ten-millionth part of the earth's polar quadrant passing through Paris. The

material representation of the unit founded on this definition—the Mètre des Archives—took the form of an end-measure in platinum, 25 mm. × 4 mm. in section. It was soon found that a re-determination of the length of the Metre in accordance with the original definition, besides being a much more formidable undertaking, would not afford the same accuracy as could be achieved in the direct intercomparison of material standards, and the material standard was accordingly given the primary status, in substitution for the verbal definition it was originally intended to represent. In 1870, following a recommendation of the Geodetic Conference held in Berlin in 1867, an International Commission was appointed to consider the establishment of international standards of weight and measure based on the French system. In the upshot the Commission reaffirmed the preference for material standards, and arrangements were made for the construction of new, international, standards of both the Metre and the Kilogramme.

For the new International Prototype Metre, and the copies of it which were made for distribution to the various nations subscribing to the treaty of 1875, known as the " Convention du Mètre," the same fundamental principles were adopted as had already been laid down by British scientists in connection with the Imperial Standard Yard ; namely they were made in the form of line measures, with the defining graduations in the neutral plane of their section, and supported on two rollers placed at the " Airy " points. It was, however, decided to make them in an alloy of 90 per cent. platinum with 10 per cent. iridium, this metal being harder than pure platinum, quite inoxidisable, and capable of taking a beautiful polish. On account of the high cost of this alloy, however, the bars were made of a specially designed section, calculated by G. Tresca to give maximum rigidity for weight, the neutral plane being exposed throughout the length on the upper surface of the " bridge " of the section, with the graduation marks on two small polished areas at either end of this surface (Plate I, Fig. 2).

The bars themselves were prepared in England by Messrs. Johnson Matthey & Co., machined in France, and graduated by Tresca at the new Bureau International des Poids et Mesures which was established under the 1875 Treaty. They were then compared at the Conservatoire des Arts et Métiers with the original Mètre des Archives. The various stages of the work took several years to complete, so that it was not till 1889 that the new standards were formally sanctioned by the first International Conference of

Weights and Measures [10], which was held in that year under the terms of the 1875 Treaty. The new Prototype Metre was declared equal [10a], within the limits of experimental error inherent in the comparisons, with the old Mètre des Archives which, from then on, it superseded. It was forthwith deposited in the vault of the Bureau International, together with two copies designated Témoins and intended for use solely as intermediaries in controlling the working standards of the Bureau. It has only once since been re-compared with its Témoins, but arrangements have now been made for a further re-comparison with a view to furnishing a report to the ninth International Conference, which takes place in 1939, the Jubilee year of the original establishment of the International Metric Standards.

The graduations of the new metre bars were superior to those of the Yard standards, and enabled a higher accuracy of measurement to be attained. We shall now proceed to discuss the degree of precision actually attainable in comparisons with the present standard of the Metre, and the constancy with which we may consider its value has been maintained. In the first place, having regard to the difficulty which still existed in 1875 in the accurate measurement of temperature, the International Committee again gave much attention to the improvement of thermometers, and a further considerable advance was achieved. None the less, it was felt desirable to eliminate doubt on this point completely by employing the melting point of ice, 0° C., as the temperature of definition for the new standards. In practice, however, it does not prove convenient to make all comparisons, even of primary standards, at 0° C., and even at the Bureau International this is not ordinarily done, while for everyday use lengths must, of course, be standardised at normal working temperatures. It follows that a knowledge of the coefficients of thermal expansion of the bars is required. These coefficients were determined, partly by means of the Fizeau dilatometer, on small samples cut from each bar when it was made, and partly by microscope observations, in a comparator with two independent water baths, in one of which the temperature of the bar under investigation could be varied, while the other, containing a reference bar for comparison, was held at a constant temperature. In this way a series of coefficients of expansion was obtained, a slightly different coefficient being assigned to each bar. In recent years this work has been repeated with great thoroughness for a certain number of the bars, and the conclusion reached that in fact the thermal expansions of all bars of the same melt were identical within the limits of experimental error, and that it

would be better to take one mean coefficient as applying to all of them [11]. The introduction of this change in the calculations of the original observations has the effect of altering the values originally assigned to the bars at 0° C. by amounts varying between $\pm$ 0·3 μ,[1] the average change being $\pm$ 0·1 μ, and the mean change zero [12].

In addition, after a period of thirty years, commencing in 1919, a re-comparison of various national copies of the Metre was undertaken, with the result that new values were found for the twenty-five bars examined differing from the original values (after correction as above for the changes in their assumed coefficients of expansion) by amounts ranging from $-$ 0·66 μ to $+$ 0·88 μ. Incidentally the two working standards of the Bureau were found both to have changed by nearly $+$ 0·4 μ [13]. It should be remarked, however, that only nine of the bars exhibited errors exceeding $\pm$ 0·3 μ. It is clear that the larger errors found, being greater than the experimental errors of the comparisons, can only be attributed to real changes in the bars themselves, and that these changes are probably due to accidental damage, though it is somewhat surprising that so high a proportion of bars kept and handled with the care that is customarily bestowed on primary national standards of length should exhibit these changes. If, however, we exclude these bars, the average change for the remaining 16 bars is $\pm$ 0·12 μ, and the mean change only $-$ 0·06 μ. On the other hand, if the changes due to the changes in coefficient of expansion and to the re-determinations are both taken into account, the differences between the latest values and those originally accepted range from $-$ 0·62 μ to $+$ 0·97 μ, and two more bars show changes exceeding 0·3 μ, the range for the same sixteen bars being now from $-$ 0·35 μ to $+$ 0·35 μ, with an average of $\pm$ 0·18 μ and a mean of $-$ 0·03 μ. If the whole of the twenty-five bars are taken into account the average change amounts to $\pm$ 0·30 μ with a mean of $-$ 0·08 μ.

From these results we may conclude, firstly, that the precision of measurement attainable with the present standards of the Metre is of the order of $\pm$ 0·2 to 0·3 μ, an estimate which is in accordance with the present writer's opinion, based on considerable personal experience. Secondly, from the smallness of the mean changes found we may infer that the whole of the standards of the main series of metres are maintaining a high order of *relative* constancy—to within 1 part in 10[7]. As to their absolute constancy we have a further source of information which we shall now proceed to consider.

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[1] $1\mu = 0.001$ mm. $= 10^{-6}$m.

It was suggested as long ago as 1875, by Dr. B. A. Gould of Cambridge (Mass.) [14] that use might be made of the wave-length of monochromatic light from a suitable source as a natural standard of length, but it was not till nearly twenty years later that Michelson designed the interferometer which bears his name, by means of which the measurement of wave-lengths with sufficiently high precision for the purpose first became possible.   Michelson selected, as the most suitable radiation for the purpose, the red line of the cadmium spectrum, and the result of his first determination, in collaboration with Benoît, of the length of the metre in terms of the wave-length of this radiation was published in 1895 [15]. During the forty years which have since elapsed, the determination has been repeated several times by different observers, in different countries, using various types of apparatus.   The original results of all these determinations are not strictly comparable, as the conditions were not identical, nor in all cases fully specified.   But, reducing them as far as possible from the data available to a common basis, the following table has been prepared.

TABLE II

VALUES OF $Cd\lambda_R(10^{-10}m.)$

Date.		As Originally Given	After (a) Correction and (b) Adjustment to Uniform Conditions *	Difference from Mean
1895	Michelson and Benoît . . [15]	6438·4722	6438·4691	− 0·0002
1905–06	Benoît, Fabry and Perot . [16]	6438·4696	6438·4703	+ 0·0010
1927	Watanabe and Imaizumi . [17]	6438·4685	6438·4682	− 0·0011
1933	Sears and Barrell . . . [18]	6438·4711	6438·4708	+ 0·0015
1934–35	,, ,, . . . [19]	6438·4709	6438·4709	+ 0·0016
1933	Kösters and Lampe . . [20]	6438·4672	6438·4672	− 0·0021
1934–35	,, ,, . . . [19]	6438·4685	6438·4685	− 0·0008
	Mean		6438·4693	± 0·0012

* *Note.*—The values originally quoted by the authors are corrected in the fourth column to take account of subsequent conclusions (a) regarding the values to be attributed to the standards of length employed, and adjusted (b), so far as the information available permits, to uniform conditions of "normal" air—*i.e.* dry air at 15° C. and 760 mm. pressure, containing 0·03 per cent. CO_2.

It will be seen from this Table that there is no evidence of any systematic change in the length of the metre, as compared with that of the cadmium red radiation. The greatest departure of any individual determination from the mean is just under 1 part in 3 millions—equivalent to $0·3 \mu$ in the length of the metre which, as we have already seen is the same order of accuracy as is associated with the measurement of lengths in terms of the existing line standards of the Metre. It was none the less regarded as somewhat surprising that the greatest divergence yet recorded should appear between the results obtained respectively at the Physikalisch-Technische Reichsanstalt in Germany, and at The National Physical Laboratory in England, in 1933, with all modern resources ; and this accounts for the repetition of both these determinations after so short an interval. With a view to tracing the source of the discrepancy, if possible, the British and German national copies of the metre, which had served as the bases for these determinations, were both returned to the Bureau International for re-verification and direct intercomparison. Their accepted values were confirmed within $0·08 \mu$ in each case, and the accepted difference between them within $0·06 \mu$ so that the explanation does not lie here.

On the other hand it has been established that the purely optical measurements of the two laboratories are in agreement to within 2 parts in 10^8. To understand how this result has been obtained it is necessary to explain briefly the manner in which the interferometric measurements are made. Taking, for example, the N.P.L. apparatus, which is a modification of that used by Benoît, Fabry and Perot, the first step is to determine the number of wave-lengths corresponding to the distance between the semi-silvered surfaces of an " étalon " consisting of two flat parallel glass plates held at a fixed distance apart. When a pencil of convergent light is directed on to such a system interference occurs between those portions of the light which pass straight through and those portions which, after passing through the first semi-silvered surface are reflected back to it by the second, and then, after being again reflected in it, pass on through the second surface. The retardation of the reflected rays depends on the angle which they make with the common normal to the two surfaces, so that, if the emergent light is viewed in a telescope, a series of alternately light and dark rings is seen.

In order to find the number of wave-lengths contained between the two surfaces it is not necessary to make any actual count, although the method employed involves a previous knowledge of the approximate ratios of the wave-lengths of several different

monochromatic radiations, and this knowledge depends, in the last resort, on actual counts having been made for short distances on some earlier occasion. The areas of the successive rings in the pattern observed increase in an arithmetical progression, so that if their diameters are measured, and squared, it is possible by a simple calculation to find out what portion of a whole wavelength corresponds to the area of the first ring. This gives the fractional part of the whole count for that particular wave-length. To determine the whole number of waves which must be associated with the fraction thus determined the method of " coincidences " is used. With this in view the fractional parts are ascertained for a number of different wave-lengths. Then, knowing the approximate whole number for some particular wave-length from preliminary measurements, the fraction observed for this wave-length is associated with this whole number, and also with a few successive whole numbers immediately above and below it, and the resulting figures are multiplied by the known ratios of this wave-length to the others which have been used. If the wrong whole number has been adopted the fractions so obtained will not all agree with the fractions actually observed (though some may do so accidentally) but amongst the various whole numbers tried the correct whole number will be picked out by the fact that the fractions calculated in this way for all the other wave-lengths are in good agreement with those actually found for them. In this way the true whole number is identified, and when this whole number is associated with the observed fraction the count for that wavelength is complete and unambiguous. The actual double distance between the semi-silvered surfaces in the smallest étalon used in the N.P.L. apparatus corresponded to a count of something over 260,000 wave-lengths of the cadmium red line.

By a somewhat different process involving interference between light passing successively through two étalons, one of which is approximately an exact multiple of the other, the count is extended to an étalon of a little over a metre in length. Between the plates of this étalon there is then introduced a metre end-gauge, having its ends optically polished to a high degree of flatness and parallelism. The distances between these ends and the plates of the étalon are then determined by direct interference, this time in light which is reflected either immediately at the semi-silvered surface of the étalon or, after passing through it, at the end surface of the gauge. By subtracting the sum of these distances from the previously ascertained length of the étalon the length of the gauge in wavelengths is finally determined.

In order to determine the length of the metre in wave-lengths it is then still necessary to compare the length of this end-gauge with that of the line standard which represents the metre—a difficult metrological operation which it would take too long to describe here. But in order to confirm the consistency of the purely optical measurements steps were taken to make direct intercomparisons, also in wave-lengths, between the two end-standards which had themselves been directly determined in this way at the N.P.L. and the P.T.R. These intercomparisons were carried out in turn at both laboratories, the apparatus of the P.T.R. being very different from that of the N.P.L., with highly concordant results and, as recorded above, showed the optical measurements to be mutually consistent within 2 parts in 10^8. The cause of the discrepancy between the determinations of the metre in wave-lengths at the two laboratories is evidently not here. It lies probably in an accumulation of small errors in the inevitable transition stages between the two end-gauges and the national copies of the metre which, it will be remembered, are line standards. These errors will, of course, be carefully sought for but, in the very nature of the circumstances, are likely to prove elusive.

The above results are extremely interesting from two points of view. In the first place they suggest a definite probability that in the near future it may be possible, for the first time, to establish units of length in terms of definitions based on a " natural " standard—the wave-length of light—and susceptible of realisation with an accuracy some ten times greater than is at present attainable by means of arbitrary material standards. So long as the discrepancies mentioned remain unexplained there will doubtless be some reluctance to make such a change. And in any case more experimental work is still required with a view to establishing more firmly the precise terms in which such a definition should be framed. In this connection more accurate determinations of the refractive index of air are required, and also further study of alternative monochromatic radiations which might possibly be preferable to the cadmium red line, though none has so far been definitely proved superior. Work on these lines is at present being carried out in the National Laboratories.

The other point of interest is that, with such a definition we should automatically revert once more to an end-standard, instead of a line-standard, as the first material realisation of the unit. The improvement effected in recent years in the perfection of end-standards has been such that the distance between the two optically finished end surfaces of such a standard is now a more

precise measure of length than the distance between two lines read under microscopes. For purposes of practical metrology, of course, both line- and end-standards are necessary, and whichever type is employed as the starting point the difficult transference from one to the other, already referred to, is unavoidable. But with the definition in wave-lengths, and its first realisation in the form of an end-standard, the uncertainties of the transition would be associated only with line measurements, whereas at present end measurements are affected both by the initial uncertainty of the line standards and by the added uncertainty due to the transition.

It may perhaps be worth while to observe that this straining after extreme precision is not merely an academic amusement. To-day there are end-gauges sold commercially for industrial use which are guaranteed correct to $2\frac{1}{2}$ parts in 10^6. These are submitted for verification to The National Physical Laboratory, and, in order to be fair both to the purchasers and to the manufacturer it is necessary, before one of these gauges is passed or rejected, to be able to measure its length, by comparison with reference gauges whose calibration is several stages removed from the initial determinations of the fundamental standards of length to, say, one-tenth of the guaranteed limit of error. A definition of the unit by reference to the wave-length of light would enable such measurements to be made directly in terms of the definition.

It might perhaps have been expected that the determinations which have been made from time to time of the ratio of the Yard to the Metre would throw further light on the relative stability of the two standards, and hence incidentally on the absolute stability of the Yard. It happens, however, that no really dependable comparison of the present Standard Yard with the Metre was made until after the construction of the new International Metre. The first reliable comparison was made by Benoît and Chaney [21] in 1895, the result being

$$1 \text{ Metre} = 39 \cdot 370113 \text{ inches.}$$

A re-determination made by Sears, Johnson and Jolly [22] in 1928 gave the result

$$1 \text{ Metre} = 39 \cdot 370147 \text{ inches.}$$

A further result was obtained by Sears and Barrell [18] in 1933, incidentally to the determination of the lengths of both the Yard and Metre in terms of the wave-length of light, namely

$$1 \text{ Metre} = 39 \cdot 370138 \text{ inches.}$$

With regard to these three results it can only be said that the extreme range of variation is less than 1 part in 10^6, and is within

the range of the possible cumulative errors inherent in the various stages of comparison involved. To this extent they afford confirmatory evidence that the length of the Yard, as well as that of the Metre has remained stable, within the limits of accuracy, associated with the measurement of each, for the last 40 years.

The mean of the above results, 1 m. = 39·370133 inches, leads to the relation 1 inch = 25·399965 mm. In America the relationship between the Yard and the Metre is defined, by the Act of Congress of 1866, as 1 m. = 39·370000 inches. There is some controversy, into which we cannot enter here, as to the exact legal effect of this enactment, but in practice it is interpreted by an Order of the United States Treasury Department, as defining the American Yard in terms of the Metre. On this interpretation 1 inch (American) = 25·400051 mm. For most industrial purposes the difference between this and the British inch is negligible, and it is the practice in both countries, for ordinary industrial measurements, to use the simple conversion factor 1 inch = 25·4 mm. But, as we have seen, there are some cases, even in industrial usage, where the difference cannot be neglected. There is reason to believe that the United States would be prepared to vary their definition in such a way as to make the 25·4 ratio exact, if Great Britain would do likewise. The International Committee of Weights and Measures has already accepted in principle, the possibility of eventually re-defining the Metre in terms of the wave-length of light, and the Report of the Board of Trade on the last Decennial Comparisons contains a recommendation that the Yard should be similarly re-defined. If the Yard in its early existence shortened in the manner suggested on p. 217, then about the year 1882 the ratio 1 inch = 25·4 mm. would have been strictly correct. It would in any case cause no serious inconvenience at the present time, and would be a great advantage for the future if, when such new definitions are adopted for the Yard and Metre, they could be so framed that this simple ratio were exact.

Turning now to the standards of mass, we find the position with regard to these is somewhat simpler, though not entirely without its difficulties. As regards the Imperial Standard Pound, in the first place, several different pounds have had legal sanction from time to time and for different purposes, in England. Until 1878 both Troy and Avoirdupois Pounds were usually current together, but they were not always similarly defined and sometimes one and sometimes the other had precedence as the primary standard. The Avoirdupois Pound of Elizabeth, which remained the standard until 1824, was in that year superseded by the Troy

Pound, while the Avoirdupois Pound was re-defined as 7000 grains of which the Troy Pound contained 5760, this involving a definite small reduction of the former Avoirdupois Pound which, as nearly as could be determined by Lord Carysfort's Committee in 1759 from the somewhat inconsistent standards then in existence was equal to 7008 grains.   The Troy Pound constructed for Lord Carysfort's Committee became the legal standard in 1824, and by the Act of that year was to be restored, if ever lost, by reference to the weight of the cubic inch of water under specified conditions. Again, the difficulty of this operation was so great that when the Standard Pound was lost in the fire of 1834, the Committee for the restoration of the standards declared [23] that the possible error of restoring the Standard Pound by this method would be at least 2829 times as great as would be involved in making use of existing weights which had previously been compared with the lost standard with known results.

It was decided, however, that the new primary standard should be an Avoirdupois Pound, and that the Troy Pound should henceforth be defined as equal to 5760 grains of which the Avoirdupois Pound contained 7000.   The relative status of the two pounds was thus again reversed, though without altering their recently established ratio.   This position existed from 1856 to 1878 when the Troy Pound was abolished.   Since then only the Troy Ounce of 480 grains, with its decimal multiples and submultiples, have been legal for dealings in precious metals, and for coinage.

The new Imperial Standard Pound, and its Parliamentary Copies, were made of platinum, in the form of cylindrical weights of diameter slightly smaller than their height, and with a shallow circular groove near their upper surfaces to take an ivory lifting fork (Plate II, Fig. 2).   The principal difficulty in connecting them with the lost standard arose from the fact that its density was unknown, and the densities of other similar weights constructed at the same time were found to vary by as much as 3 per cent.   Since the standard is strictly one of mass, and not of weight, the recorded results of comparisons made by weighings against the lost standard in air could consequently not be relied on to restore its mass with all the accuracy of which the weighings would otherwise have been capable.   In the end resort could only be had to the comparison of a carefully dimensioned drawing of the lost standard with the existing similar weights, and assuming that its density was the same as that of the weight which it most resembled.   Actual determinations were, however, made of the densities of the new standards by weighing them in air and in water, and it is probable

that the error finally involved in the establishment of their values in relation to the lost standard did not exceed 2 parts in 10^6.

As previously mentioned, the Kilogramme was originally defined by reference to a "natural" standard—the mass of the cubic decimetre of water. The material realisation of this definition, the Kilogramme des Archives, immediately became the effective standard, and within a short time the legal standard, in place of the original definition, which it superseded. It is now known that the mass of the International Kilogramme (which may be regarded as identical with that of the Kilogramme des Archives within the limits of accuracy involved in this determination) exceeds that of the cubic decimetre of water, under the conditions of the original definition, by 0·028 grammes, an amount too great to be ignored but yet small enough to reflect great credit on the skill and care of Lefèvre-Gineau and Fabbroni, who constructed the Kilogramme des Archives in the latter part of the eighteenth century, when resources for accurate measurement were much inferior to those we now possess.

The Kilogramme des Archives, a platinum weight, remained the standard until 1889 when, in its turn, it was replaced by the International Prototype Kilogramme, a plain cylindrical weight, of height equal to its diameter, made of the same 10 per cent. iridio-platinum alloy as the International Metre (see Plate II, Fig. 3). At the time of the substitution the new International Kilogramme was declared [10] to be equal to the Kilogramme des Archives within the limits of experimental error imposed by lack of exact knowledge as to the density of the latter which, owing to the nature of the material of which it was made, it was considered unsafe to subject to hydrostatic weighings.

The comparison of standards of mass attains a higher accuracy than any other form of measurement. The precision of weighing attainable with an ordinary high-grade chemical balance is in itself surprising, considering its comparatively simple mechanical construction. But in the special balances used for the comparison of primary standards of mass refinements are embodied which lead to an improved performance which is really remarkable. Plate II, Fig. 1, shows (with its cover raised) a balance recently constructed at The National Physical Laboratory for this work, and serves to illustrate some of these special features.

In the first place the balance is installed in a closed vault, and is so designed that the whole operation of weighing, including the interchange of the weights and weight pans, can be controlled from outside the vault. The beam carries a mirror and its move-

PLATE II
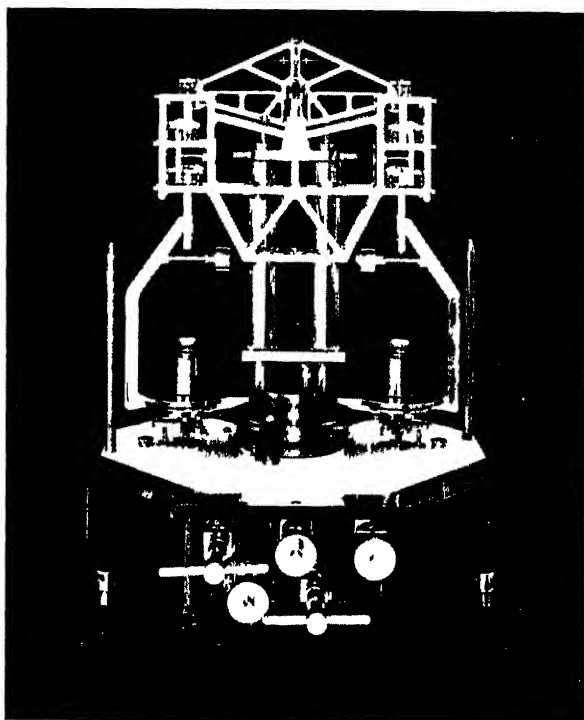

Fig 1 — Precision Balance at National Physical Laboratory
Cover removed

Fig 2 — Imperial
Standard Pound

(⅞ actual size   Actual diameter
1 15   height 1 35)

Fig 3 — British National
Copy of International Kilo-
gramme

(⅔ actual size   Actual diameter
1 53   height 1 53)

(Figs 2 and 3 are reproduced by permission of the Controller, Standards Department, Board of Trade)

ments are observed by watching the image of a cross-wire reflected by this mirror, through a glazed window in the door of the vault, on to a distant scale. To minimise wear on the weights two loose weight-pans are provided, and once the weights are placed on these they are not disturbed during the whole of one sequence of weighings. Beneath the weight-pans each pan suspension has an aperture cut in it in the form of a cross, open towards the centre of the balance, and to interchange the weight-pans on the suspensions a ring surrounding the centre of the base, and bearing two arms with suitably shaped ends which can pass through these apertures, is first raised, lifting the weight-pans off their suspensions, then rotated through 180°, and again lowered. The weighings are repeated under these alternative conditions as many times as necessary ; then the balance case is opened, the weights are interchanged on the pans and, when time has been allowed for steady conditions to be re-established, the weighings are repeated. In the mean result of all the weighings the difference in weight of the pans is thus eliminated, the weights having been set down only twice on the pans.

Another special feature to which attention must be drawn is that, instead of a single stirrup being used to hang the pan suspension on the terminal knife-edge, a system of three stirrups is employed. The first stirrup carries at its lower end a second knife-edge, parallel to the knife-edge on the beam, so that, no matter how the load is distributed on the pan this stirrup will always hang vertical, and the plane at its upper end will consequently always make contact in the same manner with the terminal knife on the beam. The second stirrup hangs by a similar plane, resting on the lower knife-edge of the first stirrup, and carries at its lower end a third knife-edge, with its direction perpendicular to the others, so that the load is always supported effectively at the same point along the length of the terminal knife on the beam, thus eliminating any variation due to the slight residual error of parallelism which must always exist, even after the most careful adjustment, between the central and terminal knife-edges. The third stirrup rests by means of a plane on the third knife-edge, and carries at its lower end a conical agate with a rounded point upon which the pan-hook is supported by means of a small spherical cup. Independent arrestments are provided for the pan-hooks, the series of stirrups, and the beam.

There are a number of other details in which the balance differs from an ordinary chemical balance, but these are subsidiary, and need not be described here. It was not found possible at first

to realise the full accuracy expected of this balance, and much experimental work has been carried out with it during the past few years. In its present condition it is capable, when used for the comparison of two kilogramme weights of similar density, of giving a series of consecutive results every one of which lies within a range of 0·01 mg., or 1 part in 10^8.

As regards the stability of our standards of mass, we have no " natural " control equivalent to the wave-length of light, to which we can turn for reference. We can only adduce such evidence as may be found in the results of repeated intercomparisons of different standards. Taking first the Pound, Table III gives the results of the successive comparisons of the Parliamentary Copies and the Imperial Standard Pound.

TABLE III

Comparison.	Difference in Thousandth of a Grain.							
	1846.	1876.	1882.	1892–94.	1902.	1912.	1922.	1933.
P.C.1–I . . .	+ 0·51	+ 2·24	—	+ 3·09	+ 3·46	+ 3·64	+ 3·88	+ 3 58
P.C.2–I . . .	− 0·89	+ 0·49	—	+ 1·51	+ 0·74	+ 0·68	+ 0 67	+ 1·06
P.C.3–I . . .	− 1·78	+ 0·13	—	+ 0·15	− 1·03	− 0·84	− 0·29	− 0·74
P.C.5–I . . .	—	—	− 1·41	(− 3·10)	− 2·84	− 2·61	− 2·58	− 2·98
Mean of P.C.1, 2 and 3	− 0·72	+ 0·95	—	+ 1·58	+ 0·86	+ 1·16	+ 1·42	+ 1·30
Mean of P.C.1, 2, 3 and 5 . . .	—	—	—	(+ 0·41)	− 0·09	+ 0·29	+ 0·56	+ 0 31

In this Table, I represents the Imperial Standard, P.C.1, 2 and 3 are the original Parliamentary Copies, all of pure platinum, and P.C.5 is the additional Parliamentary Copy made in pursuance of the Weights and Measures Act of 1878, which is of 10 per cent. iridio-platinum. The additional Parliamentary Copies of the Yard and Pound, were entrusted to the custody of the Board of Trade, one of the principal reasons for their construction being that it was considered that the Imperial Standards themselves, in the absence of such copies, were being subjected to far too much use for routine purposes. Evidence of this is seen in the apparent increase in the mean of P.C.1, 2 and 3, as compared with I, between 1846 and 1876, this being attributed [24] to an actual diminution in the mass

of I, due to wear. A similar loss, presumably due to similar causes, is exhibited by P.C.5 between the years 1882 and 1894, but since then there is no evidence of relative change amongst the weights, though the successive results are not perhaps in such good agreement as might have been expected. It must be remembered, however, that no balance of the quality described above has been available in this country until the occasion of the 1933 comparisons. The amount lost by I up to 1876 would appear to be just about 0·002 grain.

Turning to the Metric Standards, the Témoins, and the 40 National Copies, of the new International Kilogramme were all originally adjusted so as to agree with it within 1 mg. To avoid wear the International Kilogramme itself has not been used since the original comparisons with it were made. Many of the copies have, however, from time to time been re-verified by mutual intercomparisons, and the new values obtained for them, based on reference to the Témoins and working standards of the Bureau International, have been found to be in very close accord with those originally assigned to them. Of twenty-four standards so compared [25] between 1899 and 1911, only four showed any change exceeding 0·02 mg., the mean change being only − 0·004 mg., due probably to slight wear on a few of the weights. The maximum loss exhibited by any weight was 0·05 mg.

On this evidence we might be led to conclude that a very high degree of constancy had been established for the past forty years, as regards both the Pound and the Kilogramme. It is curious to note, however, that while both systems of standards have remained in such good accord amongst themselves, a *relative* change has been taking place between the two systems, as is shown in the following Table, which records the results of well established determinations of the relationship between the Pound and the Kilogramme.

TABLE IV

Date.	Value of Imperial Standard Pound in Grammes.	Compared with
1846	453·592652	Kilogramme des Archives
1883	453·592428	
1922	453·592343	International Kilogramme
1933	453·592338	

These results, when plotted, lie on a smooth curve showing a steady diminution of the Pound, as compared with the Kilogramme.

Even disregarding the earliest result, which depends on the now discarded Kilogramme des Archives, the difference of just about 5 parts in 10^7 between the results of 1883 and 1933 is more than can be attributed to experimental error. It is, of course, quite possible that a series of mass standards of similar material should remain in mutual agreement, and yet all be changing at a similar rate. As regards the Kilogrammes, experiments were made by heating samples of platinum to 350° C., no measurable loss of weight being recorded. Subsequent tests at the Bureau of Standards, Washington, indicate that even at a temperature of 900° C., the loss is negligible. Other experiments have been made at the Bureau International [26] in which one of the kilogramme standards was kept for a year in a stream of air at 100° C., without any measurable effect on its mass being detected. These results give us reasonable assurance that the Kilogramme can be regarded as a stable standard and it would therefore appear probable that it must be the Pound that has changed. That this is possible is also suggested by the fact that the metallurgical processes involved in the preparation of platinum were radically changed between the dates of construction of the original standards of the Pound, and it is quite possible that the material of these earlier standards, prepared by compression from a " mousse " of spongy platinum, may have contained some small proportion of volatile or soluble impurity, though it would normally be expected that any such impurities would be driven off when the mousse was heated to a red-heat. It is curious, moreover, that the new Parliamentary Copy, No. 5, which is made of iridio-platinum, similar to that of the international kilogramme series, should have remained in agreement with the rest of the pound series, rather than with the kilogrammes. It is possible, of course, that this is due simply to coincidence, and that slight wear of P.C.5, due to the necessity of using it more frequently than the remaining copies as a reference standard, may have accorded accidentally with a natural diminution in its companions. To this extent some small doubt still remains as to the absolute stability of our standards of mass. It is hoped to include the Kilogramme des Archives again in the re-comparisons with the International Prototype Kilogramme which it has been decided by the Committee to carry out in anticipation of the 1939 Jubilee of the establishment of the international metric standard, and this may throw interesting light on the question.

The following Table summarises the nature of the reference standards upon which our fundamental units of measurement depend, with the accuracy attainable in each case.

TABLE V

Units of	Basic Reference Standard.	Character of Reference.	Accuracy Realised.
Length	Imperial Standard Yard International Prototype Metre Wave-length of light	Arbitrary Material ,, ,, Natural phenomenon	$\pm$ 5 parts in 10^7 $\pm$ 2 parts in 10^7 $\pm$ 2 parts in 10^8
Mass	Imperial Standard Pound International Prototype Kilogramme	Arbitrary Material ,, ,,	$\pm$ 1 part in 10^8 }(accuracy of weighings)
Time	Second	Natural phenomenon	$\pm$ 1 part in 10^7 (frequency)
Electrical quantities	Electro-magnetic permeability of space	Physical constant	$\pm$ 2 parts in 10^5 (Ampere or Ohm)

As to the constancy of these units we can say with regard to the three alternative standards of length that the two material standards may now be regarded as permanently stable within the limits of accuracy to which they can be measured ; the wave-length of light is also reproducible within its corresponding limit. The standard Kilogramme is probably similarly stable and, assuming this to be the case, any future change in the present Standard Pound is not likely to exceed 15 parts in 10^8 as a maximum. It is, however, recommended in the Board of Trade Report on the Decennial Comparisons of 1933 that steps should be taken with a view to establishing an improved standard for the Pound.

As regards the second of time improved methods of measurement are even now being developed by means of which it may be hoped that the accuracy of measurement of this unit may be increased in the near future to at least 1 part in 10^8. The constancy of this unit depends on that of the natural phenomenon from which it is derived. Its rate of change is probably of the order of 1 part in 10^7 per century. Improved methods of measurement will enable us in the future to determine this with greater accuracy, and, if necessary, to re-define the unit on a more constant basis.

Finally, with regard to the electrical quantities we note that, in effect, the fourth " unit " is the only one based on a natural constant. In this way its absolute long-period invariability may be regarded as assured. But it is to be remarked that practical measurements based on this " unit " reach a much lower standard of precision than those based on material standards, whether arbitrary or natural, with the result that the actual standards of elec-

trical quantity are of a relatively low order of precision. It is not impossible that the adoption of a material standard, say, of the Ohm or Henry, in preference to the permeability of space, as the starting-point for the system of electrical quantities, would lead temporarily to a higher degree of precision. But such a standard would suffer from a disadvantage exactly comparable with that attaching to the material standard of mass—namely that there would be no means of assessing definitely the absolute stability of the unit over a long period. And, with such material standards of resistance or inductance as have so far been developed the degree of uncertainty regarding their stability would be not much less than that associated with their realisation in " absolute " measure, based on the permeability of space. It will be a matter of considerable interest to watch whether the precision of " absolute " measurements, or the stability of material electrical standards, makes the more rapid progress.

The position with regard to the electrical quantities supplies the answer to those who might ask why we have not adopted a more logical system of fundamental units, preferably all based on physical constants. The object of metrology is a severely practical one, namely to establish and maintain bases of reference, and working standards derived therefrom, whereby scientists in widely different localities, and at widely different epochs, shall be enabled to record their results in terms of quantities as nearly as possible identical. To this end those references must be chosen which afford most readily the means of precise measurement combined with adequate promise of constancy. The accuracy hitherto realised in measurements made by direct reference to physical constants— such, for example, as the velocity of light in vacuo—falls so far short of that attainable by other means that the theoretical advantage of an ideally invariable and logical basis has for the present, at any rate, to be foregone.

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THE PRESENT STATE OF OUR IGNORANCE CONCERNING THE NODULES OF LEGUMINOUS PLANTS

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1. THE IMPORTANCE OF LEGUMINOUS PLANTS

THE essential value of leguminous plants in agriculture has always been appreciated by farmers who indeed have included them in every system of agriculture. Legumes owe this importance to two qualities, their high feeding value in themselves, and the benefit which they extend to other plants grown in rotation or in association with them. The feeding value of legumes is due to their richness in protein, and to some extent to their relatively high content in mineral salts, especially calcium. The benefit which they confer on other crops is principally due to the nitrogen compounds elaborated in their root-nodules and subsequently released in the soil. The amounts of nitrogen taken from the air by various leguminous crops under field conditions have been estimated by a number of authors and found to average about 100 pounds per acre annually. A considerable portion of the combined nitrogen is left in the soil [1]. Analyses of soil under clover carried out by Shutt [2] over a period of 10 years showed that this crop enriched the soil in combined nitrogen at an average rate of about 50 pounds per acre annually. The soil enrichment thus produced by a leguminous crop may often last for several years [3] and gives special value to the inclusion of a legume break in a system of crop rotation. Legumes are extensively grown simultaneously with other crops, forming a valuable component of such mixed crops as forage mixtures and permanent grass. Here also, the nitrogen which they add to the soil must benefit the associated non-legumes. The practice of growing legumes in association with other plants is an ancient one and is much employed in primitive agriculture.

Legumes comprise an important component of natural plant associations, where the nitrogen compounds formed in their nodules help to maintain the fertility of virgin soil.

It seems likely that the evolution and spread of nodule-bearing legumes played a vital part in the geological history of the world in Tertiary times. Their spread and the consequent improvement in soil fertility and in the feeding value of the vegetation may well have contributed to the rapid evolution of mammals in the earlier Tertiary, while the herbaceous legumes in particular probably assisted the world-wide spread of grassland in the Miocene which resulted in the development of many of the modern groups of mammals.

2. THE NODULE ORGANISM OUTSIDE THE HOST PLANT

The nodule bacteria have been placed in a special genus *Rhizobium*, which is generally held to occupy a systematic position close to *B. radiobacter*.

The morphology of the nodule bacteria is complicated by the frequency with which changes in cell-shape occur. These changes, when observed in culture media, in the nodule, or in soil, seem to be associated to form a life-cycle [4]. In old cultures the majority of cells consist of small cocci; a fresh supply of suitable energy material causes these cocci to swell and to become actively motile. The motile cocci, originally described by Beijerinck under the name of " swarmers," elongate to form short, less actively motile rods, in which the stainable material is uniformly distributed. Later the rods increase in size, and lose their flagellæ, while in them the stainable material becomes segregated, first into bands, and later into definite granules. Finally, the granules are released to form the small cocci with which our description of the life-cycle began. This fundamental cycle can be observed in cultures, in laboratory media and in sterilised soil It is subject to certain modifications. Striking deviations from it have been described by Gibson [5], but more work is needed before the less common cell-types can be established as normal components of the life history. In the nodule tissue, cocci, small evenly-staining rods, and banded granular rods, usually occur at successive age-levels in a single growing nodule. In the nodule, however, the granular rod stage usually consist of swollen, pear-shaped or branched cells which are sometimes called " bacteroids," a term originally given to them by Brunchorst [6].

The life-cycle of the organism has a bearing on the spread of the bacteria through the soil, and consequently on the infection of the host plant [7]. Measurements have shown that the spread through Rothamsted soil is at a rate of about 1 inch in 24 hours. This movement through the soil does not however become detectable until the majority of the organisms have passed into the " coccus "

stage, and is thus presumably due to active migration of the flagellated "swarmers." It has been found that a small amount of calcium dihydrogen phosphate added to the soil along with the bacteria accelerates the appearance of this stage, and greatly hastens the commencement of migration. This method of hastening the spread of the bacteria through the soil now forms part of the process of seed "inoculation" for legumes that is employed in this country. The effect of the physical conditions of the soil, such as temperature and moisture, upon the migration of the bacteria offers a promising field for investigation.
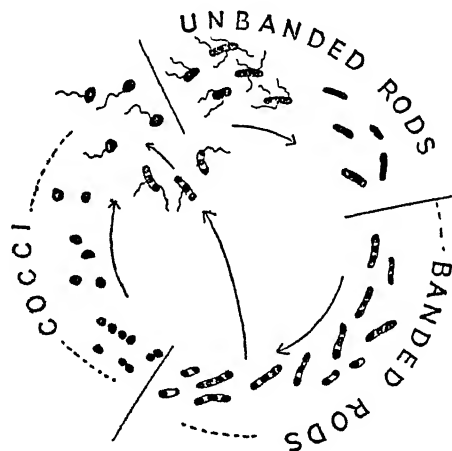
THE LIFE CYCLE OF BACILLUS RADICICOLA

Fig. 1

(From *Proc. Roy. Soc. Lond.* [7])

The reaction of nodule bacteria to soil conditions is of considerable agricultural importance, since the nature of the soil habitat not only affects their activity, but also determines whether they will multiply and maintain adequate numbers in the soil or whether they will die out.

They can tolerate a considerable range of temperature and are remarkably resistant to desiccation, so that wind-borne dust is a probable means by which they become distributed under natural conditions. Acidity is a factor frequently limiting their persistence in soil. The degree of acidity which they can tolerate is not far from that which their host plant can endure [8]. Lime exerts a beneficial action upon them, apart from its neutralising action:

the element calcium appears to be of special importance in their nutrition. The form in which the calcium is present is of importance. Albrecht and Horner [9] have found that calcium adsorbed on clay is especially beneficial, though the reason for the special activity of adsorbed calcium has yet to be ascertained.

The nodule bacteria have not been shown to utilise free nitrogen when living in the soil outside the plant : they probably take up nitrogen compounds. They can use the nitrates and ammonium salts. A variety of simple organic compounds can supply them with energy, though they are unable to attack cellulose. On agar, or in liquid media, the organisms make little growth if supplied with purely synthetic nutrients such as mineral salts and pure sugar. To make satisfactory growth they require a stimulatory substance which can be obtained from plant tissues such as legume roots, yeast, or from molasses. This substance not only increases their multiplication but also exerts a specially marked effect on respiration. It has been called Coenzyme R [10]. It can be isolated from soil and is synthesised by the soil organism *Azobacter* and probably by others.

3. The Infection of the Host Plant

In the case of lucerne the first appearance of nodules on the seedling occurs at the time when the first true leaf unfolds [11]. The solution surrounding the roots at this stage, and at later stages of growth, contains a root-secretion which stimulates the multiplication of the bacteria. Its nature has yet to be determined, but it seems likely to be the substance " Coenzyme R " which has been found inside legume roots. Nor is the seat of its formation in the plant known. Although its appearance outside the root coincides with the opening of the first true leaf, it can scarcely be formed in this leaf since the removal of the leaf as soon as it appears does not delay nodule formation.

The normal avenues of infection are the root-hairs. Straight root-hairs are not infected ; the bacteria produce a characteristic curling of the root-hair tips which is a necessary prelude to infection [12]. This curling is caused by a bacterial secretion, which is separable, by filtration, from the bacterial cells [13]. The sterile filtrate contains a thermostable active substance, which, when applied to the roots of the host plant in aseptic culture, not only produces deformation of the root-hairs, but also increases their number and their growth in length [14]. The earliest stage of infection is the formation of a small colony of the bacteria close to the apex of the root-hair. Irregular growth of the hair is then

induced, so that the end of the hair curls over to form a tight spiral. It is in the region so deformed that the bacteria enter the root-hair. The exact mode of entry is difficult to see, but it seems likely that the deformation causes a local weakening of the cell-wall.

The nodule bacteria are divisible into groups, now raised to the rank of species, each of which is normally able to infect only a limited number of host-legumes [15]. Infection by the bacteria of host plants outside their own specific groups, has been described [16], but is undoubtedly of rare occurrence. The conditions which make it possible would repay investigation. About 18 host-specific groups of *Rhizobia* have now been described, though the number is continually being increased as knowledge extends.

The nodule organisms can also be classified according to their serological reactions ; such grouping has been found to be correlated with that obtained from consideration of host specificity. The reason for this specificity is not yet known. It does not concern the preliminary curling of the root-hairs, since nodule bacteria, when placed in contact with the roots of a legume belonging to a different infection-group, are able to deform its root-hairs [13]. They cannot, however, infect such root-hairs, so that immunity must operate at the root-hair itself—after it has been deformed. Proteins of such legumes as have been tested can be classified by serological tests [17] into groups similar to those found by infection tests ; this suggests that the immunity is a protein reaction. The existence of these host-specific groups of nodule bacteria may result in a legume crop, introduced into a new district, being unable to obtain nodules owing to the absence from the soil of the appropriate nodule organism. Such deficiency can be remedied by infecting the soil with nodule bacteria of the specific group required. The organisms are most easily introduced into the soil along with the seed, this process being known as seed " inoculation."

4. Formation of the Nodule

When the bacteria have penetrated the root-hair of their host plant, they form a thread-like growth of zoogloea, known as the " infection thread," which passes down the hair and penetrates the cortex of the root. The presence of the bacteria causes the cells of the root-cortex to become meristematic, and, by division, to produce the young nodule.

This cell-division may extend inwards to involve the endodermis or even the pericycle cells. This extension into the stele may be more general than is usually supposed, since it can often be seen only in very young nodules ; later, a secondary endodermis may

wall off the base of the nodule. Penetration of the primary stele endodermis by the zone of cell-division may have an important bearing on the diffusion of nutrients into the young nodule. More detailed study of the behaviour of the endodermis in young nodules is needed ; it would make an attractive subject for a thesis.

The cause of the cell-division resulting in the formation and growth of the nodule is another problem, whose solution would have a bearing on questions outside the subject of nodule bacteria. Cell-division extends beyond the cells that are actually infected, and is perhaps due to the secretion of some diffusible stimulant by the bacteria.
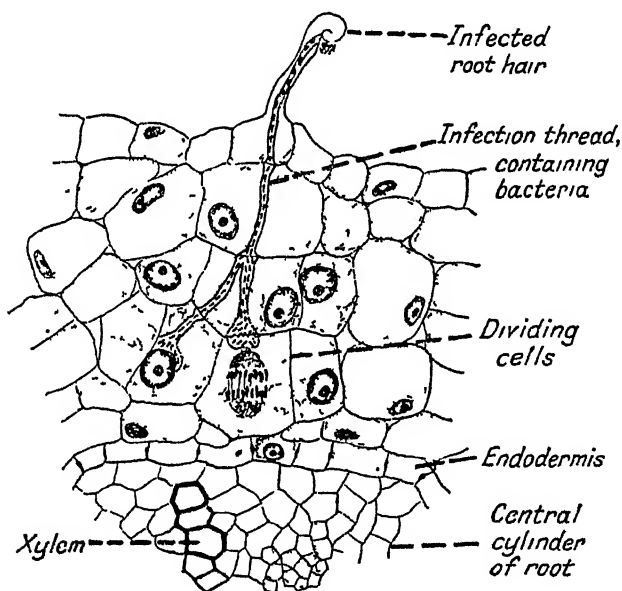

FIG. 24.—Infection of lucerne root by the nodule organism.

The method by which the bacteria are distributed through the cells of the young nodule differs in various legumes. Milovidov [18] distinguishes three types of method. In the type common to most legumes that have been studied, the bacteria spread by means of infection threads of zoogloea which pass through the cell-walls, probably utilising perforations in the cellulose, which can be demonstrated by suitable technique. The infection threads are at first naked, and in that state they apparently lose some of the bacteria by escape into the host cytoplasm. Later, those parts of the infection threads that lie within the host cells, become coated with

sheaths having the same composition as the host cell-walls [13]. Secondary release of the bacteria may take place by the formation and breaking, of blisters formed from the infection threads [19].

A second type of bacterial spread occurs in serradella (*Ornithopus sativus*). In this legume, the bacteria infect the intercellular spaces, and spread by that means. The serradella type of nodule is of special interest, since the invasion of intercellular spaces also occurs in the infection-thread type of nodule, but only when the
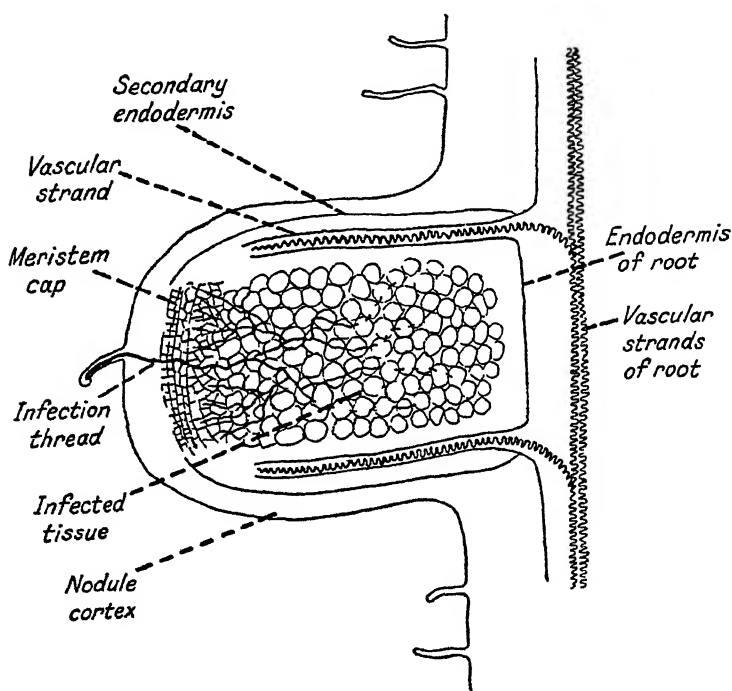
FIG. 3.—Diagrammatic section of a young nodule of *Vicia faba*.

bacteria commence to destroy the host-tissue (a phenomenon discussed below).

A third—and very interesting—type is seen in lupins. Here, the bacteria invade meristematic cells of the young nodule and, in the dividing host-cells, are carried to the poles of the mitotic spindles, and are so distributed to the daughter-cells.

In all types of nodule, the presence of bacteria in the host-cells ultimately stops their division, while permitting them to increase in size. A layer of cells, usually forming a cap at the distal end of

the nodule, normally remains uninfected, and by its continued division causes further growth of the nodule.

Active growth of this meristem seems to be essential to the healthy functioning of the nodule. It results in a continual formation of young, recently-infected tissue, owing to the invasion of its inner layers by bacteria which pass outwards from the nodule's central tissue.

The presence of the young nodule in the cortex induces an outgrowth of vascular strands from the stele. In *Medicago* and in *Vicia* the strands in a single nodule have been traced to more than one primary xylem group. In this respect the nodule differs fundamentally from a lateral root. Moreover, the strands grow out along the sides of the nodule, while a secondary endodermis is formed, surrounding each vascular strand, and enclosing the central infected tissue as far distally as the meristem cap. The cytoplasm of the infected cells in the centre of the nodule becomes closely packed with bacteria. These intracellular bacteria usually become piriform or branched, and constitute the so-called " bacteroids." The central mass of cells, most of which are densely populated with bacteria, is conveniently called the " bacterial tissue." No better instance of the poverty of our knowledge can be given than this : we are obliged to say that the bacterial tissue is *presumed* to be the seat of nitrogen fixation.

5. The Formation and Transfer of Nitrogen Compounds

Little progress has been made towards solving the problem of how nitrogen is fixed in the nodule. The nodule organisms have not been shown with certainty to be capable of fixing nitrogen outside their host plant. The host plant greatly complicates investigation of the process. For instance, the existence of both photosynthesis and respiration in the host makes it very difficult to measure how much additional energy is required to enable a nodule-bearing legume to fix nitrogen from the air. Experiments have shown that the nitrogen utilised by the nodules is absorbed from the soil atmosphere, and is not carried down from the leaves [20]. There is little doubt that the process is dependent on the active production of carbohydrate in the leaves : it can be stimulated by increasing the carbon dioxide supply [21], and is checked by darkening the plant. On the other hand, a case has been recorded of excessive insolation being detrimental to nodule activity.

The primary product of nitrogen fixation is not known. Nitrates and ammonium salts are not found in nodule-bearing legumes, unless derived from the root surroundings. Considerable amounts

of amino-acids have been recorded as present in the nodule itself [22]. Virtanen and his school have produced indirect evidence suggesting that aspartic acid and lysine are among the earliest products of fixation [23]. Great caution is, however, needed in distinguishing between the primary products of nitrogen fixation and the later products of metabolism.

There is some disagreement as to the method by which nitrogen compounds produced by the nodule bacteria are handed on to the plant. Bond [24] found, within the soybean plant/nodule system, that the rate at which nitrogen compounds were passed from the nodule into the other tissues remained fairly constant during the life of the nodules. Since disintegration of the bacteria cannot be observed in healthy nodules until the nodules are quite old, this constant flow of nitrogen compounds seems to be good evidence of nitrogen secretion by the bacteria. This view is the more probable, since the amount of combined nitrogen obtained by the plant is much greater than that contained at any one time in the bacteria within its nodules.

A similar doubt obtains about the mode of transfer of nitrogen compounds from the legume root-system into the soil. The gains in soil-nitrogen were formerly attributed wholly to root-decay of the leguminous plants, but recently it has been shown, in agar- and in pot-cultures of legumes, that nitrogen compounds appear outside the legume root-system, while the latter is still at an early stage of growth, and before any visible root-decay has taken place [25]. It is difficult to avoid the conclusion that nitrogen compounds are exuded from legume roots, or by their nodules, into the root-surroundings.

The compounds derived from nitrogen fixation—whether they are released from the legume by secretion or by root-decay—are taken up by non-legumes growing in association with the legumes [25, 26]. This associated growth may be regarded as a double symbiosis : primarily between the nodule bacteria and the host legume, and secondarily between the legume plant and other plants growing with it.

Associative growth of legumes and other plants (particularly grasses) has great agricultural importance, and is of rather special concern to this country. The survival of our agricultural industry in times of depression depends notably upon grassland. An important aspect of the problem is the effect of inorganic nitrogenous manures upon a legume/non-legume association. Nitrates and ammonium salts in ordinary dressings, though they tend to reduce or to stop nodule formation and activity, do not damage a leguminous

crop grown by itself, for legumes can, of course, obtain their nitrogen from such compounds, instead of through the activity of their nodules. Nevertheless, when mineral nitrogen is applied to a mixed crop of legumes and other plants, there is a tendency for the legumes to become swamped by competition, so that their growth may be inversely proportional to the nitrate supplied [25, 26, 27].

6. THE HOST-BACTERIA EQUILIBRIUM

Infection of the host plant, the subsequent formation of the nodule, and the proper functioning of the bacteria within it, all depend upon the maintenance of a nice physiological equilibrium between the host and the bacteria. This may be illustrated by the action of nitrate-supply to the host. Nitrates and ammonium salts exert an inhibitory action, both on infection of the root-hairs and on the growth and activity of the nodule [28].

Infection of the root-hairs is checked by nitrates and may be arrested completely by about 0·05 per cent. of sodium nitrate in the solution surrounding the roots. This inhibition is due to the nitrate protecting the root-hairs against the normal action of the bacterial secretions in deforming the hairs, this deformation being necessary to infection [12]. The action of nitrate in preventing root-hair deformation can be mitigated by the simultaneous supply of sugar to the roots, and thus appears to be connected with the carbohydrate-nitrogen balance in the root-hairs.

If lucerne seedlings, bearing young nodules, are transplanted into agar containing 0·05 to 0·2 per cent. of sodium nitrate, the nodules soon cease to grow, but develop interesting abnormalities in structure [29]. The cells of the distal meristematic cap cease to divide, and their walls become immensely thickened, while a layer of suberised cell-walls is formed along the sides of the nodule and surrounding the vascular strands. The central " bacterial tissue " is thus isolated by thickened cell-walls, and shows evident symptoms of starvation, finally becoming necrotic.

The healthy equilibrium between the bacteria and their host also breaks down in old nodules without the presence of notable amounts of nitrate being an antecedent. In such normal older nodules, necrosis commences at the base of the nodule, and gradually spreads throughout the bacterial tissue [30]. This necrosis is due to the bacteria actually attacking the tissues of the nodule, and therefore becoming parasitic upon their host. The change to parasitism may be induced, even in normal young nodules, by certain changes in the food supply to the nodule. It was first noticed in some water-culture of *Vicia faba* totally deprived of

boron [31]. The boron-deficient bean plants developed small nodules which failed to fix nitrogen. Sections of these nodules showed that the vascular strands were absent or abortive, while the bacteria were actively parasitic on the nodule tissue. It was supposed that the change to parasitism was due to the bacteria being cut off from their supply of carbohydrate, owing to the failure of the vascular strands. To test this hypothesis, well-nodulated lucerne plants were placed in the dark [30]. Similar parasitism on the part of the bacteria was produced by this method of inducing failure of the carbohydrate supply through etiolation.

It thus seems that an unhindered supply of carbohydrate to the bacteria in the nodule is essential to maintain the healthy equilibrium which leads to symbiosis. It is as yet uncertain whether the free supply of carbohydrate is needed solely to feed the bacteria, or whether adequate feeding of the meristem cap—leading to the continual formation of young nodule tissue—may not be an equally important factor.

7. Non-beneficial Strains of Nodule Bacteria

We have so far considered variations in effectiveness of the nodule that are produced by disturbances in the physiology of the host plant, whose nodules contain a strain of bacteria that are normally beneficial to it.

The efficiency of the host-bacteria association is also affected by intrinsic differences between strains of nodule bacteria. Strains isolated from the nodules of a single host plant-species or variety differ greatly in the degree of their effect upon hosts of the same species or variety. This variation is due to two causes. Firstly, different strains produce very varying numbers of nodules ; a strain may thus be relatively non-beneficial owing merely to the small number of nodules which it produces. Secondly, the amount of nitrogen fixed by the nodules varies greatly, according to the strain contained in them ; strains have been studied whose nodules cause no appreciable increase in nitrogen at all, even though growing upon an apparently healthy plant.

Strains able to infect the same group of legumes can sometimes be subdivided into several groups by serological tests, and a correlation has been found between this serological grouping and strain effectiveness [32, 33]. Differences are also found in cultural characters, but these are less clearly related to actions upon the host plant.

It is not yet understood why some strains fail to confer normal benefits of nitrogen fixation upon the host legumes. Such strains

usually produce numerous very small nodules, distributed all over the root-system, whereas effective strains produce nodules which tend to be larger, fewer, and present mostly on or near the upper part of the taproot. It has been observed in sections, that non-effective nodules are usually deficient in starch, and that their contained bacteria do not readily change into the "bacteroids" typical of the normal strains, but show a greater tendency to become parasitic upon the host tissues. These features are, however, neither universal nor confined to non-effective nodules.

The small size of non-effective nodules is apparently due to an early cessation of cell-division in the nodule meristem. This results in the nodules remaining in what is morphologically an early stage of development, in which the central "bacterial" tissue has only commenced to develop. Poor nitrogen-fixation may thus be in great measure due to the small amount of active tissue in these nodules.

Individual strains have been shown to change in their effectiveness, both in laboratory cultures and during repeated passage through the host plant [34]. The change may be in the direction of either increased or decreased effectiveness, without any evident relation to the conditions of growth of the host. The changes of "virulence" show that apparent independence of external conditions, which is characteristic of "dissociation" change.

In groups of nodule bacteria the strains of which can infect several leguminous plant species, the effectiveness of a given strain may differ according to the species of host infected. Thus, a strain found to be non-beneficial to *Trifolium pratense*, *T. repens*, and *T. hybridum*, was nevertheless effective to *T. incarnatum* [35].

The geographical distribution of non-beneficial strains has been little studied; such evidence as there is, suggests that they are not uncommon, though perhaps local. Their existence may have considerable agricultural importance, increased by the fact that different strains can compete in producing nodules on the host plant [36]. An example of this competition may be given. In some of the mountain pastures of Central Wales, clover grows very poorly, and its roots bear small nodules containing non-effective strains. One of these strains has been investigated at Rothamsted. In sand cultures of clover, the addition of this strain to the sand, together with other beneficial strains, will usually prohibit, or seriously reduce, nodule-formation by the latter. Thus the inefficient strain, in addition to being itself useless to the host, indirectly harms the latter, by preventing the entry of useful strains.

It is uncertain whether this strain-competition takes place

outside the plant, or whether the presence of nodules containing one strain may confer an immunity against other strains. Fortunately, a strain that is dominant in competition with other strains is not necessarily inefficient. Several strains have now been found that are beneficial to the clover, whilst able to compete successfully in nodule-production with the Welsh non-effective strains. Seed " inoculation " with such strains offers a prospect of improving the growth of legumes in regions where non-effective strains of their nodule bacteria predominate in the soil. A somewhat new application is thus provided for the process of legume " inoculation," hitherto used only where nodule bacteria capable of infecting the legume crop were absent or scarce in the soil.

The above outline of our knowledge of the nodule bacteria and their associations with the host legume is intended to emphasise how great and important are the gaps in this knowledge—gaps which occur at the critical point in almost every line of investigation. The nodules on legumes afford problems, whose solution would illuminate much wider fields in biology : such as those of bacterial genetics, growth-promoting substances, and the formation of pathological growths. The great mystery of biological nitrogen-fixation itself remains unsolved.

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THE MATHEMATICAL AND PHYSICAL MEANING OF PROBABILITY

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RECENT discussions [1, 2] have raised the question of what one means, precisely, by " probability," and of how sound the whole notion of " probability " may be. Can one give a description, in scientific terms, of what one means by " probability ", not only in the mathematical sense but also in a form applicable to physical problems ? It is in those senses, if in any, that " probability " has a precise meaning. The discussions referred to have raised a question which they have not answered.

1. THE MATHEMATICAL NOTION OF PROBABILITY

The most fundamental meaning of probability is perhaps the mathematical meaning. A certain class of individual events or things may be called the " universe " N, and the number of individuals in it may be denoted by the symbol (N). Suppose that the individuals are classified in accordance with some attribute A and its absence, A', and that the number of individuals with the attribute A, in N, is (A). From the universe N an individual is to be drawn at random. " Drawn (or chosen, or selected) at random " requires definition, and is here taken to mean that no one individual is to be given any preference over any other by the nature of the process of drawing. There must of course be, in any particular drawing, a preference in some sense for the individual drawn, else it would not be drawn ; but it is sufficient that the nature of the process of selection should be such as not, in general, to favour one individual more than another. Then the probability that a member of class A will be drawn is defined as the number $(A)/(N)$. As an illustration, one may consider an ordinary pack of 52 playing cards, containing four aces. One lets A denote " ace ", so that $(N) = 52$, $(A) = 4$; then if a card is to be drawn at random from the pack, the probability that it will be an ace is 1/13 by definition.

The ordinary mathematical laws of probability follow at once from this definition and from the theory of classification. Thus suppose that the universe N is classified according to attributes A and B. The probability that an individual drawn from N at random will be an A is $(A)/(N)$, that it will be a B is $(B)/(N)$, and that it will embody both attributes A and B is $(AB)/(N)$, all by definition. (AB) is the number of individuals of the sort AB (*i.e.* with both attributes) in N. Following Yule [3], one says that A and B are "independent", "positively associated", or "negatively associated" in N according to whether $(AB)/(A)$ is equal to, greater than, or less than $(B)/(N)$. Since

$$\frac{(AB)}{(N)} \equiv \frac{(A)}{(N)} \times \frac{(AB)}{(A)},$$

it follows that the probability of drawing an AB at random from N is equal to the probability of drawing an A at random from N, times the probability of drawing an AB at random from A ; this is the law of compound associated events. Likewise, if there are two independent and random draws from two universes N and M, one defines "independent and at random" to mean that no individual compound outcome n_i, m_j is to be favoured more than any other by the nature of the draws ; n_i and m_j being the general symbols for individuals in N and M, respectively. Then it is easy to show, by considering the universe $(Q) = (N)(M)$ of all possible and equally favoured individual pairs n_i, m_j, that the probability that the individual drawn from N will be an A and the individual drawn from M a B is the product of the separate probabilities. This is the law of compound independent events. By proceeding further one can prove the binomial law expressing the probability of there being exactly s successes in the course of R independent trials,

$$p(s) = p^s(1-p)^{R-s}\frac{R!}{s!(R-s)!},$$

a "trial" being a random drawing and a "success" being an individual with some specified attribute A, for which the probability is p, say, each time. From the binomial law, in turn, one deduces Bernoulli's theorem : "let ε and β be any two positive numbers, smaller than unity. Corresponding to them there exists a number n, such that when the number R of independent trials exceeds n, then the probability, that the ratio s/R of successes to trials will differ from p by more than ε, is less than β."

It is well to notice that the mathematical definition of probability has at the outset nothing at all to do with any physical

problems or observations ; the definition involves the concept of random drawings which often, in physical cases, could not practically be carried out. And a careful examination of Bernoulli's theorem shows that it really predicts nothing very definite about the actual behaviour of the ratio s/R of successes to trials, except that a certain mathematically defined probability—" the probability that $|(s/R) - p|$, after R independent trials, will be greater than ε " —approaches zero as a limit, as R approaches infinity.

2. The Extension, to Classical Physics, of the Mathematical Notion of Probability.

A wheel, mounted on a horizontal axis, carries a pointer ; the wheel is spun rapidly by hand, and allowed to come to rest under a constant frictional retardation. The angle between the pointer and the vertical, in the sense of the rotation, is denoted by θ ; what is the probability that when the pointer comes to rest it will be in the quadrant 0 to $\pi/2$? Now if one were to select at random some one quadrant from the universe of four possible quadrants, the probability that it would be the first quadrant would be 1/4, by definition ; but when the wheel is spun, one can no longer say that the probability is 1/4, for the process of selection is not now necessarily at random in the universe of the different possible outcomes. With a physical wheel, never perfect, and practically certain not to have its centre of mass on its axis, the nature of the process of selection is almost certain, depending as it does upon dynamical laws, to involve some preference for one or another of the four quadrants. The random drawing of a card from a pack can perhaps be practically achieved. The pack is thoroughly shuffled, and someone draws a card from the pack after freeing his mind, to the greatest possible extent, of all personal bias. But the spin of the wheel involves a different sort of selection, depending upon the physical properties of the wheel, and the laws of motion ; is there any " probability ", and how may one define it ?

The problem of defining the " probability " of a particular quadrant, when the wheel is spun, involves a difficulty that exists in all problems of a classical physical nature. The difficulty appears in different guises in different problems, but is always essentially of the form : the process of selection, for physical reasons, is not " at random " in the universe of all possible and different outcomes. In such cases, is there any " probability " at all, and what can one mean by it ?

Consider first the case of coincidence between the centre of mass and the axis. If I, θ, $\dot{\theta}$, and $I\eta$ are the moment of inertia,

angle of pointer to vertical, angular velocity, and frictional moment, the equation of motion is

$$\ddot{\theta} + \eta = 0,$$

whose solution is

$$\theta = \theta_0 + \dot{\theta}_0 t - \eta t^2/2,$$

where the subscripts denote initial values. When the wheel comes to rest,

$$\theta = \theta_0 + \dot{\theta}_0{}^2/2\eta.$$

For the trial to be a success,

$$2\pi r < \theta_0 + \dot{\theta}_0{}^2/2\eta < 2\pi r + \pi/2,$$

where r is any integer or zero. Consider the θ_0, $\dot{\theta}_0$ plane, between the lines $\theta_0 = 0$ and $\theta_0 = 2\pi$. Corresponding to the above inequalities, the θ_0, $\dot{\theta}_0$ plane is divided into regions of two sorts, S and F, such that if the initial point θ_0, $\dot{\theta}_0$ lies in any region S the outcome must be a success, and if in any region F a failure. The region S consists (Fig. 1) of a set of strips, and each strip is the portion of the plane between the two parabolas

$$2\pi r = \theta_0 + \dot{\theta}_0{}^2/2\eta \text{ and } 2\pi r + \pi/2 = \theta_0 + \dot{\theta}_0{}^2/2\eta,$$

and has in general the area

$$A_s = \frac{\pi^{3/2}\eta^{1/2}}{3} \{(4r + 1)^{3/2} + (4r - 4)^{3/2} - (4r - 3)^{3/2} - (4r)^{3/2}\}.$$

The whole area between the upper boundary of one strip S and the upper boundary of the next strip S is

$$A_t = \frac{\pi^{3/2}\eta^{1/2}}{3} \{(4r + 5)^{3/2} + (4r - 3)^{3/2} - 2(4r + 1)^{3/2}\},$$

and the ratio of A_s to A_t approaches 1/4 as r approaches infinity. The integer r is merely the number of complete turns of the wheel, before it comes to rest. Further, when r is large, the height (parallel to the $\dot{\theta}_0$ axis) of a strip S does not vary appreciably from $\theta = 0$ to $\theta = 2\pi$. In any region (B, say) in the θ_0, $\dot{\theta}_0$ plane, the ratio of the area of the contained region S to the total area is some constant which approaches 1/4 as r increases, and which is in any case a definite quantity that could be computed. Now let the possible initial point lie in some region B, determined by the nature of the process of starting the wheel. We may divide B into equal areas dB, small in comparison with the extensions of S and F in B; if all parts of B are equally favoured by the nature of the process of starting, so that all dB's are equally favoured, then the probability of a success exists, and equals the ratio of the area of S in B to the total area in B. If one part B_1 of B is favoured

more than another part B_2, we can consider each part separately, dividing the first into equal areas dB_1 and the second into equal areas dB_2, and we must then find very nearly the same probability of success in each part. Thus one concludes that if r is large enough, the probability of a success exists, and may be defined
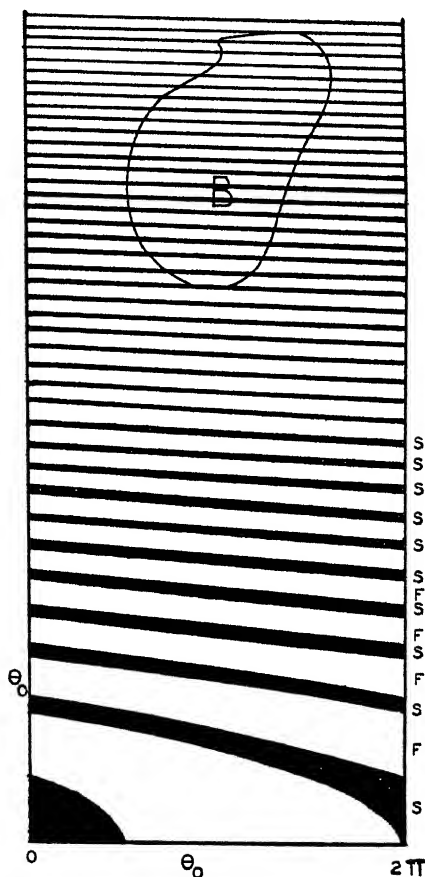

FIG. 1.—The phase space of a gaming wheel.

in terms of the universes B_1 and B_2 of equally favoured dB's, of which some are in S and some in F.

If, as is usually the case, the centre of mass is not on the axis, the same general argument applies, and the probability of a success, while no longer in general equal to 1/4, may be shown to be the ratio of the area of S to the total area, in some region B of the θ_0, $\dot{\theta}_0$ plane. The ratio will not depend upon B appreciably,

if the number of complete turns is large, and can, at least theoretic-
ally, be computed from the constants of the wheel and the appro-
priate laws of motion. The probability of success is essentially a
physical constant associated with the wheel and its friction, and
like any other physical constant exists independently of measure-
ments of it. Really, the empirical determination of the probability
of a success can be carried out nearly as easily as the determination
of the wheel's moment of inertia ; and as is usual in measurements,
the probable error of the measured probability varies (from the
binomial law) as the number of separate trials to the minus one-
half power. Similar remarks would apply to the probability that a
particular (and imperfect) die would come to rest, when cast, with a
particular face uppermost.

The preceding argument may be rendered more general and
more rigorous. The way of escape from the difficulty of defining
the " probability " of a physically selected event lies through phase
space, *via* the dynamical equations of motion of the system con-
cerned. Any state of the system (conservative or not) may be
specified by a number of co-ordinates and the corresponding
velocities, and may be described completely by a single point in a
space of $2n$ dimensions, when n is the number of degrees of freedom.
Such a space one calls a " phase space ", any point in it a " phase ",
and any possible initial point an " initial phase ". The trajectory
through any point is described completely by the Lagrangian
equations of motion

$$\frac{d}{dt}\left(\frac{\partial T}{\partial \dot{q}_r}\right) - \frac{\partial T}{\partial q_r} = Q_r$$

and thus phase space may be divided into regions of two sorts,
S and F, such that if the initial phase is in an S the result will
be a success, and if in an F a failure. The ratio of the extension
of S to the total extension, within some region B of phase space,
does not depend appreciably upon B, and thus even when the
initial conditions are selected in a fashion which prefers some parts
of B to other parts of B, the probability of a success exists and
may ideally be computed as the ratio of the extension of S to the
total extension in the region of possible initial phases. It is merely
sufficient, as is in general the case, that the favouring of different
parts of this region is on so large a scale that it ignores the fine
division of the region into parts S and F. Beyond this, the precise
nature of the favouring of initial conditions is not important, for
on it the final probability of a success does not sensibly depend.
Before the time of Poincaré, many confused and often conflicting

views were held upon the nature of probability in physics. Our own approximate discussion has proceeded along the lines laid down by M. von Smoluchowsky [4], who improved the arguments of Poincaré by a more detailed and precise analysis. For a discussion of great mathematical rigour, the reader is referred to an article by Eberhard Hopf [5].

Probability, in a still common development, is defined differently : as the limit approached by the ratio of the number of successes to the number of trials, R, as R approaches infinity. But the definition is futile, for in a physical problem no appeal to experiment can ever establish the existence of the mathematical limit, and no theoretical argument can establish it either. An appeal, with considerable reverence, is often made to Bernoulli's theorem to show that the ratio does approach a limit. But Bernoulli's theorem, as we have seen, says nothing about the ratio's approaching any limit ; it merely states (and states it only when the probability, p, of success at a single trial, exists) that a certain probability approaches a limit. If a probability is defined as a limit, then the limit must exist whenever the probability exists— but no one can prove by that circular argument that a particular physical problem leads to any " probability " at all. We have seen that mathematically defined " probability " is applicable to a physical system, and we have seen it through a consideration of the system's dynamical nature. The probabilities in classical physics are of the mathematical sort.

Often probability is held to be inconsistent with causation : but we have seen that through the dynamical constants of a physical system, and its equations of motion, probability is to be defined. If the outcome had not been determined by the initial phase, our analysis would have broken down. There is probability, in classical physics, only because there is causality. The essential points are presented clearly and generally by Poincaré [6] in the chapter on chance in *Science et Méthode*. If there is a strictly causal relation between the initial conditions and the results, and if small differences in the initial conditions produce large differences (like those between successes and failures) in the results, the results are fortuitous. Differences in the initial conditions are said to be " small " if they do not correspond to sensible changes in the probabilities of the initial conditions.

3. " PROBABILITY " IN QUANTUM MECHANICS

We have seen that in classical physics probabilities, when they exist, can be defined in the usual mathematical fashion, as the ratio

of some class frequency (A) to the total frequency (N) in some universe of equally favoured outcomes. In quantum mechanics, the notion of probability is in some respects anomalous. Following Dirac, one may denote any state of a system by a symbol ψ, and likewise by a symbol ϕ; a ϕ symbol may be multiplied by a ψ symbol to yield an ordinary complex number, $\phi\psi$. Let us normalise the symbols ϕ and ψ denoting some definite state, and then expand ϕ and ψ in terms of the eigen-ϕ's and eigen-ψ's of some real observable α :

$$\phi = \Sigma_a \phi_a \; ; \quad \psi = \Sigma_{a'} \psi_{a'}$$

where ϕ_a belongs to the eigenvalue a and $\psi_{a'}$ to a'. The ϕ_a's and $\psi_{a'}$'s are not to be normalised. Then the "probability" $P(a)$, that if an observation is made on the observable α when the system is in the state ψ the result will be a, is $\phi_a \psi_a$. The preceding rule apparently yields correct results in dynamical applications, if the real number $\phi_a \psi_a$ is interpreted as an ordinary probability, of the same character as the ratio $(A)/(N)$ in some universe N of equally favoured outcomes. The anomaly is that one is unable to trace $\phi_a \psi_a$ back to the mathematical definition by any appeal, as in classical physics, to strictly deterministic dynamical laws; yet the expressions $\phi_a \psi_a$ obey all the usual laws of probability. From the point of view of our definition of probability, which seems to be the correct and usable one in classical problems, one is really not justified in calling the "probabilities" in quantum mechanics "probabilities" at all, unless perhaps one evades the logical difficulty by saying, "A number $\phi_a \psi_a$ may be used as though it were the probability of obtaining the result a."

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THE ADULTERATION OF FOOD

By J. R. NICHOLLS, B.Sc., F.I.C.

ADULTERATION is the intentional addition to an article, for purposes of gain or deception, of any substance or substances the presence of which is not acknowledged in the name under which the article is sold. In the limiting case this may be regarded as covering the complete substitution of one article for another. Strictly the term does not include accidental contamination or impurities unintentionally present; but such may be an offence under the Food and Drugs (Adulteration) Act, 1928, which requires that foods and drugs sold shall be of "the nature, substance, and quality demanded." It is an offence under that Act to sell articles which do not conform to standards laid down in Acts and Regulations. Where no such standards exist, a magistrate may make his own standards on the evidence before him; and such evidence often comprises standards accepted in the trade or which follow custom.

The addition of one article to another is not always adulteration; but a declaration of such addition may be necessary. Where any such addition is for the purpose of concealing poor quality, or where the article added is injurious to health, an offence is committed. Similarly, if any accidental contamination is due to the article having been prepared in an unclean manner, the purchaser is prejudiced, even if the contaminating substance is not in itself harmful.

There is no widespread adulteration to-day. Eighty years ago, Arthur Hassall, a pioneer worker in the detection of adulteration, affirmed that there were few articles of consumption which were not extensively subjected to adulteration. He examined a large number of samples purchased at random, and in a book published in 1855 he records his reports on some of these. Without the more comprehensive methods of examination available to-day, he found the following numbers of adulterated samples : Bread, 49 out of 49, Coffee, 165 out of 193, Sugar 83 out of 87, Mustard 42 out of 42. In the cases of arrowroot, flour, pepper, oatmeal and milk he found at least 50 per cent. of the samples adulterated.

Whilst black tea was usually genuine, green tea was almost invariably impure. Only one type of article was wholly satisfactory and that was bitter beer. He says : ' It is very satisfactory to find a beverage of such general consumption is entirely free from every kind of impurity." It is interesting to note that he specially examined beer on account of foreign allegations that strychnine was used to give the bitter taste.

The following table shows the nature of the adulterants recorded by Hassall as being used at that time :—

Food.				Adulterants.
Bread	.	.	.	Copper sulphate, pipe clay, alum.
Flour	.	.	.	Calcium sulphate, bone earths, powdered flints.
Milk and cream		.	.	Flour, treacle, sheep's brains, chalk.
Pepper	.	.	.	Clay.
Gin	.	.	.	Cayenne.

He found the following injurious substances to have wide application : Cocculus indicus, copper arsenite, lead chromate, lead oxide, orpiment, prussian blue, bronze powders, cinnabar.

The conditions to-day are very different. The Annual Report of the Minister of Health for 1934–5 showed that in that year over 140,000 samples were examined by public analysts. Of these 7,451 or 5·3 per cent. were reported against, the corresponding figure for the previous year being 5·5 per cent. Milk was the greatest offender, 5,506 samples being either not up to standard, or containing preservative or dirt. The remaining samples covered a wide range of articles, but in many cases the offence was slight, such as an excess of water, the presence of a preservative, an excess of a permitted preservative or a deficiency in a particular ingredient. When it is remembered that the finding of an adulterated sample usually results in an intensive campaign against such articles in a number of districts, and that inspectors have become familiar with the types of articles liable to be adulterated, it can be seen that the proportion of samples reported against is an exaggerated measure of present-day adulteration. It is, of course, impossible accurately to assess its extent, but it must be very small. This state of affairs is partly due to the control exercised by public analysts and partly to the efforts of the food manufacturers and their chemists who check the purity of their raw materials and of their products to a much greater extent than the public is aware.

ANALYTICAL CONTROL

The work of the food chemist becomes more complicated every day. In addition to being a chemist, he must be an expert micro-

scopist and be something of a physicist, biologist and bacteriologist. Microscopy is a very important part of food examination, as by its means the purity of natural substances can be checked and the presence of adulterants detected. For certain substances occurring in small amounts only, *e.g.* preservatives, it may be necessary to carry out microscopic examination of crystalline precipitates or of deposits obtained by microsublimation. Increasing use has to be made of ultra-violet light and of spectroscopy.

Natural products vary very much in composition and the collection of data from a variety of sources enables the composition of any article to be compared with that of the normal product. In many cases it is only the interpretation of a series of analytical results which enables a decision as to purity to be made. Whilst much is known regarding food there is still much to be learned and unless an adulterant has characteristic properties, the recognition of the purity or otherwise of an article may rest on the interdependence of constituents determined chemically. In addition, a distinction has to be made between adulteration and poor quality. It has been aptly said that nature is a mass producer but its calipers are not applied with the precision of industrial articles.

General Forms of Adulteration

Water.—An excess of water in an article means that the public pays for water at the price of the article ; or it may be regarded from the buyer's point of view as a deficiency in weight of the article supplied. Certain foods, *e.g.* butter, have legal limits for water and in consequence it is easy in such cases to determine if an offence has been committed. In articles such as meat pastes, the consistency of the articles is due to a blend of water and fat, a deficiency in one of these being set off by an excess of the other to obtain a marketable product. In such cases it may be very difficult to determine what should be regarded as excess water. Some foodstuffs tend to deteriorate rapidly when the proportion of water exceeds a certain value.

It is illegal to add water to milk.

Ash and Sand.—All natural foods yield ash to a greater or less extent. An excess over the natural amount may be derived from added material. Such additions may be necessarily incorporated in preparing the article, *e.g.* salt or saltpetre. Sand is objectionable and usually means an inadequate cleaning of natural material.

Preservatives.—The Public Health (Preservatives, etc., in Food) Regulations prohibit the use of preservatives in food, with the exception of sulphur dioxide or benzoic acid in certain specified

articles, and in these the proportion is limited. For the purpose of the Regulations the term " preservative " does not include such substances as sugar, salt, vinegar, etc. Certain preservatives other than those allowed in this country are permitted abroad, and the use of foreign raw material may, therefore, result in an article which is considered adulterated in this country. Some preservatives disappear gradually in foodstuffs and the controlling of their use is, therefore, more difficult.

Metallic Impurities.—Certain metals may unavoidably contaminate foodstuffs. In some cases limits have been laid down or suggested for the proportion of such metals. Manufacturers must take due care to avoid contamination either by absorption during preparation or subsequently from containers. Acid material is unsuitable for keeping in tin-lined cans and the use of solder for sealing cans is to be avoided if there is a likelihood of it dissolving in the contents of the can.

Mites, Weevils, etc.—If due care is not taken in storing foodstuffs they may become infested with insects.

Staleness.—A stale or rancid article sold as fresh would involve an offence under the Food and Drugs Act.

Labelling.—Any article which is suitable for food may be offered for sale and the question of adulteration does not arise if the description of the article represents its true nature. A misdescription or a failure to disclose an essential ingredient may result in a charge. Certain foods, *e.g.* condensed milk, must be labelled in a prescribed manner and conform to certain standards.

PARTICULAR FORMS OF ADULTERATION

Bread and Flour.—Alum used to be largely used with the idea of producing a whiter and better loaf from inferior material. It is still very occasionally found. The partial substitution of other flours or starches sometimes occurs and calcium sulphate may be present as an impurity in a bread improver added. The Departmental Committee of the Ministry of Health reported in 1927 that it considered nothing should be added to flour in the way of improvers or bleachers ; but in the first instance it might suffice to limit the use to those which appeared least open to objection, such as nitrogen peroxide and persulphates for bleaching, and acid calcium and ammonium phosphates for strength. The use of chlorine, nitrogen trichloride and benzoyl peroxide was condemned. No parliamentary action has followed this Report. The detection of such treatment is, in some cases, very difficult.

Diabetic or gluten flours are often misdescribed since instead of

consisting of flours from which much of the starch has been removed, they may be ordinary flour.

Self-raising flours are flours mixed with materials which liberate carbon dioxide when dough is made. If the proportions of gas-forming ingredients are incorrect or if the flour has become damp so that reaction has occurred, the flour may be deficient in available gas. Calcium sulphate may be present, derived as an impurity from the acid constituent in the gas-forming ingredients. Also such impure material may contain appreciable proportions of lead or arsenic. Alum is sometimes used as an ingredient of the gas-forming ingredients and whilst permitted in certain parts of the world, it is regarded in this country as an adulterant.

Rice.—In order to improve the appearance of the husked grains before sale, rice is " faced " or glazed by polishing with talc. Where more than minute traces of talc remain, objection can be taken to its presence.

Pearl Barley.—Less frequently than with rice, pearl barley is faced ; in addition to talc, rice starch may be used as a facing agent.

Oatmeal.—Impure oats may contain barley, weed seeds, etc. The detection of such adulterants must be wholly microscopical and the quantitative determination of the foreign material is difficult and tedious. It usually resolves itself into the counting of particular starch grains or those of a definite size, using known mixtures as standards. Both tests and standards may be mixed with a reference material, *e.g.* lycopodium powder, to facilitate ratio counting.

Cornflower.—Tapioca, potato, and sweet potato starches are sometimes substituted in part. Rice starch has been sold as " British cornflour."

Sago.—An imitation sago is made by moulding pearl tapioca in the form of sago. Although often slightly dearer in price, it is a misdescription to call it sago.

Arrowroot.—Sweet potato starch is sometimes substituted. A mixture of potato and maize starches has been misdescribed as " British arrowroot."

Sugar.—White sugar is one of the purest products in commerce. Demarara sugar is occasionally simply white sugar coloured with a dye. Treacle may be mixed with glucose or starch sugar.

Chewing Gum may contain paraffin wax.

Jam is a conserve of fruit, boiled in mass with sugar and water. Sufficient sugar must be used to give consistency to the product and to act as a preserving agent, preventing fermentation and mould growth. Sufficient fruit must be used to give flavour and

to cause the jam to set. The setting is jointly due to the presence of sugar, pectin and acids. With the best proportions of these a minimum boiling is required, resulting in the best-flavoured product. Certain fruits are naturally deficient in pectin or acid or both, and manufacturers claim that the addition of one or other of these substances ensures the consumer receiving a better product. Whilst this may be so when a satisfactory proportion of fruit is used, it does not warrant the use of these substances to cover a deficiency of fruit, nor to conceal the use of out of condition preserved pulp or fruit which has lost practically all its pectin. The Food Manufacturers' Federation has laid down agreed standards for the amount of fruit to be used in a number of varieties of jam to warrant the use of the term " Full fruit standard." The checking of the amount of fruit used from the analysis of a sample of the finished product is a difficult matter since it involves comparison of the results of analysis with those given by the variable natural product.

Misdescription of a jam may be determined by microscopic examination but the addition of fruit juices containing no cellular fibre often escapes detection.

Wooden seeds have been alleged to have been found in raspberry jam. Gingelli seeds have also been reported and timothy, clover and alfalfa seeds are stated to be likely adulterants.

Honey.—The addition of sugar or glucose syrup can be detected by the optical properties of the honey. Artificial invert sugar cannot be detected by ordinary analysis, since the naturally occurring sugars are the same. The artificial article always contains oxymethylfurfural which gives definite colour reactions, and which has a characteristic absorption band which can be detected spectroscopically.

The botanical derivation of a honey may be deduced from the identification of the pollen grains and in the case of heather honey the phenomenon of thixotropy is evident. This enables the description to be checked and assists in preventing the sale of foreign honey as English.

Milk.—It is illegal to add water, colour, dried milk, preservatives, etc., to milk, or to remove fat. The sale of milk deficient in fat through standing is due to negligence and is an offence. The Sale of Milk Regulations lays down minimum standards for fat and for solids not fat, and milk which is sold below these limits is presumed not to be genuine. Milk below the standard is not necessarily adulterated, neither is milk above the standard necessarily genuine. A man charged with selling milk below the standard must prove that it is genuine to escape conviction. Average milk

is well above the minimum limits and it is possible to add appreciable quantities of water without bringing it below the legal minimum.   It has been found that the freezing-point of milk is its most constant property ; and it is often possible by the determination of this constant to state that water has been added, even if the sample is above the legal standards.   In certain cases the addition of water to a sample of milk may be shown by finding some substance in the sample which is present in the water and is foreign to milk.

Milk is occasionally coloured to simulate richness, the most common colours used being annatto and methyl orange.   Milk claimed to be pasteurised can be tested for phosphatase, a ferment which is present in raw milk and which hydrolyses phosphoric esters ; pasteurisation destroys this ferment.

Formaldehyde or other preservative is occasionally found. This may not have been intentionally added but may be due to contamination from fluids used to sterilize plant. Vendors of dirty milk are often prosecuted.

Skimmed milk must conform to a legal standard of solids not fat.

Condensed milk must conform to standards laid down in the Public Health (Condensed Milk) Regulations, and the label, which must be in a prescribed form, must indicate the correct equivalent of the contents of the tin in terms of a standard milk.

Cream.—Artificial cream, *i.e.* cream which has been produced by emulsification of milk powder, butter and water, must be so labelled.   Imitation creams made from vegetable oils are sometimes used in confectionery but are not offered for sale as separate commodities.

The consistency of cream is sometimes increased by the addition of thickening agents ; this is illegal. The articles used for this purpose are calcium sucrate or gelatine. No preservative is now allowed in cream, but boric acid or borax, which used to be permitted, is occasionally added.

Butter.—There is a legal maximum of 16 per cent. for water in butter.   The fat must be wholly milk-fat.   The natural variations in the usual analytical constants of milk-fat are sufficiently wide to enable a product adulterated with certain other natural fats or oils still to come within the natural range. The presence of even small amounts of vegetable fats in milk-fat may be detected by isolating the sterols and determining the melting-point of the sterol acetates after suitable fractional crystallisation. The sterol of milk-fat is cholesterol whilst in vegetable fats it is phytosterol, and the acetates of these sterols have widely differing melting-points.

Boric acid, although not allowed, is occasionally found and cane sugar and milk powder have also been added.

Margarine.—This butter substitute must be clearly marked with the word "margarine." The unusual Act of 1899 prevented the quality of margarine being too good by limiting the amount of butter fat to 10 per cent. The same water limit applies as with butter. Starch and paraffin wax have been found, but it is rarely, if ever, adulterated nowadays. If claimed to be vitamin-enriched the claim must be capable of substantiation.

Cheese.—There are no legal standards for cheese, but the article must correspond with the description under which it is sold and must contain no fat other than milk fat. Any article which is prepared in imitation of cheese and which contains fat not derived from milk must be marked "margarine cheese." Such articles are prepared by churning skim milk with an emulsion of clarified animal or vegetable fat and processing the mixture in the same way as ordinary cheese.

Oils and Fats.—The possibility of adulteration is perhaps greater with these articles than with other types. The analytical constants of any one oil are subject to wide variations and the ranges of different oils often overlap. Certain oils, however, give specific colour tests which suffice for their detection in mixtures. Salad oils have been adulterated with mineral oils. Olive oils have been mixed with cottonseed, sesame, arachis or tea-seed oil. Lard and dripping occasionally contain appreciable quantities of water. Shredded suet may contain more than the limit of rice starch accepted as allowable to prevent caking of the shreds. All edible oils should be free from rancidity or excess of acidity.

Meat.—Whole meat is little liable to adulteration on account of the familiarity of the public with its appearance. Serological tests have been used to identify horseflesh. Preservatives may occasionally be found.

Sausages may be deficient in meat, the consistency being obtained by excess of water and bread or other filler. Unsound meat is usually apparent from the smell or taste, but preservatives may tend to obscure this. Spores and bacteria liable to cause such things as botulism are usually due to accidental contamination rather than adulteration.

Fish.—Coalfish, catfish, cod or haddock have been sold as hake, and witch, megrim or brill as lemon sole.

Eggs.—Foreign eggs are required to be marked and the removal of such marks is deduced from the appearance of the shell under ultra-violet light. Preserved eggs, *i.e.* those stored in lime-water,

silicate or oil, must also be marked before sale. The process of preservation seals the pores of the shell and this can be detected by examination of a portion of the shell. Treatment of the shell with particular dyes enables the type of preservation to be determined.

Tea.—Occasionally other vegetable matter is present, but in this country it is rare to find any adulteration except excess of stalk. Sweepings, exhausted leaves and iron filings have been found. Green tea is sometimes faced to give a bright appearance.

Coffee.—The usual adulterant is chicory, but dandelion root, dried turnips, bean flour, rye flour, etc., have been used. Such mixtures would not be regarded as adulterated if not sold as coffee. Chicory itself is sometimes adulterated with other roots.

Cocoa.—Occasionally additional husk or added starch is present. Iron oxide has been used to bring up the colour of a mixture of cocoa and starch. Other fats have been incorporated to replace the valuable cocoa butter.

Spirits.—When adulteration occurs, it is usually the addition of water. There is a legal minimum for the strength of potable spirits. Rum may be an artificial product produced by flavouring plain spirits ; the real article is obtained by distillation from fermented sugar juice or molasses.

Beer.—Partly due to Excise control and partly to the care of manufacturers, beer as it leaves the brewery is never adulterated. At one time arsenical poisoning occurred through accidental contamination, but since that time strict control has rendered a recurrence practically impossible. The arsenic reached the beer through the use of arsenical fuel in the drying of the malt and hops, and through the use of sugars prepared with arsenical sulphuric acid.

Vinegar in this country is essentially a brewed article, but diluted and coloured acetic acid is sometimes sold as such. The usual forms of adulteration are a deficiency of acetic acid, the presence of lead or other impurities and a misdescription as malt vinegar. Occasionally eel worms are present.

Lemon Juice.—Wholly artificial products have been sold under this designation. Such articles contain no vitamin C (ascorbic acid), to which the antiscorbutic properties of lemon juice are due.

Spices.—Owing to inadequate cleaning, spices are liable to contain an excess of sand. Mustard often contains added starch and turmeric may be added to give a better colour. Pepper may contain an excess of husk or added starch or both. Ground olive stones have been used as an adulterant and rapeseed, linseed, lentils and palm-nut powder have also been reported. Ginger may

contain exhausted root or added starch together with turmeric. Ground mint has been found in recent years to be mixed with powdered ailanthus leaves (tree of heaven).

SUMMARY

From the foregoing it will be seen that such adulteration as exists to-day is of a relatively minor character. Manufacturers and retailers endeavour to obtain and to retain a high reputation on the quality of their products. Gross adulteration is easy of detection and the profits from small frauds are meagre and more than counterbalanced by loss of reputation through publicity if legal action is taken. The efforts of medical officers of health and sanitary inspectors who condemn unsound food, of public analysts, who check adulteration, and of reputable manufacturers who serve the public have together resulted in the nations' food being of a higher standard of purity to-day than at any stage of our history.

THE MOBILITY OF GASEOUS IONS

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WHEN the study of the mobility of gaseous ions was first under-taken in the closing years of last century one purpose was no doubt to gain thereby some information on the mass of the ions at high pressures, since the method of magnetic and electric deflection so successful in the case of electrons at low pressures was not applicable. The speed of an ion in a given gas was found over a considerable range to be proportional to the field and inversely proportional to the pressure ; and the values of mobility were for convenience, therefore, tabulated in cm./sec./volt/cm. at atmospheric pressure and room temperature.

These results were all obtained under conditions in which the speed of drift of the ions in the electric field was small compared with that of their thermal agitation. Even in the early experiments of Chattock on the pressure of the electric wind from points, historically the first method to be applied to both signs of ion [1], and in which the fields were very much higher than those of other methods the velocity of drift in air was certainly less than 10 per cent. of the thermal velocity of oxygen or nitrogen molecules.

With this limitation in drift velocity attempts were made to derive a theoretical expression for mobility in terms of the kinetic theory of gases. The most satisfactory theory in this respect was that of Langevin [2], who treated the ion and molecule as elastic spheres ; the sum of their radii, and hence the distance of closest approach, being denoted by σ. Langevin assumed moreover that the molecule would be polarised by the field of the ion, so that an attractive force would exist between them depending on their distance r apart. On simple assumptions this force is given by μr^{-5} where $\mu = \dfrac{(K-1)e^2}{2\pi N}$. K is the dielectric constant of the gas and N the number of molecules per c.c. This polarisation will not only increase the chances of a direct hit between an ion and a molecule, but will also give rise to a number of other deflec-

tions depending in magnitude on relative velocity and apsidal distance.

This work of Langevin deserves emphasis because it embodied as special cases, and indeed anticipated, several other tentative theories put forward by other workers. The expression that he derived for the mobility κ was:

$$\kappa = \frac{A}{\sqrt{\rho(K - 1)}}\left(1 + \frac{m}{M}\right)^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where ρ is the density of the gas, m and M the masses of gas molecule and ion respectively. A is a factor which is a function of a parameter λ where $\lambda^2 = \dfrac{8\pi p\sigma^4}{(K - 1)e^2}$, p being the pressure. Thus for any given ion in a given gas the value of A will depend upon the values of σ and K, in effect upon the relative importance of size and of polarisability of the gas molecules in determining the path of the ion through the gas.

When λ is large, i.e. when σ is large or the attractive field is small, the only encounters that need be considered are direct hits. On the other hand, when λ is small, it is the attractive field which is all important. Between these limits the value of A passes through a maximum value which will be referred to below in connection with experimental data.

By comparing the experimental and theoretical data the general conclusion was reached that they could only be reconciled by assuming that the mass of the ion was considerably greater than that of a single molecule and the conception of an ion as a cluster of molecules of the gas around a charged centre (atom, molecule or electron) therefore arose. On Langevin's treatment $\dfrac{m}{M}$ will then be small and σ large. Indeed, Hassé [3] on the model of a cluster of closely packed spheres made estimates of the number of molecules in a cluster which would give values of mobility in accord with experiment.

Some confirmation of the cluster view was obtained from the experimental fact that the mobility of an ion seemed to depend only on the gas in which its speed was measured and not upon the gas in which it was initially produced [4]. This would occur if the mass of the cluster is large compared with that of the initial ion on which it is formed.

But with the improvement of technique grave doubts were cast on the view that these ions were truly typical of the gases in their pure state. Experiments by Townsend [5], Lattey [6],

Franck [7], and Chattock and the writer [8], showed that with greater care in purification the mobility of the negative ion rose to high values suggesting that it was an electron around which no cluster had congregated.

Even without our present knowledge of the effects of traces of impurity in gaseous electrical phenomena and also of the order of magnitude of binding forces between atoms and ions, it could no doubt have been concluded from these experiments that the cluster, normally formed, was one of impurity molecules and not of the gas itself. That this conclusion was not immediately accepted was due to the fact that no similar effect was observed with positive ions, nor were they appreciably affected as were negative ions by the addition of further impurities such as water vapour. But later Phillips and the writer [9] showed that by adding small amounts of vapours possessing long chain polar molecules, such as those of the aliphatic alcohols, definite evidence of loading of both signs of ion were obtained, the reduction in mobility increasing with increasing length of chain and presumably therefore increasing σ and M.

By that time Erikson [10] had obtained evidence confirmed by others that the mobility of a positive ion was somewhat greater in the early stages of life (though not so great as recently observed).

The way was now clear to the conclusion that it was not that the positive ions were unaffected by impurity, but that under the normal conditions of experiment polar molecules of impurity had already become attached to them even at concentrations of impurity which were small enough to ensure a long life in the free state to the much more mobile electrons.

In order, therefore, to make a study of the mobility of positive ions, it became necessary to devise an apparatus which was entirely of glass and metal so that contamination could be reduced by vigorous heat treatment, and to exert every precaution in securing high purity of the admitted gas. At the same time it was desirable to remedy a defect in all previous methods, namely, an inadequate power of resolution of groups of ions possessing different mobilities which under certain conditions may be present together. A considerable amount of work has been done in the Wills Laboratory with a method working on the following principle [11].

In Fig. 1, A, B, C, D are two pairs of equi-distant perforated electrodes, E an electrode connected to an electrometer and S a source of ions. Steady electric fields are applied between S and A, B and C, D and E. When S is a discharging point, another electrode T is inserted. The pair of electrodes AB may be thought

of as a shutter which if opened for a short time permits a thin layer of ions drawn from the source to pass into the main field between B and C ; similarly, the pair CD acts as another shutter through which this layer can move on to the electrometer. The action of the shutter depends upon the fact that when an alternating potential is applied to its electrodes and ions are incident upon it, they can only pass through it in the advancing phase of the alternation. In this way a layer of ions enters the main field through AB once every cycle. By superposing a small steady opposing potential across the shutter, of value less than the peak voltage of the alternation, the duration of admission can be further diminished and the layer made quite narrow. After crossing the main space BC in a time depending on the field and their mobility the ions reach C. If the potential between C and D also alternates in phase with that between A and B, then, for certain values of the frequency, ions of a given mobility arrive at C at the beginning of the advancing phase of the potential. Those of them which are not caught by the electrodes pass through them to the electrometer. If the electrometer current is plotted against the frequency of the alternating potential and if the ions of only one mobility are present, peak values of the current occur at frequencies n, $2n$, $3n$ . . . corresponding to the passage of the ions across from A to C (or B to D) in 1, 2, 3 . . . periods of the alternating poten-

FIG. 1.

tial. Fig. 2 gives an actual graph for sodium ions in argon, with the first order omitted and the fourth and fifth orders replotted on a larger scale. When groups of ions differing in mobility are present, each group gives rise to its own characteristic series of peaks. To use an optical analogy the apparatus produces an " ionic spectrum," showing several " orders." The " resolving power " depends upon the sharpness of the peaks and this is determined by the narrowness of the layer which within the limits of sensitivity is under control. The resolving power increases but the electrometer current decreases with increasing " order." By suitable choice of conditions it is possible to avoid confusion due to overlapping spectra by suppressing the higher orders. On the other hand it is sometimes useful to have several orders present
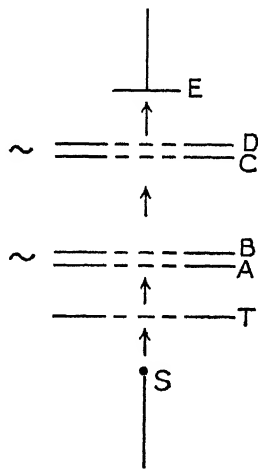
as an aid to the identification of a particular ion when several types are present together. Again it is sometimes convenient to arrange that the alternating potentials applied to the two shutters are out of phase so that the ions cross the gap in odd half-periods of the alternation.
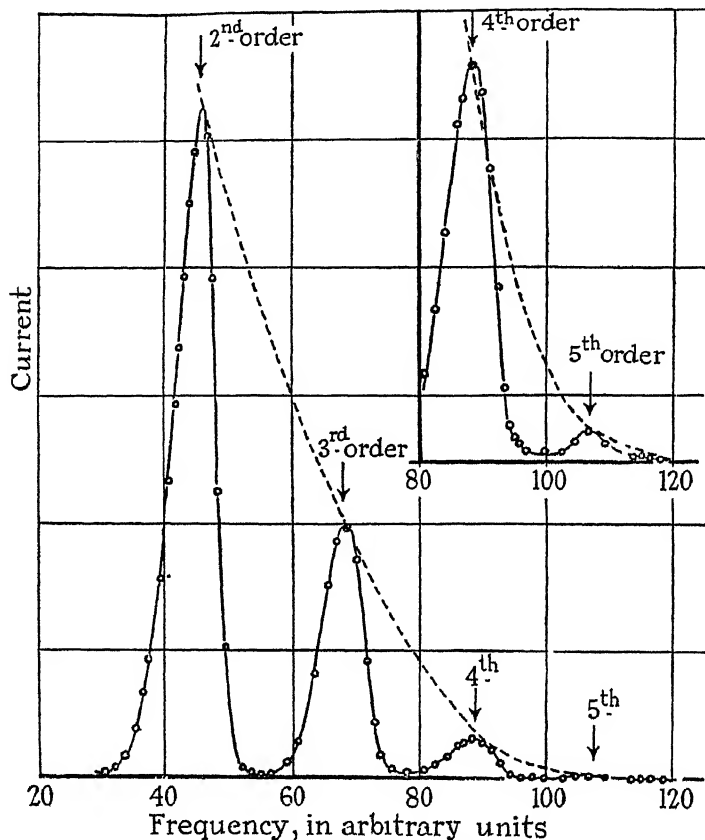

Fig. 2.

The method is strictly absolute given the values of the distance AC and the field in the intervening space. But, in practice, the part in the shutter itself is variable and this involves an end correction of doubtful magnitude. It may be shown, however, that this does not affect relative values and in a given apparatus a given peak frequency is always proportional to the mobility of the ions producing it. In order to convert it into an absolute method Powell and the writer [11] set it up in a modified form

in which the distance BC could be varied. A fixed main field and a fixed frequency were used. When the electrometer current as ordinates was plotted against the distance BC as abscissæ a series of peaks were obtained, the distance between adjacent peaks being that traversed by an ion in one period of the alternation. In this way an absolute value for helium ions in helium was obtained and this has been since used as the standard by which the simpler form of apparatus has been calibrated. As an illustration of the profound effect of removing residual impurity it may be mentioned that whereas Franck obtained the value 5·09 for the mobility of positive ions of helium in helium regarded as pure and in which the negative ions were electrons, the value found by Powell and the writer was 21·4.

Despite all precautions it is advisable to decrease the time taken by an ion in crossing the measuring apparatus and hence the probability of attachment to an impurity molecule. Most of the work has therefore been carried out at pressures of the order of a centimetre. But Mitchell and Ridler [12], have shown that for each ion in a given gas there is a critical value of the ratio of field to pressure above which the speed of the ion is no longer proportional to the pressure ; thus there is a limit below which the time of passage must not be reduced.

In the general case of the use of this method the source of the ions S was one of two forms :

(1) For the investigations of ions of the gas in itself the ions were drawn through the electrode T from a small region of point discharge. By this means it was also possible to investigate to some extent the ionic products in slightly contaminated gas. Thus, Fig. 3 shows the type of graph obtained between electrometer current and mobility for ions from point discharge in nitrogen. The single peak curve (a) of mobility 2·67 was obtained in very pure nitrogen and is believed to be due to N_2^+. Curve (b) resulted from the admission of traces of hydrogen and mercury vapour to the same sample. The small hump between two peaks of higher and lower mobility is all that is left of the N_2^+ peak. Owing to the chemical action of glow discharge one may expect traces of ammonia to be produced and this suggests that the other ions are NH_3^+ and Hg^+. The mechanism of production of these ions in such large quantities at these concentrations (e.g. saturated mercury vapour at room temperature) does not seem to be covered by a single process. Some cases of similar results obtained in helium might be explained by collisions of the second kind between metastable atoms of the gas and impurity molecules. But in

nitrogen the available energy of a metastable molecule is less than the ionisation potential of either mercury or ammonia. On the other hand if one attributes the results to direct impacts with high speed electrons in the glow discharge it is not clear why a larger proportion of ions of the gas itself are not found, since its concentration is so great compared with that of the impurity. Like many other problems in the field of glow discharge a complete solution must await the accumulation of more data under carefully controlled conditions.

(2) For the investigation of the mobility of metallic ions in a gas the source S took the form either of a Kunsman source [13] or of a base of iron oxide upon which a thin metallic layer had
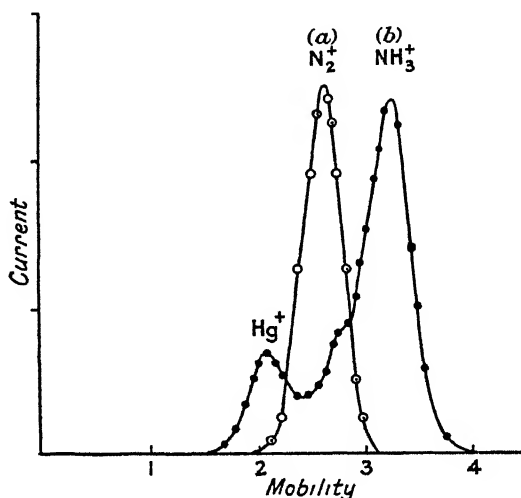
FIG. 3.

been previously deposited by evaporation from an atomic gun [14]. Iron oxide is a substance of high electronic work function. If a superficial layer of metal of the order of atomic thickness is deposited upon it and the oxide raised to a high temperature the metal evaporates. But in the case of a metal with an ionisation potential of considerably lower value than that of the work function of the oxide surface most of the metal atoms leave an electron behind them giving a beam of positive ions.[1] When more than one metal is present the ionic beam will contain an excess of the metal which has the lower ionisation potential of the two. For

[1] To avoid a rise of temperature of the gas as a whole the source must be almost completely enclosed in a water jacket.

instance, a source of sodium of ionisation potential 5·1 containing also small quantities of cæsium of ionisation potential 3·8 will act predominantly as a strong cæsium source until the surface layer is mainly depleted of this metal. In the manufacture of a Kunsman source the metal is disseminated throughout the oxide. But Powell and Mercer [15] in their examination of surface films by the positive ion method have shown how the surface concentration may be
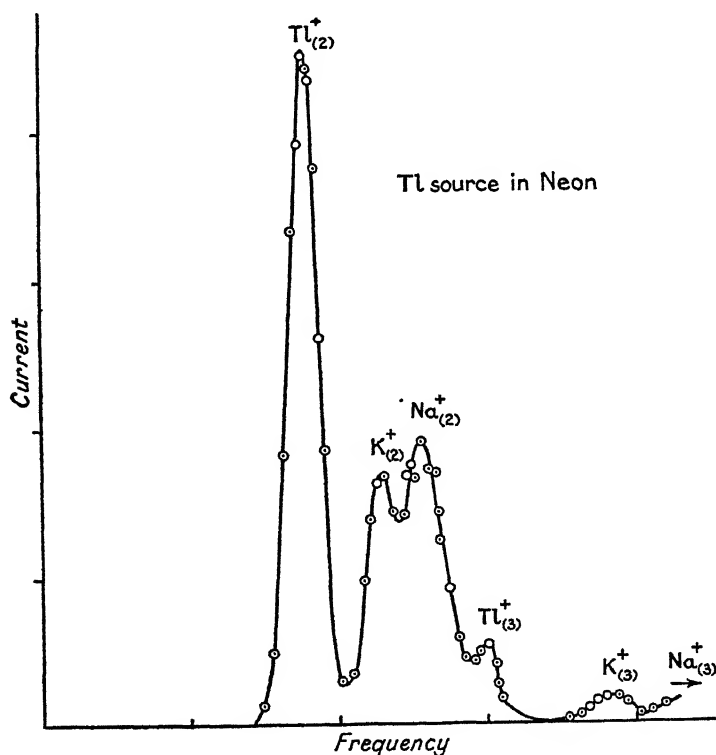
FIG. 4.

maintained by diffusion from underlying layers by the process of migration of atoms over the surfaces of the granular structure at high temperatures.

Fig. 4 gives an illustration of peaks obtained by the evaporation method by Brata [14] with a thallium source in neon. The main peak may be attributed to thallium ions in the second order. The shutters worked out of phase and the peak therefore repeats diminished in height at five thirds the frequency. The smaller peaks in the second and third orders were due to traces of sodium

and potassium present in the conical tungsten spiral in which the source was originally formed.

Since this apparatus is not directly a mass spectrograph we may ask what the grounds are for identifying the ions from mobility measurements, apart from the case of ions from a known metal.

To consider this let us take the case of nitrogen gas which has been most fully investigated by Powell, Brata, Mitchell and Ridler, and plot a graph between mobility and the mass of these monatomic metallic ions [12]. This graph is shown as a full line in Fig. 5, ten metals being included between lithium and
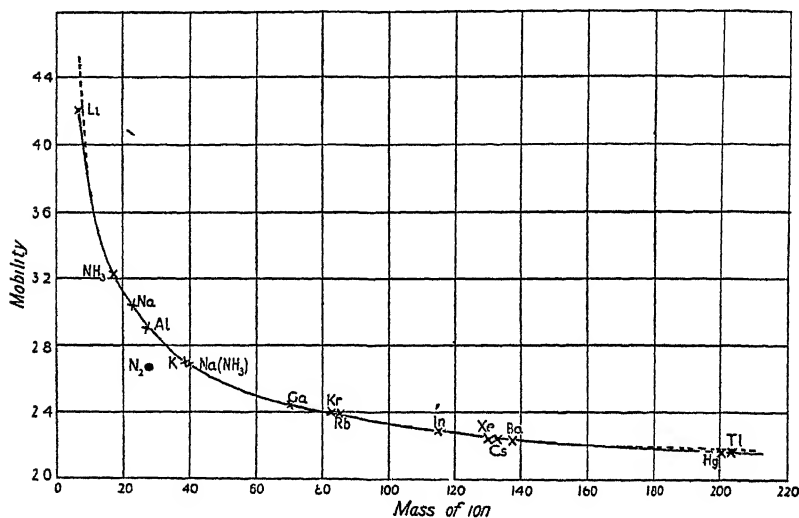
Fig. 5.

thallium. If, now, we assume that the smooth curve upon which these lie gives the relation between mobility and ionic mass for all singly charged ions, we may take the observed values of mobility obtained in other cases and read off from the graph the masses that they ought to have. If, for instance, we take the value of mobility 3·22 in Fig. 3 suspected on general grounds to be NH_3^+, we can read off from the graph that an ion of this mobility should have a mass 17, confirming our deduction. Similarly, when traces of hydrogen are present with a red hot Kunsman sodium source in nitrogen, traces of ammonia are formed by the catalytic action of the surface, as in the Haber process. In this case the experiments showed two peaks, one due to sodium and the other deduced from the graph to have a mass 40. This is

the value to be expected if some of the sodium ions attached themselves to an ammonia molecule giving an ion Na^+NH_3. At higher concentrations of hydrogen there were also ions of lower mobility present which were possibly $Na^+(NH_3)_2$ and it would be interesting to investigate in detail the growth of a cluster by an extension of experiments of this kind.

Reverting to the theoretical expression of Langevin we can calculate in some cases the mobility from published values of atomic and ionic radii [16] and other data and compare them with experiment. Some examples are shown in Table I for ions in argon :

TABLE I

Ion.	A	κ Theoretical.	κ Experimental.
Na^+	0·580	3·42	3·22
K^+	0·586	2·97	2·78
Rb^+	0·589	2·55	2·39
Cs^+	0·590	2·38	2·24

It will be seen that there is quite a fair agreement between the theoretical and experimental values of mobility. The value of A changes only slightly through the series and is practically at the maximum of the A, λ graph referred to above. For these ions then the mobility is approximately a simple function of their mass $\kappa \propto \left(1 + \dfrac{m}{M}\right)^{\frac{1}{2}}$. In Fig. 5 for nitrogen the dotted curve is plotted on the same assumption that A is constant throughout the whole range. Except at low and high values of ionic mass it will be seen that the experimental values do fit a relation of this form quite closely. This is also true for krypton, but not for the less polarisable gases neon and helium.

But there is one type of ion which does not fall under this treatment, namely, a positive ion of the gas itself. In Fig. 5, for instance, it will be seen that, whereas from the smooth curve N_2^+ should have a mobility 2·89, its observed mobility is 2·65. It is indeed the only ion which does not fall on the graph. Although this is a value nearly double that found by early experimenters the greatest care was taken to ensure that it was not brought below the curve by traces of impurity. Similarly He^+ in He has a mobility lower than would be expected from the alkali results in that gas.

This phenomenon, however, may be attributed to a breakdown

in the classical theory when applied to ions of this class. There is definite experimental evidence that when positive ions of helium, for instance, make collisions with neutral helium atoms the positive charge does not remain long associated with a given atom, but may be passed on from ion to atom at collision by the process of electron capture. The probability of capture depends upon the relative speed of atom and ion and the distance between their centres. Now this is clearly a factor not allowed for in any classical theory. On the other hand the only attempt, that of Massey and Mohr [17], to apply a wave mechanical treatment to the problem of mobility is not yet qualitatively satisfactory. But the process may be regarded as equivalent to an increase in the target area presented by the atoms to the positive helium ions and this would lead to a reduction in mobility as indeed has been found to occur.

This interchange of electrons does not happen in the case of the foreign ions investigated because these atoms have ionisation potentials less than that of the gas in which they move. For instance, sodium has an ionisation potential of 5·1 volts and argon of 15·7 volts. A sodium ion could therefore only capture an electron from an argon atom if approximately 10·6 electron volts of energy were available from another source ; the kinetic energy of the ion in the electric field is far too small to provide it. Consequently, except in the case of ions moving in their own gas, we may employ a classical treatment because, in the absence of electron interchange, the results deduced from it should not differ widely from those obtained from a wave mechanical model.

But the conception of atoms and ions as elastic spheres is an arbitrary one. Following Maxwell, Chapman studied the problem of diffusion of gases by assuming the colliding atoms to be point centres of attractive and repulsive forces each varying inversely as some power of the distance between the atoms. Lennard-Jones [18] applied this method successfully in other cases and extended it to crystals. Hassé and Cook [19] have similarly applied it to mobility assuming the force between atom and ion to be of the form $\mu_{attr.}r^{-5} - \nu_{repuls.}r^{-9}$ the attractive field term being the sum of the van der Waals and polarisation forces. He showed that this formula led to a value of mobility of He^{+} in helium of the right order of magnitude.

But there is a severer test of any model of a collision process. Among the data for his calculations Lennard-Jones used the equation of state over a wide range of temperature. Similarly in the mobility problem a knowledge of the variation of mobility with temperature is required. Some early data on the effect of temperature

recorded in the literature and referring to clustered ions cannot of course be applied and recently, therefore, the matter has been taken up in the Bristol laboratory. Data have been obtained for He^+ in helium and N_2^+ in nitrogen by A. F. Pearce and the writer [20], and for Cs^+ and Na^+ in helium by A. F. Pearce [21]. The position may be summarised as follows : Owing to the phenomenon of electron capture no comparison between theory and experiment in the case of ions in their own gas is at present possible. In the case of foreign ions a detailed analysis shows that one would expect the most marked effect of temperature with large ions and weak attractive fields. This determined the choice of cæsium ions in helium, helium being a gas of small dielectric constant and therefore small polarisability. The experiments in this case were carried out between 79° K and 492° K. The results in the upper half of the temperature range are in satisfactory agreement with the Langevin expression and in even better agreement with that used by Hassé. At low temperatures there is a departure from both expressions, but in opposite directions. This suggests that both the conception of an infinite repulsive force at a certain critical distance, and that of a repulsive field between point centres, varying inversely as the ninth power of the distance, are faulty. Since the deviations from experiment are in opposite senses it may be that agreement may ultimately be secured by using a higher power index in the repulsive field term, as indeed Lennard-Jones found in various cases in his analysis of the collisions between neutral atoms. Whether, however, with the advent of the wave mechanics it is worth while to carry out the elaborate mathematical analysis required is open to question. But even without it the whole subject of the mobility of ions may be said to be of a more satisfactory basis than was the case some years ago.

There remain other problems in mobility to which, despite their importance, only passing reference can be made. In the case of electrons which occur as the negative carriers in pure gases the term mobility has no definite meaning because the velocity of the electron is not proportional to the ratio of the field to the pressure. But readers may be referred to a general summary by Loeb [22] and to the work of Bloch and Bradbury [23] on electron attachment. Negatively charged atomic and molecular ions are found in certain cases such as low pressure discharge phenomena, but mobility data are still lacking.

Finally, in emphasising the profound effect of impurities on the nature of gaseous ions, it must not be forgotten that it is just those ions which have been ruled out from detailed study in this

article that are all important in the field of meteorology. As is well known, a study of the electrical conductivity of the atmosphere reveals at least two groups of ions, "small" and "large." The early measurements of the mobility of ions in ordinary dry laboratory air gave values of the order K_- 1·8., K_+ 1·4 ; in "moist" air K_- was lowered to about 1·6. Meteorological stations in various parts of the world have obtained values of the same order though sometimes somewhat lower for the open atmosphere. The complexity of the problem may be gauged by reading a recent summary by Hogg [24], but on present views one may assume that these ions are clusters in which water, ozone and nitric oxide molecules play a part, the different concentrations of these constituents being responsible for the variations between the values obtained at different stations and at the same station under different conditions.

The "large" ions have a very much lower mobility, of the order of 0·0003. It is now generally assumed that these may be associated with condensation nuclei, familiar in the work of Aitken, which have acquired positive and negative charges. It is suggested that these nuclei are water or hygroscopic drops of the order of diameter 10^{-6} cm. Under given steady meteorological conditions there will be a balance in the atmosphere between small ions, large ions and neutral nuclei. Nolan, Baylan and de Sachy [25] showed that with the advent of a fog the ratio of the number of large ions to small ions increases. In addition, of course, one may have charged drops produced by splashing or particles of gross matter charged sometimes by friction in generation. With the variation in size which is possible one cannot, therefore, definitely prescribe a specific value of mobility to these ions though there may be some groups of more frequent predominance.

There is a simple lecture demonstration illustrating the production of large ions, and in view of the possibility that some teachers of Physics may find a use for it, this article concludes with its description in the form in which the writer has shown it. A glass jar or beaker has a loose cover through which two electrodes pass. One of these is a well-insulated central wire connected to an electroscope, the other is earthed and supports a metal plate to which a small piece of polonium is attached. A glass tube through the cover may be used to fill the jar with cigarette smoke as required. The strength of the polonium source is such that in ordinary air the leaves of the electroscope when charged fall at an appreciable but not too rapid a rate, owing to the motion of ions produced between the electrodes. This rate is determined by the strength of the polonium source and the mobility of the

ions. If, now, the experiment is repeated with the jar filled with cigarette smoke the mobility of many of the ions is greatly lowered by their conversion into large ions by attachment and the rate of fall of the leaves is appreciably slower. The normal rate of fall may be instantly restored by lifting off the cover with its attached electrodes so that they are out of the smoke.

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THE COMPOUNDS OF THE INERT GASES

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As is well known Ramsay and his co-workers made repeated but unsuccessful attempts to combine the inert gases with other elements. The valency was therefore zero, the new elements fitted conveniently into Group 0 of the periodic table and they were expected to be chemically uninteresting. Paradoxically, however, the new elements were of the greatest importance to chemical theory, since they are the stable types to which other atoms tend to approach. The world is mainly composed, on the surface, of atoms in a state of combination, *i.e.* of atoms which have usually electronic structures on the same plan as those of the inert gases.

It would be surprising, however, if the rule of zero valency were strictly true. In the first place the inert gases have not, actually, the maximum number of electrons in their outer shells. The electronic configurations are He 2 ; Ne 2, 8 ; A 2, 8, 8 ; Kr 2, 8, 18, 8 ; X 2, 8, 18, 18, 8 ; Rn 2, 8, 18, 32, 18, 8. Since the maximum number of electrons in a quantum group n is $2n^2$, it follows that the third and higher groups in A, Kr, X and Rn are capable of expansion. The inert gases might therefore be expected to act as acceptors of electrons.

There is no obvious reason, if we accept the view that molecules co-ordinate by giving electrons to an acceptor, as in the co-ordination of NH_3 to Co in $[Co(NH_3)_6]Cl_3$, why the inert gases should not also act as donors of electrons. If, however, co-ordination proceeds by the attraction of a molecule with a dipole moment to a central ion, the likelihood of union of an ion with the inert gases is more remote. (Note that the difference between the two ways of regarding co-ordination is not as great as is apparent, since dipole attraction will involve a distortion of the electronic structures of the two atoms which combine.) But even on this view of co-ordination there is the possibility that under suitable conditions a central ion might polarise by induction an adjacent inert gas atom, sufficiently to form a compound. Naturally such compounds

would be unstable at room temperatures, and would have dissociation pressures of one atmosphere only at a low temperature, but there is no reason why, taking a broad view, they should not be classed as " chemical compounds."

Examples of the first type of co-ordination, in which the inert gas atom accepts electrons, were studied soon after the discovery of the inert gases by de Forcrand [1], who found that when a mixture of the inert gas and water was cooled under pressure crystalline hydrates appeared. The dissociation pressures are very high at 0°, except for X, which gives the most stable hydrate, with a dissociation pressure of 1·4 atmospheres at 1·4°. The heats of formation of the solid hydrates from gas and liquid water for A, Kr and X are 14,885, 14,712 and approximately 17,000 cals. The number of water molecules combined with one atom of the inert gas is given as 5·5 for A, 5 for Kr and 6–7 for X. This number is calculated from the heat of formation from liquid and solid water, and it is likely from symmetry considerations that each inert gas atom is surrounded by 6 water molecules. These structures would then fall into line with co-ordination compounds of the cobaltammine type.

The formation of hydrates receives additional support from the study of the solution of the inert gases in water. The work of Lannung [2] shows that in all cases the solubility decreases with temperature, previous work to the contrary being inaccurate. The heats of solution range from approximately zero for He to a few thousand calories for Rn, but the log of the solubility does not vary perfectly linearly with the inverse absolute temperature, and the heat of solution varies considerably with temperature. It is difficult to assess the energy changes involved. The heats of solution are however large compared with the heats of condensation of the gas to the liquid, and there is evidence here of the action of polarisation forces.

It is probable that the formation of these compounds may be represented as due to dipole attraction, in which the water dipole polarises the inert gas atom to which it is attached and a loose compound results. Probably some of the six water molecules may be arranged in pairs with opposite poles directed towards the inert gas atom, a structure which will confer stability. The polarisation of the central atom is almost entirely limited to the spherical cap nearest the polarising water dipole, since the field due to the latter varies rapidly with the distance. Thus the water molecules can polarise independently of one another.

An interesting example of co-ordination has recently been studied by Booth and Willson [3]. They find that compounds are

formed between A and BF_3 under high pressures and at low temperature. Maxima appear in the freezing point curves in the region of $-129°$, corresponding with the compounds $A(BF_3)_n$, where $n = 1$, 2, 3, 6, 8 and 16. The second of these is the most stable. Presumably the inert gas is acting as an electron donor, and the BF_3 accepts an electron pair as in the co-ordination compound $NH_3 \rightarrow BF_3$, $H_2S \rightarrow BF_3$. This accounts for the compounds for which $n = 1$, 2 and 3. In the higher members BF_3 molecules are probably linked through fluorine atoms. Booth and Willson point out that liquid BF_3 at 5–10 atmospheres becomes viscous and is probably associated. The compound $A(BF_3)_6$ may be represented $(F_3B \leftarrow F_3B)_3A$, *i.e.* a fluorine of the BF_3 molecule attached to the argon atom acts as a donor of two electrons to a second BF_3 molecule. Higher members may be similarly represented. This opens up an interesting field of research ; with xenon there is no doubt that the compounds would be much more stable.

Helium does not form compounds of the above type, as would be expected from its small size and polarisability, and stability of structure in the ground state. The atom may, however, become excited to a higher level by the absorption of 19·75 electron-volts, and then one electron is promoted to the second quantum level. In this state the atom has some resemblance to hydrogen, *e.g.* it forms a diatomic molecule He_2, present in discharge tubes and well known to spectroscopists although unobtainable in bulk. Another similar molecule is HeH. One might expect the excited helium atom to combine with the halogens, but up to the present nothing has been reported, perhaps because of experimental difficulties. But compounds have been obtained with certain metals and non-metals in weighable quantities. Manley [4] found that when helium at low pressure is contained in a tube over mercury, oscillation of the latter sets up a glow discharge, and there is a decrease in pressure. Very careful experiments, in which the greatest care was taken to ensure the purity of all materials used, showed that a gaseous mercury helide was formed. The compound is not condensed in liquid air, but forms gas-grown skins which partly accounts for the decrease in pressure. Later the helide was synthesised by a tube with external electrodes containing platinised asbestos (a tube of this type may be used as an ozoniser). The helide was decomposed by a glowing platinum wire and the mercury formed was condensed and weighed. At first the formula was reported as $HgHe_{10}$, but a second analysis using a new apparatus for synthesis and Aston's microbalance for determining density gave the formula HgHe, which is more probably $HgHe_2$ (differing

only slightly in its analysis from HgHe). It is possible that different compounds may be formed under varying conditions and that both $HgHe_{10}$ and $HgHe_2$ exist. The helide was quickly resolved into its constituents by ultra-violet light. It is interesting to notice that traces are undoubtedly formed whenever helium is used in a system containing mercury.

The preparation of several other helides has been reported. Boomer [5] found that a glowing tungsten filament in helium in a discharge tube gave off a solid black deposit which could be collected and weighed. The amount of helium combined could be calculated from the change in pressure, and the formula given was WHe_2. The compound has properties which are distinct from those of tungsten. In 20 per cent. HNO_3 or KOH the helium is liberated rapidly and the tungsten dissolves. Any excess of metal above that required for the formula WHe_2 remains unattacked. These results were probably not noticed before because the formation of the compound is sensitive to the conditions, the best results being obtained at $0 \cdot 1$–$0 \cdot 45$ mm., using 5–10 millamp. at 1,000 volts, when 5 c.mm. of He (at N.T.P.) combine per minute. Mercury vapour increases the velocity of formation.

The excited helium atoms could also be made to combine with the vapour of mercury, iodine, sulphur and phosphorus, giving compounds condensable in liquid air, with rapid disappearance of the He, the pressure of which fell from $0 \cdot 1$–$0 \cdot 2$ mm. to $0 \cdot 005$ mm. On warming up the condensate a sudden evolution of helium occurred at temperatures far below the freezing points of the vapours, *viz.* for mercury and iodine at $-70°$ and for sulphur and phosphorus at $-125°$, so that it is improbable that the helium was merely occluded. With mercury it is interesting to notice that results somewhat different from those of Manley were obtained.

Further work on similar compounds has been published by Damianovich [6] and co-workers, who find that a discharge in helium using platinum electrodes gives a platinum helide. This is a solid which is attacked by dilute aqua regia with a speed which increases with the amount of helium present. X-ray studies indicate the probability of the presence of a compound, since haloes are formed similar to those with colloidal PtS_2 and Bi_2S_3, the haloes being destroyed by heating. At $380°$ the helide reverts to its elements. The density is naturally less than for platinum. It is clear that the helide has some properties in common with the hydride, but appears to be a more definite compound.

The possibility arises that other inert gases may form compounds after excitation, but up to the present the evidence is

negative. Antropoff, Weil [7] and Frauenhof thought that they had obtained a krypton chloride by passing a discharge through krypton and chlorine and condensing in liquid air, but later work by Antropoff, Frauenhof and Krüger showed that NO had been formed by traces of air and HCl had been formed by the grease. The compound between NO and HCl, which is red, was at first thought to be the krypton chloride. From considerations of ionic radii Pauling [8] has predicted the formation of XF_6 and KrF_6 and the unstable XF_8.

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A FORGOTTEN ELECTRICIAN

By G. TOVEY

VASSILY VLADIMIROVICH PETROFF was born in the town of Oboyan, Central Russia, in 1762. He was educated in the Harkoff College and the Teachers' Seminary at St. Petersburg, from whence, in 1788, he was appointed teacher of mathematics and physics in the Mining School at Barnapool, Western Siberia.

Three years later he was recalled to the capital and appointed lecturer on mathematics at the Engineering School of the Ismailovsky Regiment. In 1793 he was invited to lecture on mathematics and physics at the Medico-Surgical College attached to the Military Hospital.

On the re-organisation of the College in 1795, into the Medico-Surgical Academy, Petroff was appointed extraordinary professor, and in this capacity he succeeded in organising an adequate laboratory, the nucleus of which was formed from a part of the collection of Count Bootoorliss, purchased by order of the Tsar. In addition, some pieces of apparatus were bought from abroad, and others were constructed after Petroff's own design.

This laboratory when completed enabled Petroff to experiment in physics, chemistry and electricity.

In 1801 he published his first book, *Collections of Physico-Chemical Experiments*, in which he quoted the names of over seven hundred scholars of all ages, from Aristotle and Pliny to Lavoisier and Boyle. This book was intended to refute the phlogistonic theories of Macquer and Scheele, and Petroff achieved the purpose with experiments on combustion, conducted with characteristic thoroughness and precision. Chapters on phosphorescence have attracted considerable interest recently, and became a subject of research of the section of physics of the Academy of Sciences. This book resulted in his appointment as full professor.

The discoveries of Galvani and Volta, and probably the premium of 70,000 francs offered by Napoleon for the discovery of the substance of electricity, influenced the production of his second book, under the complicated title : *Reports on Galvani-Volta experiments*

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conducted with particularly large battery, consisting sometimes of 4,200
discs, by Professor Vassily Petroff, 1802.

Experiments described in the book include the electrolysis of
water with a single zinc-copper pair, the production of the volta-
arc, burning metals into oxides, reducing oxides into metals and
incandescence in vacuo.

As this book was intended for the benefit of amateurs of natural
philosophy, who were deprived from a closer contact with science,
Petroff described methods of construction and using the battery
with painstaking thoroughness and extreme minuteness of detail.
As his plans called for an extraordinarily powerful battery, he
constructed one of 4,200 discs, which would have towered to a
height of forty feet if erected vertically. To avoid this inconvenience
of height, Petroff arranged the battery horizontally, deriving
independently the method described by William Cruickshank in the
Nicholson Journal (Sept. 1800).

If Petroff had heard of Cruickshank's method, he would
undoubtedly have adopted it as a simpler and more economical
way of using material, in square plates instead of discs. Haldane's
arrangement, published in the same number of the *Journal*, appar-
ently also remained unknown to Petroff, although he might have
heard of using silver and zinc, as used by Haldane, as he recom-
mended copper-zinc pairs in preference to other combinations, both
as being the cheapest and the most efficient.

Arranging his forty-foot pile, Petroff adopted the usual sequence
of zinc-copper-paper-zinc-copper—assembling the pile in a ten-foot
mahogany box, running four lines the whole length. The box and
partitions inside were covered with sealing-wax and lined with
paper soaked in vegetable oil to protect the insulation from the
corrosive action of the fluid used for the battery.

Connection of the four lines in series showed Petroff that one
disc at the end of each section served as a mere conductor and that
it might be taken off without affecting the strength of the battery.
This had not been noticed by any of his predecessors. Petroff also
discovered the effects of insulation and of the size of the cross-
section of the wire.

Contrary to Fowler's experiments on galvanism published in
1793, in which he used frog legs as electroscopes and found that
contractions were excited as readily through media of rods covered
with sealing wax as those not coated, Petroff definitely stated that
insulated wires were more effective than non-covered, and that
thicker wires were more effective than those of smaller cross-section,
thus foreshadowing Ohm's law.

Using the full strength battery, Petroff produced between two pieces of charcoal connected to the poles of the battery, the phenomenon known later as the " Volta-arc." When the pieces were brought together as close as 0·1 inch or 0·3 inch, a very bright light or flame of white colour appeared between them, from which they became, sooner or later, ignited, and with which flame a dark chamber might be brightly lighted. Taking thin metallic foil and using the same procedure, Petroff obtained a more or less bright flame. Metals were momentarily fused, and burned with a flame of characteristic colour into oxides. Reversing the process, and subjecting mixtures of oxides, powdered charcoal, tallow and oils to the action of the " Galvani-Volta fluid " he recovered metals, lead, tin and mercury.

Similar experiments were performed by Davy, using 150 4-inch plates in Cruickshank's battery, but he did not claim originality for all his experiments, admitting that the field could have been broken by foreign philosophers.

Though the experiments were the forerunners of electrometallurgy, as it was proved more than a century after, Petroff was more interested in experiments on combustion, burning paper, dry grass, straw and bits of timber in vacuo, probably still under the influence of the ideas dealt with in his first book. His experiments with the incandescence of wire were the prototype of modern electric bulbs with metallic filaments.

New Electrical Experiments dealing with the nature of frictional electricity and the application of electricity in medicine, was published in 1804. The publication was somewhat affected by the death of his wife, who " departed this life, leaving him a widower with five little daughters." A *Treatise on Physics*, containing over 1,000 pages, was completed by him in 1807. It was compiled for use in Grammar Schools, and it served its purpose as a text-book until the late 'thirties.

In 1803 Petroff was elected Corresponding Member of the Academy of Sciences and became a full member in 1807, retaining his membership until his death when his place was taken by Emil Lenz.

Acclaimed by his contemporaries as an excellent lecturer and instructive professor, he continued lecturing in the Medico-Surgical Academy, in the Academy of Arts and in the second Cadet Military School, and carried on meteorological investigations, of which he was put in charge about 1807. His articles on the evaporation of ice and snow, arrangement of lightning conductors on powder magazines, etc., were published in the *Transactions of the Academy of Sciences*, 1807–1826.

Having served in the Medico-Surgical Academy for forty years, he was retired in February 1833, and was awarded an annual pension of 5,000 roubles. He died on the 22nd July 1834.

The Council of the Academy of Sciences proposed to commemorate his work by erecting a monument over his grave ; the idea was abandoned in favour of some appropriate memorial, but nothing was done, and his name was completely forgotten until 1892, when it was associated with the building and power plant supplying the Medico-Surgical Academy. In 1922 the plant was dismantled and the building converted into a store-house, his only monument being the collection of his books in the former Imperial Public Library of Leningrad.

The centenary of his death renewed some interest in his work, and one may hope that something may now be done to commemorate his life and achievements in a suitable manner.

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By R. W. WRIGLEY, M.A., F.R.S.E., Royal Observatory, Edinburgh.

ζ AURIGÆ.—The variable star ζ Aurigæ is one of the most interesting systems in the whole sky, and it has lately been subjected to intensive observation by a large number of observatories. It consists of a giant K type star round which revolves a relatively small companion of type B, and the sun is sufficiently near to the plane of the orbit to produce the effect of an eclipsing variable. The dimensions and physical characteristics of the two stars, as well as the relative orbit, can be determined, as in the well known parallel case of Algol, by a combination of photometric and spectroscopic observations. The length of the period, 973 days, is unusual for this type of variable, and the system presents, in addition, a uniquely favourable opportunity of determining by direct methods the diameter and mass of a red super-giant star. But a further and greater interest centres in the fact that for two short periods which precede and follow the total eclipse of the smaller star, there is a phase of partial eclipse during which the light of the latter shines through the atmosphere of its giant companion, and shows, superposed on its spectrum, the absorption lines of the gases which are present there. These periods of partial eclipse are periods of constant spectral change, for the absorption lines which become visible originate from varying levels in the star's atmosphere, and naturally show a corresponding variation in the abundance and nature of the elements whose presence they indicate. If, therefore, allowance can be made for the direct light received from the K type star, the changes observed in the spectrum of the smaller star give an actual record of the constitution of the atmosphere of a giant star. ζ Aurigæ has some further exceptional advantages for this investigation. The two components are bright enough to make very long exposures unnecessary for securing spectra of moderate dispersion, while the relatively small diameter of the B type star enables it to be treated as a point source of light shining through the extensive tenuous atmosphere of its giant companion.

The orbital elements of the system were first obtained by Harper in 1924, and at the eclipse in the winter of 1931–32 the nature of the two components was demonstrated by Guthnick and Schneller. Considerable attention was therefore focussed on the 1934 eclipse, especially as the next one, in April 1937, will take place when the star will be too near the sun for favourable observation. Among the large number of papers on the subject, mention may be made of the photometric observations of Smart and Green at Cambridge (*Monthly Notices R.A.S.*, 95, No. 1) and of Oosterhoff at Mount Wilson (*Contributions*, No. 518), while spectroscopic observations are described by A. Beer (*Monthly Notices R.A.S.*, 95, No. 1) and by Christie and Wilson (*Astrophysical Journal*, 81, 426).

In the last-mentioned paper new elements for the system have been derived from a combination of photometric observations made at various stations, and they show considerable differences from those calculated by Hopmann at Leipzig in 1933. The duration of the eclipse is given as 40·0 days, totality lasting for 36·6 days and the partial phase for two periods of 1·7 days. As expressed in terms of the sun, the masses of the two components are 15·3 and 8·3 respectively, their radii are 192 and 8·5, and their densities are $2·16 \times 10^{-6}$ and 0·013. The period of rotation round its axis of the giant star is of the order of 785 days. Christie and Wilson devised a satisfactory method of eliminating the effects of the light of the giant star from measures of microphotometer tracings of the composite spectra of the two bodies obtained during the periods of partial eclipse, and they were therefore enabled to measure the total absorption of the continuous spectrum of the B star produced by the various elements present in the atmosphere of the giant. These measures have made possible the calculation of the relative numbers of atoms existing at different levels, and figures are given for the neutral metals ; Mg, Te^+, H, and Ca^+. The results are not in accordance with any one of the different theories so far formulated, for the distribution of the neutral metals seems to be according to an exponential law, while the hydrogen shell surrounding the star appears to be of uniform density. A further interesting point is that changes in the distribution of certain of the atmospheric constituents were observed between August 22 and October 1. As nothing is known regarding the rate or frequency of such changes, it cannot be definitely assumed that the atmosphere of ζ Aurigæ is typical of normal K-type giant stars.

In *Harvard Observatory Bulletin*, 900, S. Gaposchkin estimates the density gradient, density, and mass of its atmosphere from observations of the secondary effect in the light variation before

and after the eclipse. Regarding the loss of light as an extinction effect, he uses the equation

$$I_1 = I_0 e^{-\kappa \rho_1 \int ds}$$

where I_0 is the undiminished light intensity of the B star, I_1 is the intensity observed through a certain portion of the atmosphere, ρ_1 is the corresponding mean density per cubic centimetre along the optical path of which ds is an infinitesimal portion, and κ is the mass absorption coefficient. The mean density found by Christie and Wilson gives a value of 0·9 for $e^{-\kappa}$ (called the transmission coefficient). By integration over the path the mean density of the stellar atmosphere is found to be $2\cdot4 \times 10^{-14}$ gm./cm.³, while the density near the photosphere is about three times this value. The total mass of the atmosphere is $1\cdot2 \times 10^{27}$ gm., or about one-fifth of the mass of the earth.

THE MINOR PLANET ANTEROS.—For over thirty years following its discovery in 1898 Eros was regarded as the asteroid which approached most closely to the orbit of the earth, the minimum distance being 13,800,000 miles. In 1932, however, two bodies were found which approach much nearer ; the first, named Amor by its discoverer Delporte, coming within 10 million miles, and the second, found by Reinmuth at Königstuhl, approaching to less than 3 million. Unfortunately, owing to uncertainty in the determination of its period, the latter body proved too faint to be recovered, and so failed to receive a permanent number or a name. Its record has, however, now been broken, for on February 12 last Dr. Delporte at Uccle photographed a faint object of magnitude 12·5 which turned out to be nearer to the earth than any planetary body hitherto observed. Papers on its orbit by Dr. A. C. D. Crommelin appear in the *Journal of the British Astron. Assoc.*, 1936, March, April and May.

This new planet, suitably named Anteros by its discoverer, was at a distance of 5·5 million miles on February 12, but five days earlier the distance was less than 1·5 million. It was followed assiduously in Europe and America, and the last reported observation was secured with the 100-inch telescope at Mount Wilson on April 11, when the magnitude had fallen to 20·5. To have secured a photograph of so faint an object is a noteworthy achievement, and makes a record for planetary photography. The period is subject to more uncertainty than the other orbital elements, but is about 2·5 years. The eccentricity, 0·77, is very large for an asteroid and more like that of a comet, but, on the other hand, its inclination, less than 1°·5, is small. At perihelion,

which occurred on December 24, Anteros was a little distance outside the orbit of Mercury, while at aphelion it is between the orbits of Mars and Jupiter. Its orbit has two points of near approach to that of the earth, distant about a million and a million and a half miles respectively, the former point being passed by the earth in February and the latter in July. When the planetoid is also in the vicinity in July it is more favourably placed for southern observers. The small inclination of its orbit suggests a possible danger of collision with the earth, but happily the ascending node lies well within the earth's orbit and the descending node well outside it, and it is improbable that perturbations will ever reduce the inclination to zero. Dr. M. Davidson (*Observatory*, 1936, April) suggests the possible existence of a group of asteroids forming an Earth family just as the Trojan group belong to Jupiter, and he considers it conceivable that the Arizona Meteor Crater and the Siberian collision of 1908 were caused by members of this group.

By comparing the magnitude of Anteros with that of Mars when at the same distances from the earth and the sun, Crommelin calculates that, assuming equal albedoes for the two bodies, the diameter of the asteroid must be only 0·3 mile. Allowing for the fact that it has probably a rather lower albedo than Mars, it is safe to fix an upper limit of half a mile for this tiny body.

Crommelin has calculated approximate Jupiter perturbations for the revolution 1935–38, and finds that observation of Anteros should again be possible in June 1938, and July 1943, while a very close approach may be expected in February 1954.

There is as yet no sign of any slackening in the rate of discovery of new minor planets. Whereas the number found during the years 1801–1909 was 1370, the additional discoveries made between 1910 and 1934 reached a total of 3152. The time has come to consider whether some of the energy spent in searching for new bodies would not be more profitably employed in consolidating our knowledge of the older ones, for, out of these 3152 discoveries, only 610 had their orbits determined with sufficient accuracy to warrant the assigning of a number. In fact, while in 1910 nearly one-third of the newly discovered planetoids could be numbered, the orbits of 90 per cent. are now too uncertain for this to be done. The task of keeping track of these small bodies by correcting their orbital elements and computing their ephemerides has been left mainly to the Astronomisches Rechen-Institut, Berlin, and in *Astronomische Nachrichten*, No. 6107, Dr. G. Stracke gives an account of the work accomplished there during

the last 25 years, which, aided by the contributions of observers and computers in various parts of the world, has made possible the annual publication of *Kleine Planeten*. The primary object of the Institute has been to publish corrected ephemerides by which the asteroids could be identified and observed at successive oppositions. Dr. Stracke adds a valuable table comprising 1301 bodies, giving for each the year of discovery, the number of observed oppositions from 1801 to 1909 and also the number up to 1935, together with the year in which the last observation was made. Notice is drawn to 80 of the planetoids which have strayed by more than one degree from their predicted places, and 300 are marked as either having no recorded observation since 1930 or less than four oppositions in all.

This work of the Rechen-Institut has now been supplemented by an important volume compiled by Dr. A. O. Leuschner of the University of California (*Public. Lick Observatory*, XIX). Under the title "Research Surveys of the Minor Planets" it contains a summary of all the information available on 30 June 1929, concerning the bodies 1 to 1091, giving for each an account of its discovery, the earlier orbits, the work of correction as further observations became available, the residuals obtained, and a tabulation of the more important sets of elements. Leuschner states that his ultimate aim is to determine mean elements and general perturbations for each asteroid which will hold for very long periods within the limits of accuracy required by observers, and he anticipates that the Newtonian Law of Gravitation will be fully able to account for the motions. The attainment of this ideal is very desirable in the case of the brighter bodies for, as has been pointed out at the meetings of the International Astronomical Union and elsewhere, they offer considerable advantages as compared with the bright inner planets in the determination of corrections to the equator point and the equinox. For this work of fundamental importance in positional astronomy much more accurate ephemerides are required. A scheme has been drawn up at the Yale Observatory involving the observation of fourteen bright minor planets photographically at two stations over the ten years 1936–46. It is estimated that the taking of some 4000 photographs and the determination of 164 unknowns will be necessary to complete the work.

PHYSICS. By W. N. BOND, M.A., D.Sc., F.Inst.P., The University, Reading.

THE ACCURATE MEASUREMENT OF TIME.—W. de Sitter pointed out in a very interesting article in *Nature* (Supplement, Jan. 21

1928), that in choosing between the different motions that may be used for measuring time, we are guided by two considerations ; which motion can be most accurately observed, and which represents the best approximation to an isolated simple phenomenon.

In the past, considerable improvements have been made in the design of pendulum clocks and of chronometers ; but these time-keepers have all been standardised by astronomical observation. It was found, however, that the various possible astronomical methods of measuring time do not always agree. Thus, time may be measured in terms of the motion of the earth on its axis (" astronomical time "), of the moon round the earth, of the earth round the sun, and so forth ; and on comparing the different methods of measurement, curious fluctuations are detected. De Sitter considered that about three-quarters of the fluctuation in astronomical time was due to changes in the size and shape of the earth, one-quarter of the fluctuation being due to variability of the coefficient of tidal friction.

In the concluding part of his article, de Sitter discussed briefly the question as to whether mechanical clocks might ultimately be made so perfect that they would furnish as satisfactory a standard of time as any obtainable by astronomical methods. It was the Shortt or " free pendulum " clock that had suddenly made such an accuracy of time-keeping seem a possibility.

This type of clock, which is associated with the names W. H. Shortt and F. Hope-Jones, was exhibited at the British Empire Exhibition (*Handbook to the Exhibition of Pure Science*, p. 194, 1924), and has been described by Hope-Jones (*Roy. Soc. Arts., Journ.*, **72,** 446, 1924). The essential feature of the clock is a pendulum that swings freely in an evacuated vessel, except when it is given slight impulses to compensate for the frictional losses in the suspension spring. Each impulse is given by the fall of a small lever, which is raised and released by a subsidiary " slave clock." After the termination of each impulse, the lever falls further, and makes an electric contact, thus sending a signal to the slave clock. This signal is sent at a definite time after the free-pendulum has passed its central position, and is used to control the time-keeping of the slave clock. But the signal is not sent whilst the lever is in contact with the free pendulum, and therefore the swinging of the free pendulum is not affected at all by the transmission of the signal.

Shortt " free pendulum " clocks have been used at Greenwich Observatory, the National Physical Laboratory, and elsewhere. Although the pendulums were approximately compensated for temperature change, it was found necessary to keep the clocks at a

constant temperature, in order to obtain a high degree of accuracy. Also the pendulums were found to increase in length slightly with age. Apart from these difficulties, the clocks kept regular time to within a few hundredths of a second, for periods of months or years. The time-keeping was definitely more accurate than had been previously obtained. Unless a fairly extensive set of astronomical observations was made, the errors in the astronomical measurements were greater than the errors in the clocks. It was also possible, however, to test the clocks by comparison with one another and with the most accurate of the earlier types of clock.

Attempts have been made to prevent the pendulum-rod from gradually increasing in length, either by using a metal rod that had been manufactured many years previously, or by using a quartz rod. A general account of some of the important results obtained with free-pendulum clocks was given by Hope-Jones in a discourse on " Time-keeping " (*Journ. Sci. Inst.*, **10**, 43–9, 1933). The effects of certain changes in the mechanism were investigated by G. A. Tomlinson (*Proc. Phys. Soc.*, **45**, 41, 1933). In order to get frequent and precise signals from the free pendulum itself, without interfering with the freedom of its motion, Tomlinson used a photoelectric cell device. An arrangement of multiple slits, carried by the pendulum, let an appreciable amount of light pass only when these slits came opposite a similar but stationary set of slits. Each series of slits was spaced in a special non-uniform way, so that in no position of the pendulum, except its rest position, were there more than two slits in coincidence. Moreover, in the displaced positions, the two series of slits are not parallel, owing to the angular motion of the pendulum. This reduces still further the amount of light that can get through when the sets of slits are not in exact register. Tomlinson gave the result of timing 40 consecutive swings of the pendulum by the photoelectric device and a precision chronograph (described in *J. Sci. Inst.*, **8**, 21, 1931). The 40 measurements range between 1·0059 and 1·0063 seconds in value. Tomlinson also tried the effect of giving the impulses to the pendulum electrostatically, instead of by means of the small falling lever. Recently, the way in which the rate of the free pendulum depends on the angular amplitude has been investigated by E. C. Atkinson (*Proc. Phys. Soc.*, **48**, 606–25, 1936).

The free pendulum clocks, depending as they do on the gravitational attraction, will be subject to some of the changes discussed by de Sitter. A type of clock has been developed, however, which does not have this disadvantage. It has been known for a number of years that crystals of substances such as quartz and tourmaline

acquire electrostatic charges on their surface when compressed or distorted (the piezoelectric effect). Conversely, the application of positive and negative charges at appropriate parts of the surface will cause changes in the shape of the crystal. If these charges are applied periodically, a small forced oscillation is set up ; and, by adjusting the frequency of the applied e.m.f. till it is equal to one of the natural frequencies of oscillation of the specimen, resonance is obtained, the energy of the forced oscillation reaching a maximum value. The periodically varying electro-motive force can be obtained by using a thermionic valve oscillator. Moreover the oscillating crystal can be arranged to have a secondary effect on the oscillations in the valve circuit, so that the frequency of the whole system is controlled by the crystal, being almost exactly equal to one of the natural frequencies of the crystal. The control of a frequency by the oscillations of a piezoelectric crystal was developed by W. G. Cady (*Proc. Inst. Radio Engineers*, 10, 83, 1922). Descriptions of the method are given by E. B. Moulin (*Radio Frequency Measurements*, p. 215, 1931), and in the *Handbuch der Physik* of H. Geiger and K. Scheel (13, 322–31, 1928).

Piezoelectric clocks have been constructed and they have been found to give an extremely accurate method of measuring time. As an example we may consider work that has been carried out at the Reichsanstalt. A. Scheibe and U. Adelsberger (*Phys. Zeits.* 33, 835, 1932) described a clock consisting of an oscillator of frequency 60,000 cycles per second controlled by the piezoelectric vibrations of a quartz crystal. The crystal was thermostatically controlled, in order to keep its natural frequency constant. The 60,000 frequency was stepped down in three stages, each frequency being a sub-multiple of that from which it was derived. The final frequency of 333 per second was used to drive a synchronous motor, which discharged a condenser every 4·5 or 9 seconds. This condenser discharge was used to operate an automatic recorder.

Subsequent work at the Reichsanstalt is described by Scheibe and Adelsberger in later papers (*Ann. d. Physik*, 18 (1), 1–25, 1933 ; *Phys. Zeits.*, 37, 185–203, March 15 1936). The last of these papers describes the comparison of three quartz clocks with the mean astronomical time provided by three observatories, during the period Jan. 1933 to June 1935. The quartz clocks are found to be more consistent with one another than with the astronomical time. In particular in June 1934 all three clocks showed an increase in " rate " of about 0·004 seconds per day, relative to astronomical time. It is concluded that the variations are due to changes in the length of an astronomical day, the length of the day having rapidly

decreased by 0·004 seconds during June–July 1934, owing to an increase in the angular velocity of the earth of about one part in 22 million.

This supposed change in the length of the day by about 0·004 seconds may be compared with the sudden changes postulated by de Sitter (*Nature*, Jan. 21 1928), the seven of which range in value from + 0·003 to − 0·003 seconds per day.

Recent changes in the definition of time are discussed by R. v. d. R. Woolley in a paper on Nutation, Longitude and Wireless Time Signals (*Observatory*, **59**, 164–8, May 1936) ; and an account is given by H. T. Stetson (*Terrestrial Magnetism*, **41**, 287–297, Sept. 1936) of the apparent dependence of the velocity of radio time-signals on the region of the earth that they have traversed.

In connection with piezoelectric oscillators, the question of the effect of temperature is of importance. Three recent papers have appeared on this subject (A. Langevin, *J. de. Physique et le Radium*, **7**, 95, 1936 ; A. Pitt and D. W. R. McKinley, *Canad. J. of Research*, **14**, Sect. A., 57, 1936 ; I. Koga, *Proc. Inst. Radio Engineers*, **24**, 510, 1936). One considerably earlier paper may also be mentioned, on the Adiabatic and isothermal piezoelectric constants of tourmaline (D. A. Keys, *Phil. Mag.*, **46**, 999, 1923).

MAGNETISM AND HEAT.—The amount of heat emitted or absorbed during the magnetisation of specimens of ferromagnetic substance has been extensively investigated by T. Okamura (*Sci. Reports*, Tôhoku University, **24**, 745–807, 1936). When ferromagnetic material is subject to a number of cycles of magnetisation in succession, the total heat emitted may be considerable. The amount of heat emitted during a small portion of a single cycle is, however, quite small. In Okamura's experiments, a sensitive thermo-couple system was used, which was capable of detecting a change in temperature of $10^{-6}°$ C. Bars of ferromagnetic material and of German silver, to which the thermo-couples were attached, were hung inside a Dewar vessel, 70 cm. long, capped by a smaller Dewar vessel. The magnetising coils were outside the Dewar vessel, and surrounded by water cooling systems, maintained constant to about $\pm\ 0·1°$ C. The cooling of the specimen was almost entirely due to conduction along the wires from the thermo-couples, and the necessary cooling correction was determined experimentally. Heating due to Eddy currents was shown to be insignificant.

The heat production is equal to the sum of reversible and irreversible magneto-caloric effects. Certain simple results of the Honda-Ôkubo theory of ferromagnetism are used to enable the heating due to each of the effects to be deduced from the measure-

ments. The values obtained for the reversible effect were found to be in good agreement with values deduced thermodynamically from the measured change of magnetisation with temperature.

The results of the experiments (on iron, nickel, cobalt, steel, alloys and on single crystals of iron) can only be briefly indicated. As the specimen is magnetised, there is irreversible emission of heat, at first slowly, then rapidly, and finally slowly again. On decreasing the field to zero there is little irreversible emission. Subsequent increase of field in the reverse direction causes an irreversible emission, which is most rapid where the rate of change of magnetisation is largest. The reversible effect, on first magnetising the specimen, is usually an absorption which passes through a maximum, and changes to an emission when the applied field has become large.

Before leaving this subject, mention must be made of two comprehensive papers by E. A. Guggenheim (*Proc Roy. Soc.*, A, **155**, 49–70 and 70–101, 1936) on the thermodynamics of magnetic and electrostatic systems that are free from hysteresis.

A SUPER-CONDUCTOR OF HEAT.—A general account of the low temperature researches that have been carried out by Kamerlingh Onnes and his successors at the University of Leiden, since 1882, is contained in *Revue générale des Sciences* (**47**, 100–6, Feb. 1936). The article, which is entitled " Le Laboratoire Cryogène de Leyde," is illustrated by a number of interesting photographs.

In particular, mention is made (pp. 103–4) of the discovery that there are two kinds of liquid helium. At atmospheric pressure, liquid helium I changes, on cooling, into liquid helium II at about 2·19° K, a sudden change in the specific heat occurring in an interval of less than 1/1000 of a degree. Changes also occur in the density, thermal expansion and compressibility. The triple point, where solid helium, liquid helium I and liquid helium II, are in equilibrium, is at about 1·8° K and 30 atmospheres.

A preliminary note (*Physica*, **3**, 359–60, May 1936) of measurements of the thermal conductivities of the two kinds of liquid helium is given by W. H. Keesom and Miss A. P. Keesom. They find that the thermal conductivity of liquid helium I at 3·3° K. is 6×10^{-5} c.g.s., (comparable with that of air at 0° C). Liquid helium II, at 1·4° K, and 1·75° K, however, they find to have a thermal conductivity of the order of 190 c.g.s. units. This is about 200 times the conductivity of copper at room temperature and is the highest value so far found for a thermal conductivity. This result calls to mind the large electrical conductivity suddenly acquired by metals when they are cooled below some particular low temperature.

METEOROLOGY. By E. V. Newnham, B.Sc., Meteorological Office, London.

The Meteorology of Greenland.—Some knowledge of the meteorology of Greenland has gradually been obtained from the observations made on sledge journeys carried out during the summer, but recently this has been greatly increased as a result of expeditions which have established meteorological stations in the interior. In 1930–31 there was the British Arctic Air Route Expedition led by H. G. Watkins and the German Greenland Expedition under Alfred Wegener. Both these expeditions maintained stations during the winter, and the observations made gave for the first time an idea of the complete climatic cycle in regions far from the coast. The high plateau of Greenland lies near to that region around Iceland that is so important for the weather of the whole of Europe, being the principal breeding ground for the depressions that in combination with the nearly permanent high-pressure area of the Azores give us our prevailing south-westerly to westerly winds. Greenland has long been thought to play an important part in the formation of those depressions, and F. Loewe has done English meteorologists a great service by giving in English a summary of what has been learnt about its climate and by indicating the extent to which that country does actually appear to play the part attributed to it. The summary has taken the form of a lecture—the G. J. Symons Memorial Lecture to the Royal Meteorological Society delivered on March 18 1936 and published in the *Quarterly Journal* of that Society for July 1936—with the modest title of " The Greenland Ice Cap as seen by a Meteorologist." The author has had first-hand experience of the country described, for he accompanied Wegener's expeditions of 1929 and 1930–31.

There is probably some vagueness in the minds of most people as to the size of Greenland, for it is well known that its apparently huge size on the ordinary Mercator's projection is partly due to the distortion of areas in high latitudes caused by the use of that projection. The lecturer therefore began his account with some figures tending to clear up any such uncertainty. The ice-cap, he pointed out, extends from south to north through a distance equal to that separating England from the Canary Islands, and from west to east over a distance averaging about that between London and Geneva. The area, about 650,000 square miles, is about seven times that of Great Britain, and the mean altitude of that large part which is continuously covered by ice is about 7000 feet. The crest of the plateau, stretching from south to

north, never sinks below 8000 feet, and attains over 10,000 feet near its centre.

The inland stations for which winter data are available for the study of the climate are in the centre and the south-east and afford better evidence of the differences that arise from elevation and from distance from the sea than of those arising from latitude. The inland station of the British expedition was 140 miles from the coast, at an elevation of 8200 feet, and for the German station " Eismitte " the corresponding figures were 250 miles and 10,000 feet. The earlier sledge journeys likewise have given most information about the contrasts between coastal conditions and those obtaining on the lofty plateau far inland, but of course only for the summer.

In sympathy with the decreased cyclonic activity of the Icelandic low-pressure area in summer, this season is the one in which pressure is highest and the pressure differences between different parts of Greenland, and the daily variations at a single station, are least, and in which fine weather is most common. The average duration of sunshine expressed as a percentage of the possible duration is high, and its actual duration owing to the high latitude is very high, In the border zone (20–40 miles from the coast) the strong solar radiation melts the accumulated snow of winter. In the fjords thawing is assisted in the early summer by the ready absorption of solar radiation by rock faces too steep to have a snow cover, and the mean temperature is in many places nearly 50° F. by July ; on the ice cap far from the sea the universal whiteness is very unfavourable for absorption of radiation, the sun's rays being largely reflected by the snow crystals. This same whiteness does not, however, prevent the loss of heat by direct radiation from the snow or ice surface, moreover the amounts of water-vapour, and on most days of cloud, are so small that little of the out-going radiation is absorbed.

As the sunshine cannot be appreciably absorbed directly by the air, there is therefore normally a layer of very cold air near the surface, in summer as well as in winter, on the ice cap. At Eismitte, in spite of almost continuous sunshine, the mean air temperature from May to July inclusive was about 0° F., and no reading higher than 28° F. was ever observed. There is no month in which temperature may not descend to − 30° F., and this figure has already been observed in June and August. At Eismitte in sunny weather a substantial fall of temperature nevertheless was observed as the sun's elevation decreased when it traversed the northern horizon, the fall for example averaging

7° F. on such days between 8 p.m. and 9 p.m. when the sun was still about 10° above the horizon ; the whole diurnal variation in clear weather in early summer had the large average value of about 30° F. These facts may at first sight appear inconsistent with what has been said earlier of the smallness of the percentage absorption of solar radiation, but in reality are not so, the magnitude of the diurnal range being due to the fact that such heat as is absorbed is, owing to the stability of the surface layers of cold air, applied to only a small thickness of the atmosphere and is not carried to great heights by convection as happens generally in the summer in lower latitudes.

It is inevitable that, in the presence of such vast expanses of permanently snow-covered ground, relative humidity should be high, the more so as there is no ready exchange of air between the cold surface layers and the upper levels either through turbulence, which requires the presence of obstructions to the free run of the wind, or through convection, which is not very easily developed with such a stable atmosphere. In actual fact the vapour pressure is rarely much below that corresponding with saturation over ice at the temperature of the snow surface, the exceptions being generally observed when there is a foehn wind, which happens occasionally even at Eismitte. Owing to the absence of condensation nuclei super-saturation is a very common aerial condition at night.

One of the most interesting and important features of Greenland meteorology is the katabatic wind. This is a gravitational flow of the surface layers of cold air from the highest to the lowest lying regions. The katabatic wind is of course liable to occur practically anywhere throughout the world on cloudless nights provided that there is a suitable slope down which the air cooled by contact with the chilled ground can flow, but in Greenland it is not simply a night phenomenon for it is characteristic of the sledging season, when there is no night, as well as of the winter, and is on a gigantic scale. It gives the prevailing winds their characteristic distribution—a direction inclined somewhat to the right of that corresponding with a direct movement from high to low ground on account of the air being deflected to the right by the rotation of the Earth. Owing to the remarkable lack of big surface irregularities on the ice cap, turbulence is very small, and these winds have exceptional steadiness ; they are sometimes used by explorers as an aid to maintaining a straight course when sledging. They show a diurnal variation, being strongest at night, or when the sun is nearest to the horizon during those months

when it does not set. Their thickness is from 1000 to 1500 feet near the coast, but has not been determined with any certainty by direct measurement in the interior, where they may be expected to be less deep. It is through this gravitational flow of cold air from the ice cap that Greenland might be expected to be so important in the formation of Icelandic depressions, but the observations do not entirely support this theory. A fall of pressure over any part of the seas to the east, south or west of Greenland increases the pressure gradient from the ice cap to the coast and this brings an increase in the strength of the katabatic winds, which may develop into severe foehn gales in the fjords, but at a height of about 10,000 feet the cloud movement is towards the interior. Sometimes the depressions actually pass from the sea to the ice cap and changes of pressure in the upper atmosphere will then often destroy the katabatic wind by imposing a contrary pressure gradient. It was found at Eismitte that when the southerly to south-westerly winds of cyclonic type came to an end, and the time had presumably arrived for a powerful movement of cold air from the north-west to take place, on the supposition that the ice cap provides the cold sector of the depression, only very light northerly winds appeared and the normal katabatic wind, which was easterly at this station, was quickly resumed. Another fact that seems to negative the preconceived notions about the part played by the ice cap in cyclonic development, to which reference has several times been made, is that when violent outflows of cold air are observed they are generally local phenomena ; they often fail to cross the region between the ice cap and the coast. While admittedly there are large gaps in our knowledge of the Greenland wind system and its relationship to the Icelandic depressions, the incomplete picture that has been obtained of the relationship of the katabatic wind and the depression seems to be consistent with that found in other parts of the world, *e.g.* in the Mediterranean, where changes take place at high levels and eventually produce surface pressure conditions in which katabatic winds attain their best development, and we see the resulting " mistral " or " bora " best developed in the rear of an active depression that must presumably have derived the main part of its energy elsewhere.

Although many will prefer to reserve their opinion as to the part played by the ice cap in cyclonic activity there can be no question as to its main climatic features. Strong outward radiation from the snow surface under a clear or only thinly clouded sky may be regarded as the normal state of affairs, resulting in a layer

of very cold surface air, in which temperature increases vertically with height, but tends to decrease with height along the ground level as well as with distance from the sea. At Eismitte temperature is lower than at any other place in the world where continuous observations have been made, not excluding the coldest parts of Eastern Siberia. The mean for the whole year was − 22° F. In an abnormally "warm" winter temperature fell repeatedly to − 84° F. or − 85° F. during January, February and March; it reached − 60° F. for the first time on October 10 and for the last time on April 12. Compared with the coldest parts of Siberia the period of severest cold is longer and in addition to the lower temperature there is a relative infrequence of complete calm. Out of more than 1000 observations at Eismitte there was no case of a complete calm and only 2 per cent. of cases with a wind speed of less than 2½ miles an hour. The clear weather régime is interrupted by cyclonic influences, and, as has already been stated, occasionally by the passage of the centre of a depression. The cyclonic winds may be strong enough to break up the cold surface layer and clouds may descend to ground level. It is at such times that the highest temperatures occur, and in mid-winter at Eismitte temperature sometimes rose above 0° F.; on sixteen occasions between September and April it increased more than 35° F. in 24 hours. The increase of temperature is not entirely due to the breaking up of the cold surface layer and its mixture with potentially warm air from above, for the clouds assist by checking the loss of heat by radiation, and often snow which descends from the less cold upper atmosphere has some warming effect. During strong winds the air becomes so filled with driven snow from the surface that it is impossible to tell how much is due to precipitation at the cloud level. A notable phenomenon is the rapidity with which temperature falls in winter when the normal radiation processes are resumed after a cyclonic interruption. Temperature fell more than 35° F. in 24 hours ten times in the winter at Eismitte and once descended 90° F. in a little over three days. The inversion of temperature on passing upwards through the cold surface layer averages about 12 to 15° F. in summer and 25–30° F. in winter.

The evidence of the existence of a glacial anticyclone of the kind postulated by Professor Hobbs is on the whole satisfactory so far as the wind and weather of the cold surface layer is concerned, but the centrifugal winds are katabatic and not of the kind normally associated with anticyclones, and there is no conclusive evidence of the existence of relatively high pressure up to a high level in comparison with surrounding seas.

BIOCHEMISTRY. By W. O. KERMACK, M.A., D.Sc., F.R.S.E., Research Laboratory, Royal College of Physicians, Edinburgh.

THE NATURE AND FUNCTIONS OF COENZYMES.—A number of important papers have recently been published on this subject, one series by Warburg and his co-workers (cf. *Biochem. Zeit.*, 1935, **282**, 157), coming from the Kaiser-Wilhelm Institut für Zellphysiologie in Berlin, and another from the Laboratories of von Euler in Stockholm (cf. *Zeit. f. Physiol. Chem.*, 1935, **232**, 6, 10). Warburg has studied the enzyme system present in horse red blood cells, which catalyses the oxidation of hexose monophosphoric acid to " hexose phosphonic acid " in presence of atmospheric oxygen. It will be remembered that he had already shown that the enzyme system involved could be analysed into a number of components. One of these was the Warburg-Christian yellow oxidation pigment, which was shown, by the researches of Kuhn, Theorell and others, to consist of two parts (*a*), a flavin phosphoric acid capable of undergoing reversible oxidation and reduction, and (*b*), an enzyme-like protein component, with which the flavin phosphoric acid is in intimate association (cf. SCIENCE PROGRESS, 1935, XXIX, 695). Warburg now gives the name " flavin enzyme " to the combination of these two components (*a*) and (*b*). The other part of the enzyme system also proves to consist of two components. One of these is protein in nature and is to be regarded as a true enzyme (" *zwischenferment* " or intermediate enzyme), the other has now been isolated by Warburg from horse red blood cells in a relatively pure state, and proves to be a compound related to, but somewhat more complex than, the coenzyme of muscle glycolysis, which Lohmann showed to be adenosine triphosphoric acid. Warburg calls his new compound the " hydrogen transporting coenzyme." Like the flavin component of the flavin enzyme, it can reversibly take up and give off two atoms of hydrogen. Sodium hydrosulphite converts it directly into the reduced form, but hexose phosphoric acid can only do so in presence of the intermediate enzyme, being itself oxidised in the process to " hexose phosphonic acid." We may regard the intermediate enzyme as the dehydrogenase proper, its action being to activate the two hydrogen atoms of the hexose phosphoric acid so that they are readily taken up by the coenzyme. When the system contains only hexose phosphoric acid, intermediate enzyme (A), and the " hydrogen transporting coenzyme " (B), the oxidation of the hexose phosphoric acid comes to an end as soon as the whole of (B) has been reduced. The last-named cannot be oxidised directly by atmospheric oxygen and so remains in the reduced form. When however, the flavin enzyme (C) is added, it is quickly reduced by the

reduced form of (B), which is thereby brought back to its original state. But the reduced form of (C) is readily oxidised by atmospheric oxygen. One molecule of oxygen takes up two atoms of hydrogen from the reduced (C) molecule to form hydrogen peroxide. It will thus be seen that hexose phosphoric acid, in presence of the three components (A), (B) and (C) of the enzyme system, will be continuously oxidised by atmospheric oxygen, the products being "hexose phosphonic acid" and hydrogen peroxide. Two atoms of hydrogen are removed from each molecule of the substrate and transferred in stages, first to the coenzyme (B), then to the flavin enzyme (C), and finally to the oxygen.

Warburg has not only investigated the function of the coenzyme as part of the oxidation system; he has also analysed the carefully purified compound. It yields on hydrolysis, one molecule of adenine, three molecules of phosphoric acid, two molecules of a pentose, and one molecule of a base $C_6H_6ON_2$. The coenzyme is apparently formed by the union of these components with the loss of six mole-

cules of water. Its probable formula is therefore $C_{21}H_{28}O_{17}N_7P_3$. The base $C_6H_6ON_2$ proves to be the amide (I) of nicotinic acid and it is evidently an essential constituent of the coenzyme molecule. In fact, according to spectroscopic observations, it is this constituent which takes up the two atoms of hydrogen, the addition probably taking place at one of the double bonds between two of the carbon atoms. It is therefore to be regarded as the functional group in the coenzyme molecule, and so its isolation and identification is a most important advance.
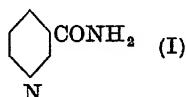
The researches of von Euler and his collaborators have been mainly concerned with the nature of the enzyme systems present in yeast cells. It is now thirty years since Harden showed that the fermentation of glucose by yeast juice is dependent on the presence of a substance, relatively thermostable and dialysable, which is to be regarded as the coenzyme of yeast fermentation and was therefore called cozymase. It was later found possible to separate the coenzyme in a solid form and, in recent years, von Euler has succeeded in obtaining from it two distinct compounds. One of these proves to be identical with the hydrogen transporting coenzyme of Warburg already described. The other is the true cozymase, that is to say, the coenzyme which is necessary for

phosphorylation and fermentation of sugar to occur under the action of yeast zymase.

In chemical composition, cozymase closely resembles the hydrogen transporting coenzyme. It contains adenine, phosphoric acid, a pentose and nicotinic acid amide. It evidently plays a rôle in yeast fermentation somewhat analogous to that which adenosine triphosphoric acid plays in muscle glycolysis. In fact it can replace adenosine triphosphoric acid in the latter process although adenosine triphosphoric acid is unable to act as a cozymase in yeast fermentation. But it can do more, for, like the hydrogen transporting coenzyme, it contains nicotinic acid amide and, presumably in virtue of this group, can act in some cases as a coenzyme in oxidation reduction processes. Thus cozymase appears to combine the functions of the hydrogen transporting coenzyme on the one hand and the muscle coenzyme, adenosine triphosphoric acid, on the other ; possibly we may regard the latter two coenzymes as specialised or degenerate types, suitable for phosphorylation and oxidation processes respectively, which have been derived from the less specified cozymase. The wide distribution of cozymase is shown by the fact that Warburg and Christian (*Biochem. Zeit.*, 1936, **285,** 156) have isolated it in relatively large quantities from horse red blood cells. The fact that it can participate in the two important processes of phosphorylation on the one hand, and of oxidation-reduction on the other, suggests that it plays a particularly important rôle in cell metabolism.

The discovery of the amide of nicotinic acid as an important constituent of living cells is at first sight very surprising and unexpected. Nicotinic acid had originally been isolated from rice polishings and was at one time erroneously thought to be identical with vitamin B. It is of interest to note that for several years the diethylamide of nicotinic acid has been widely employed under the name of coramine as a heart stimulant in cases of collapse. The discovery of its action was empiric and not based on any knowledge of its possible rôle in tissue metabolism. It now turns out that the parent substance, the amide of nicotinic acid, is an important constituent of living cells, and here it may be added that Warburg considers that the hydrogen transporting coenzyme actually present in heart muscle is probably identical with that found in blood. We may look forward to further new discoveries in these important and fundamental subjects in the near future.

STRUCTURE OF POLYPEPTIDES AND PROTEINS.—Of the three main classes of organic compounds, carbohydrates, fats and proteins, the last named is the most intimately connected with the processes

of metabolism, and the other activities which are characteristic of living cells. Fundamental progress in biochemistry is therefore largely dependent on an increase in our knowledge of the proteins, and so any addition to the methods, by means of which the detailed structure of these complex substances may be elucidated, is of very considerable importance. As the result of the classic work of Emil Fischer, it is now generally accepted that proteins and their fission products, the polypeptides, consist essentially of amino-acids joined together by amide linkages. The definite determination of the order in which these amino acids are united has proved to be a difficult and complicated task.

Fischer himself introduced the method of treating the protein, or polypeptide, with naphthalene sulphonyl chloride, and so converting the free amino groups into naphthalene sulphon-mido groups, thereby, as it were, labelling them as having been originally free. The protein or peptide may then be split up, by fission of the amide linkages under conditions such that the newly introduced naphthalene sulphonyl groups are not dislodged. The resulting amino acids can then be examined in order to discover which ones still carry the labels, and so were in a free condition in the original protein. An improvement on this method has recently been put forward by Gurin and Clarke (*J. Biol. Chem.*, 1934, **107,** 395), who employed the benzene sulphonyl instead of the naphthalene sulphonyl group as label. They found that the fission of the amide linkage can be effectively brought about by treatment at 90–100° with 50 per cent. formic acid containing 0·3 per cent. of hydrochloric acid. This does not appreciably disturb the benzene sulphonyl groups. Of course, in this method of analysis, all the terminal amino groups are labelled, that is to say, not only those at the end of the peptide chain itself, but also free amino groups on any of the side chains, which are a feature of the protein molecule. When the method is applied to gelatin it is found that, of all the free amino groups, at least 50 per cent. are to be assigned to the terminal amino groups of the diamino acid lysine whilst not more than 0·5 per cent. are attributable to monoamino acids.

Another method of labelling free amino groups was introduced by Bergmann in 1927. This consisted in treating the protein or polypeptide with phenyl *iso*cyanate and so forming the phenyl ureide

$$C_6H_5NCO + NH_2R = C_6H_5NHCONHR$$
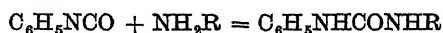
This method has recently been used by Hopkins and Wormall (*Biochem. J.*, 1934, **28,** 228) who were primarily interested in preparing modified proteins (analogous to the diazo proteins of Land-

steiner), and investigating their immunological reactions. They came to the conclusion that the chief amino groups involved in the reaction with phenyl *iso*cyanate were the terminal amino groups of lysine.

Information about the order of the amino acids in polypeptides and proteins may also be obtained from the application of certain methods, or reagents, which modify or remove the terminal amino acid without altering the rest of the molecule. Sometimes this may be done by means of specific enzymes. For example, a carboxy-polypeptidase will split off the amino acid, at the end of the chain, which carries the carboxy group. Recently, a method for eliminating a terminal amino acid unit by purely chemical means has been devised by Bergmann and Zervas (*J. Biol. Chem.*, 1936, **113**, 341). This ingenious process seems capable of wide application, and it may ultimately prove of great importance in the detailed elucidation of protein structure.

Bergmann's method consists in the application of the well known Curtius method of degradation to the terminal carboxy group of the polypeptide chain. It is converted into the hydrazide and then by treatment with nitrous acid into the azide which, when heated with an alcohol undergoes a molecular rearrangement, yielding a urethane. The reactions involved are shown as follows:

$$R_1.CO.NH.CHR_2COOH$$
$$\downarrow$$
$$R_1.CO.NH.CHR_2CONH.NH_2$$
$$\downarrow$$
$$R_1 \ CO.NH.CHR_2CON_3 \quad (I)$$
$$\downarrow$$
$$R_1.CO.NH.CHR_2.NH.COOAlk$$
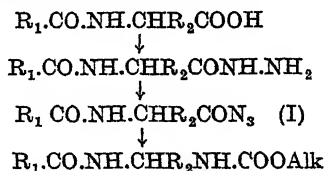
Previous attempts to apply the Curtius method to the stepwise degradation of polypeptide chains, broke down at this point, for the use of hydrolytic agents to break up the —NHCOOAlk group resulted in the hydrolysis, at the same time, of the rest of the polypeptide chain. Bergmann, however, now makes use of the property of benzyl esters which he has exploited with so great success for the synthesis of polypeptides, namely, the ease with which they can be reduced catalytically to yield toluene and the free acid. Thus, when the azide (I) is heated with benzyl-alcohol it yields the benzyl carbamido compound, and the free acid obtained from this on catalytic reduction rapidly loses CO_2 to yield the compound $R_1.CO.NH.CHR_2.NH_2$. The latter compound is decomposed with great ease to yield the aldehyde R_2CHO, ammonia and the intact residue of the polypeptide chain R_1COOH. The aldehyde may be identified, and thus the nature of the terminal amino acid established, whilst the rest of the chain can again be subjected to the same

series of reactions. In this way the successive amino acids can be identified, and the complete structure of the polypeptide established. Bergmann shows how the process works in the case of a tetrapeptide of known composition, and it would seem that a new and valuable method has been supplied to the biochemist for the determination of the detailed structure of proteins.

The substantial advance made in recent years in the field of peptide chemistry is exemplified by the recent work on glutathione, culminating in the synthesis of this polypeptide by Harington and Mead (*Biochem. J.*, 1935, **29,** 1602). Glutathione was discovered by Hopkins as a constituent of yeast and various tissues, and isolated by him in 1921. It has been the subject of many researches because of its possible importance in cell metabolism, and in particular it is now recognised to act as the coenzyme to glyoxalase (cf. SCIENCE PROGRESS 1934, XXVIII, 687). It was at first thought to be a dipeptide, namely glutamyl cysteine, and this conclusion remained unchallenged until some doubt was cast upon it in 1927 by Hunter and Eagles (*J. Biol. Chem.*, **72,** 147). The work of these two authors led Hopkins to reinvestigate glutathione and he was successful in isolating it as the well-defined cuprous salt, and it then became evident that it was really a tripeptide of glutamic acid, cysteine and glycine. The same conclusion was reached almost simultaneously by Kendal, McKenzie and Mason (*J. Biol. Chem.*, 1930, **84,** 657). It was then necessary to discover the order in which these amino acids were arranged within the molecule. This is a special case of the general problem discussed above and illustrates some of the methods which may be employed. Hopkins observed that prolonged boiling of the tripeptide resulted in the splitting off of glutamic acid leaving the dipeptide cysteyl glycine in the form of its anhydride. This indicated that glutamic acid was situated at one end of the chain. Furthermore, it had previously been shown by Quastel, Stewart and Tunnicliffe (*Biochem. J.*, 1923, **17,** 586) working with impure material—and subsequently confirmed by Kendal, Mason, and McKenzie (*J. Biol. Chem.*, 1930, 88, 409) in the case of the pure compound—that oxidation with hydrogen peroxide led to a product which yielded succinic acid on acid hydrolysis. The succinic acid can only be derived from the glutamic acid and so it follows that the latter is connected to the rest of the molecule through its carboxy group, the reactions involved being as follows :

$$RNH.CO.CH_2.CH_2.CH(NH_2)COOH$$

$$\downarrow$$

$$RNH.CO.CH_2.CH_2.COOH \longrightarrow COOH.CH_2.CH_2.COOH + RNH_2$$
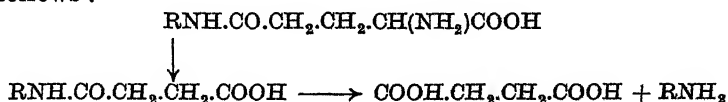
The fission of the tripeptide by carboxypolypeptidase, yields glycine and glutamyl-cysteine. This shows that glycine is at the other end of the polypeptide chain, a conclusion fully confirmed by other evidence. Thus reduced glutathione was definitely formulated as a tripeptide of the following structure :

$$\underset{\overset{|}{NH_2}}{COOH.CH.CH_2.CH_2.CO.NH.CH}\ \underset{\overset{|}{CH_2.SH}}{CO.NH.CH_2.COOH}$$
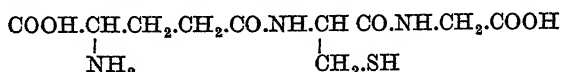
The synthesis of a tripeptide of this structure presented unusual difficulties, due to the fact that it contained the two amino acids glutamic acid and cysteine. The first amino acid contains two carboxy groups of which the one must be protected whilst the other is linked up with the cysteine. The presence of the ·SH group in the cysteine also introduces peculiar technical difficulties, so that the problem of synthesising glutathione did not appear very hopeful of solution. All the difficulties have, however, been successfully overcome by Harington and Mead, who developed a modification of Bergmann's valuable method of polypeptide synthesis, which renders the latter capable of being applied to peptides containing cysteine. It is unnecessary here to describe the details of the work ; it is sufficient to state that the product finally obtained was identical in crystalline appearance, melting point, optical rotation and chemical behaviour, with the natural product.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., D.Sc., The University, Glasgow.

IGNEOUS ACTION AND IGNEOUS ROCKS—EXTRA-BRITISH.—The veteran Norwegian petrologist W. C. Brøgger has published two further memoirs in continuation of his series describing the petrology of South Norway. These memoirs belong to a new series entitled " On Several Archàan (sic) Rocks from the South of Norway." The first deals with " Nodular Granites from the Environs of Kragerø " (*Norske Vidensk-Akad. Oslo, I. Math.-Nat. Kl.*, 1933, No. 8, 1934, 97 pp.). The Kragerø rocks are fully described and compared with other nodular types, and there is a full discussion of the origin of these peculiar structures.

The second memoir is entitled " The South Norwegian Hyperites and Their Metamorphism " (*ibid.*, 1934, No. 1, 1935, 421 pp.). The first and longest section of the work deals with the hyperites (gabbro rocks with orthorhombic pyroxene) of the Kragerø and other districts and their metamorphism. By metamorphism is meant chiefly the transformation of the rocks by the development of coronas and related structures. This is by far the most thorough-

going and best-illustrated recent account of these remarkable structures. The next section treats of the hyperites of Ødegarden and their transformation into the well-known scapolite rocks of that locality. The remainder of the volume is occupied by the description of numerous albitites and quartz-albitites, and especially the rutile-albitite known as *kragerøite*, with full petrogenetic discussions. In this astonishing work petrologists will find an immense amount of information regarding some unusual rock types. A long review has been published by A. Johannsen (*Journ. Geol.*, **44**, 1936, 420–2).

W. Larsson has published a long and immensely-detailed memoir, unfortunately without a summary, on " Der Nygård-pluton : eine geologisch-petrographische Detailstudie eines basischen Intrusivkörpers im Grundgebirge von Fennoscandia " (*Bull. Geol. Inst. Upsala*, **25**, 1935, 13–134). This igneous mass consists of various types of pyroxenite, norite, gabbro, anorthosite, etc., which were injected in three separate successive pulses. The earliest intrusion is least, the latest most, differentiated, with anorthosite and pyroxenite as extreme terms. The Nygård intrusion, with other norite-gabbro masses of Central Sweden, is probably of sub-Jotnian age. Twelve new chemical analyses are given.

The Arvidsjaur porphyries, an Archæan lava series described by E. Grip (*Bull. Geol. Inst. Upsala*, **25**, 1935, 135–220), consist of basalt, andesite, dacite, keratophyre, quartz-keratophyre and liparite, extruded in the order as given. Remarkable lithophysal and spherulitic structures in the acid rocks are described and figured. This series lies immediately above the Leptite formation of Sweden, and is thought to represent the effusive equivalent of the Older Granite (Jörngranit).

R. Norin's memoir " Zur Geologie der südschwedischen Basalte " (*Medd. Lunds Geol.-Min. Inst.*, No. 57, 1934, 1–174), with 15 analyses, represents an amplification and completion of his earlier work on the same subject (SCIENCE PROGRESS, April 1934, p. 694). In addition a long section gives the results of magnetic investigations on some of the basalt localities.

A modern account of the two great anorthosite areas of western Norway, Egersund and Bergen, is given by C. F. Kolderup (*Rept. XVI. Internat. Geol. Congr., Washington* 1933, Preprint May 1935, 6 pp. 2 maps). Both masses are of Pre-Cambrian age. The dominant anorthosite is associated with smaller bodies of congenetic norite, mangerite and birkremite. In the Bergen zone eclogites and serpentines also occur.

The memoir by A. Orlov, " Zur Kenntnis der Petrochemie des mittelböhmischen Plutons " (*Min. u. Petr. Mitt.*, **46**, 1935, 416–46)

reviews the 43 published analyses and gives short petrographical characterisations of the rocks of this igneous mass, which covers an area of 3200 sq. km. The rock types range from aplitic granite to olivine-gabbro. Essential differences between the north-western, eastern and southern parts of the pluton have been established. The internal petrological structure of the mass clearly depends on the tectonic arrangement of its original envelope, and on the dynamic processes that occurred during the act of intrusion.

In their paper " Beitrage zur Kenntnis einiger jungvulkanischer Gesteine Spaniens," C. Burri and I. Parga-Pondal (*Schweiz. Min. u. Petr. Mitt.*, **15**, 1935, 266–88) describe some hitherto not well-known volcanic rocks of recent geological ages, including trachyte, monchiquite, basalt, ankaratrite, potash limburgite and olivine-nephelinite, with seven new analyses. These rocks belong to the Iberian Foreland Province, which appears to consist largely of basic alkaline rocks (C. Burri, " Die Iberische Vorlands-provinz," *Schweiz. Naturf. Ges.*, 1935, 333–4).

N. Bezborodko (*Journ. Geol. All-Ukrainian Acad. Sci., Kyiv*, 1934, **1**, 61–94. English summary, 90–4. See *Min. Mag., Min. Abstr.* **6**, No. 3, Sept. 1935, 122, for longer abstract) describes a new rock type from Podolia—*sabarovite*—which is the extreme acid member of a new igneous rock series called the *bugite* series (N. Bezborodko, *Trav. Inst. Min. Acad. Sci., U.R.S.S.*, 1931, **1**, 127–59). These are stated to be pyroxene-tonalites with silica percentage ranging from 50 to 72, and pyroxenes from 40 per cent. to zero. The potash content is always less than 2 per cent., hence the paucity or absence of orthoclase. These rocks exhibit analogies to the charnockite series.

" Die sauren Ergussgesteine von Kap Franklin," described by H. G. Backlund and D. Malmqvist (Zur Geologie und Petrographie der Nordöstgronlandischen Basaltformation II. *Medd. om Grönland*, **95**, No. 3, 1935, 84 pp.), consist of rhyolitic lavas of Tertiary age which were extruded between the Older Dolerites and the Plagioclase Basalts of East Greenland. These rocks are notably richer in potash than comparable types in other parts of the Thulean igneous province.

In a second memoir describing his " Geological Investigations in East Greenland. Part II. Geology of Cape Dalton," L. R. Wager (*Medd. om Grönland*, **105**, No. 3, 1935, 32 pp.) deals with the thin Eocene Cape Dalton sedimentary series which begins with a conglomerate the pebbles of which consist of highly alkaline nepheline- and leucite-bearing lavas. These may have been derived from a Late-Cretaceous or Early Tertiary igneous series which is postulated,

on the basis of the occurrence of hypabyssal alkaline types, to have once overlain the Davy Sound region.

Two remarkable laccoliths consisting mainly of ultrabasic rocks are described by E. Ingerson from the Trout River area of Newfoundland (*Amer. Journ. Sci.*, **29,** 1935, 422–40). In each laccolith there is a basal zone of dunite and bronzite-peridotite over 10,000 ft. thick, followed by a zone consisting of alternating layers of feldspar-rich and olivine-rich rocks. Above this critical zone is an upper layer of dunite and peridotite only one-tenth as thick as the lower layer. Both laccoliths have been tilted, peneplaned, uplifted and partly dissected.

R. W. Chapman and C. R. Williams have published a very full account of the "Evolution of the White Mountain Magma Series" of New Hampshire and neighbouring states (*Amer. Min.*, **20,** 1935, 502–30). This series of igneous rocks may belong to the New England extension of the Monteregian Province of Canada. The writers show that a combination of fractional crystallisation, pure melting and assimilation has been necessary to produce this magma series. Their map shows a number of obvious ring structures.

The "Percy Ring-Dike Complex," belonging to the White Mountain Magma Series of New Hampshire, is fully described by R. W. Chapman (*Amer. Journ. Sci.*, **30,** 1935, 401–31). The ring-dikes are composed of various types of syenite and granite, and are of elliptical shapes with an average diameter of about six miles. At least two of the rings were probably complete ellipses, but have been partially destroyed by later intrusions. The rings are grouped about three separate centres of intrusion which are arranged along an east to west line. Stocks of later granite have obliterated much of the earlier ring structures. Both ring-dikes and stocks are believed to be the results of Late-Palæozoic cauldron-subsidences.

A connected general account of the White Mountain Magma Series of Late-Palæozoic age, with descriptions of several older magma series reaching back to the Ordovician, is contained in two publications by M. P. Billings and C. R. Williams (Geology of the Franconia Quadrangle, New Hampshire. *The State Planning and Development Commission, Concord, N.H.* 1935, 35 pp. Geology of the Littleton and Moosilauke Quadrangles N.H., *ibid.* 1935, 51 pp.).

In a discussion of the "Pegmatites at Collins Hill, Portland, Conn.," W. F. Jenks (*Amer. Journ. Sci.*, **30,** 1935, 177–97) makes a valuable contribution to data on the origin of pegmatites.

T. S. Lovering's memoir on the "Geology and Ore Deposits of the Montezuma Quadrangle, Col." (*U.S. Geol. Surv., Prof. Paper* 178, 1935, 119 pp.) is of considerable petrological interest in that it

contains a good résumé of the Tertiary intrusive igneous rocks of the Front Ranges of Colorado, with a collection of 57 analyses. The rocks are mainly of calc-alkaline characters, but a late (Miocene) alkaline group consisting of phonolites and alkali-syenites also occurs.

"A Brief Review of the Geology of the San Juan Region of South-western Colorado" by Whitman Cross and E. S. Larsen (*U.S. Geol. Surv., Bull*, 843, 1935, 138 pp.) is of interest to petrographers as it contains a brief account of the pre-Jurassic Iron Hill stock consisting of limestone (hydrothermal ?) with pyroxenites, melilite-rocks, ijolite, theralite and nepheline-syenite—an association recalling that of the Fen District of Norway described by Brøgger. The San Juan region also contains a long series of Tertiary volcanic rocks, chiefly andesites and rhyolites.

In a paper on "Mother Lode and Sierra Nevada Batholith," E. Cloos (*Journ. Geol.*, 43, 1935, 225–49) shows that the silicified and mineralised zone (the Mother Lode) which accompanies the western border of the Sierra Nevada batholith of California is divisible into three parts : a northern area of numerous scattered lodes, a continuous medial zone, and a barren southern portion. This arrangement coincides with a similar tripartite arrangement of the batholith ; *i.e.* a northern area of numerous scattered small massifs, a central continuous steep-walled mass, and a southern portion which transgresses its western wall obliquely along eastwardly-dipping contact planes.

According to E. B. Mayo (*Journ. Geol.*, 43, 1935, 673–89) an irregular discontinuous septum of metamorphic rocks separates two great composite intrusions along the eastern slope of the Sierra Nevada of California. The field evidence indicates that stoping and assimilation have played only minor rôles, that the intrusions have been forcefully emplaced, and that they are later than the folding of the country rocks.

The dark inclusions in the Bonsall tonalite of southern California, described by C. S. Hurlbut (*Amer. Min.*, 20, 1935, 609–30), are believed to represent xenoliths of an older gabbro which have been altered by reaction with the magma.

The "Basaltic Rocks in the Umpqua Formation" of Oregon are described by F. G. Wells and A. C. Waters (*Bull. Geol. Soc. Amer.*, 46, 1935, 961–72) as flows of amygdaloidal and ellipsoidal basalts, olivine-basalt dikes, norite sills and necks, and hypersthene-augite-basalt dikes, with four new chemical analyses. The intrusive rocks are probably Miocene, but the basaltic lavas are intercalated in the Umpqua formation of Eocene age.

A study of the geology and petrography of igneous rocks from the Bolivian Andes has been made by R. Kozlowski and K. Smulikowski (*Arch. de Min. de la Soc. des Sci. et des Lett. de Varsovie*, **10**, 1935, 122–242). The rocks include granodiorites and pegmatites from the Pre-Palæozoic ; a single phonolite of probably Carboniferous age ; dolerite, shoshonite, banakite, picrite, and limburgite of Mesozoic age ; and numerous granodiorites, granites, porphyries, andesites, dacites, dellenites and rhyolites from the Tertiary.

Four new chemical analyses of phonolites and one of basanitoid from the Canary Islands are recorded by K. Smulikowski (*Odbitka z Pamiet. XIV Zjazdu Lek. i Przyrodn. Polsk. w Poznaniu*, Warsaw, 1933, 233–6).

In his paper on " Extinct Volcanoes and Associated Intrusions in the Libyan Desert " K. S. Sandford (*Trans. Roy. Geol. Soc. Cornwall*, XVI, Pt. 7, 1935, 331–58) outlines the distribution, geology and petrology of the volcanic rocks in part of the Libyan Desert and the N.W. Sudan. Craters and lava plateaus are of frequent occurrence in this region. The rocks range from rhyolites, through trachytes (both quartz- and feldspathoid-bearing varieties), to dolerites, basalts and basanites. They thus conform to the general alkaline character of the great petrographical region of N.E. Africa.

The " Charnockite Series of Uganda, B.E.A.," according to A. W. Groves (*Quart. Journ. Geol. Soc.*, **91**, 1935, 150–207), comprises acid, intermediate, basic and ultrabasic types which compare closely with the original charnockite series of India. Dr. Groves fully describes the series with several new chemical analyses of the rocks and of some of the constituent minerals. He regards the charnockite series as due to the process called " plutonic metamorphism " by Tyrrell, *i.e.* recrystallisation in the solid under the influence of very high temperature and high, mainly uniform pressure, at great depths in the earth's crust, acting upon a granite-gabbro differentiation series of normal composition.

In his paper on " The Compositions of Some African Granitoid Rocks," F. F. Grout (*Journ. Geol.*, **43**, 1935, 281–96) gives chemical analyses and short petrographic descriptions of twenty-one large intrusive masses from the African Shield. Together with earlier data, these analyses show that the core of the continent is not abnormal in composition, as might be thought if attention were confined to the numerous analyses of rarer rock types from coastal regions.

Rock specimens dredged at 744 fathoms near the Providence Reef 220 miles N.N.E. of Madagascar are described by J. D. H. Wiseman (*Trans. Linn. Soc. London*, 2nd Ser. Zoology, XIX, Pt. 4,

1936, 437–43). They consist of fragmental basaltic material (limburgitic) in a matrix of calcite. The occurrence of an Orbitoid foraminiferan places the age of the rock, which probably represents the foundation of the reef, as approximately Eocene-Oligocene.

The memoir entitled "Mongolian Magmas" by G. W. Bain (*Bull. Geol. Soc. Amer.*, **46**, 1935, 1745–1814) deals with the extensive and complex igneous rock series of the Gobi Desert, which range from Pre-Cambrian to Tertiary. A Pre-Cambrian series exhibits gabbro, diorite and granite. The Mongolian Batholith, of late-Pre-Cambrian or Palæozoic age, has much the same rock types and the same range of chemical composition as the Pre-Cambrian series. A Jurassic igneous episode produced gabbros and basalts, porphyries and aplites. Tertiary igneous activity gave rise to basalts, trachytes, rhyolite, and some curious "serpentform" dikes of hornblende-gabbro. Forty-six new chemical analyses are published from which certain petrological conclusions are drawn. F. F. Grout makes adverse comments on this memoir chiefly on the basis of alleged inaccurate analyses and is answered by the author (*Bull. Geol. Soc. Amer.*, **46**, Supplement, 1935, 2066–73).

J. Suzuki summarises "Volcanic Activity in Japan in the Mesozoic Era" (*Proc. Fifth Pacific Sci. Congr.*, Canada 1933, 1934, 2381–4). The early part of Mesozoic time was free from volcanic activity, but in the Upper Jurassic and the Cretaceous porphyrites and their tuffs, dolerites, andesites, liparites and tuffs, were erupted in some quantity.

Granitic rocks occupy about one-eighth of the total area of the principal islands of Japan. The published chemical analyses of Japanese granites, to the number of 135, have been collected by J. Suzuki and T. Nemoto (*Journ. Fac. Sci. Hokkaido Imper. Univ.*, Ser. IV, **3**, No. 1, 1935, 48 pp.). From 94 of these a general average of Japanese granites has been computed. Compared with Daly's average granite the Japanese average differs in its higher silica and ferrous oxide, and its lower potash.

On the basis of a detailed discussion of the 121 available analyses T. Tomita shows that there is a distinct Kainozoic petrographical province consisting of rocks of the trachybasaltic kindred in the region surrounding the Sea of Japan (*Journ. Shanghai Sci. Inst.*, Sect. II, **1**, 1935, 227–306). Several new types of variation diagram are utilised for the comparison of this series with that of the Japanese calc-alkaline province, and with a generalised trachybasaltic series. Tomita regards the olivine-basalt magma type of the circum-Japan Sea province as the primary magma, the tholeiitic magma type of the Japanese province being obtained from it by silicification.

The lavas of the Alaid volcano and of the islet of Taketomi in the Kuriles have been described by H. Kuno and H. Tanakadate (*Jap. Journ. Geol. and Geogr.*, **12**, 1935, 153–62 ; *Proc. Imper. Acad., Japan*, **11**, 1935, 155–7). Both volcanoes are built of olivine-anorthite-basalts which, notwithstanding an often considerable amount of modal olivine, are over-saturated with silica, as shown by the appearance of quartz in the norms of the analyses.

Geological and petrographical observations on some of the Kurile islands are reported by Y. Sasa and J. Suzuki (*Proc. Fifth Pacific Sci. Congr., Canada* 1933, 1934, 2467–71 ; 2473–7 ; 2479–82). The arc of the Kurile Islands consists of a frontal zone of Mesozoic sediments with plutonic rocks of the same age, and a main zone consisting of many recent volcanoes resting upon a Kainozoic foundation. The volcanic rocks are chiefly hypersthene-bearing andesites. They are described with several new analyses.

In his Presidential Address to the Royal Society of New South Wales W. R. Browne gave " An Account of Post-Palæozoic Igneous Activity in New South Wales " (*Trans. Roy. Soc. N.S.W.*, 1933, 9–95). Mesozoic volcanic activity was not extensive, but during the Tertiary basic eruptions took place on a regional scale throughout a wide coastal belt. Older and newer groups of lavas and intrusions are recognised. The predominant basaltic lavas are of both calcic and plateau-basalt (undersaturated) types, with intrusive teschenites and theralites corresponding to the plateau-basalts, and quartz-dolerites which are probably the intrusive equivalents of the calcic basalts. The whole series is co-magmatic with the Tertiary igneous rocks of Queensland, Victoria and Tasmania, and shows petrological resemblances to the Thulean and other basaltic regions of the world.

The memoir by Miss Ida A. Brown on " The Geology of the South Coast of New South Wales, with Special Reference to the Origin and Relationships of the Igneous Rocks " (*Proc. Linn. Soc. N.S.W.*, **58**, Pts. 5–6, 1933, 334–62) gives "an account of the distribution, lithological characters, mutual relationships and tectonic structures of the sedimentary rocks, whose ages range from Cambrian (?) to Post-Tertiary, and summarises the field-occurrence and petrological and chemical characters of the associated igneous rocks, which belong to three principal periods of igneous activity. These are referred to the Devonian, Kamilaroi and Tertiary periods, which are represented by rocks of sub-alkaline, monzonitic and alkaline facies, respectively." The progressive change in the basicity and alkalinity of the injected magmas within this region suggests an ultimately co-magmatic origin for all the igneous rocks.

In Miss G. A. Joplin's second memoir on the petrology of the

Hartley District of New South Wales, entitled " The Metamorphosed Gabbros and Associated Hybrid and Contaminated Rocks " (*Proc. Linn. Soc. N.S.W.*, **58**, Pts. 3–4, 1933, 125–58) she shows that differentiation of an early partial magma of gabbro gave rise to a slightly more acid core. The whole mass was then enveloped by a ring-like intrusion of a later, more acid, partial magma of quartz-mica-diorite, and the gabbros have thereby suffered three types of metamorphism : 1, Thermal metamorphism ; 2, Reaction or partial hybridisation ; 3, Hydrothermal metamorphism.

In his work on " Acid Rocks of the Taupo-Rotorua Volcanic District " P. Marshall (*Trans. Roy. Soc. N.Z.*, **64**, 1935, 1–44) describes under the new designation " ignimbrite " tuffaceous rocks of acid composition which have been produced by a " nuée ardente Katmaienne " (Lacroix's nomenclature). Ignimbrites are coherent rocks of uniformly fine texture, are devoid of bedding, possess a pronounced prismatic jointing, and typically show a flow structure due to the bending of viscous glass shreds around pre-existing crystals. Rocks of this character are said to cover 10,000 sq. miles in the North Island of New Zealand.

C. E. Tilley describes " Enderbite, a New Member of the Charnockite Series " from Enderby Land, Antarctica (*Geol. Mag.*, **73**, 1936, 312–16). It is an extremely quartz-rich member of the charnockite series with over 40 per cent. of quartz. It differs from other acid charnockites by the substitution of plagioclase (antiperthite) for orthoclase. This rock appears to be almost identical with the sabarovite of Podolia (see p. 314), but is slightly richer in quartz.

BOTANY. By PROFESSOR E. J. SALISBURY, D.Sc., F.R.S., University College, London.

VOLUME 46 of *Berichte der Schweizerischen Botanischen Gesellschaft*, which appears as a Festband on the occasion of Professor Rubel's sixtieth birthday, contains a number of papers of ecological interest. The first, by the present writer, treats of the light climate of woodlands. The estimations were made by means of photoelectric cells of the rectifier type and preliminary experiments showed that over a considerable range of light intensity the values recorded are closely proportional to the photosynthetic efficiency. Data obtained in diverse types of woodlands at various seasons not only demonstrate the striking quantitative differences between " light phase " and " shade phase " but also the great significance of such biological features in the ground flora as the periods of assimilatory activity, the height of the assimilatory surfaces, and the spatial structure of the canopy as influencing the duration

and frequency of sun-flecks. Alike in a Fagetum, a Betuletum, and a Quercetum the intensities both in the light and shade phases were appreciably higher at 140 cm. than at ground level. As a rough approximation it is estimated that light intensities up to from 17 per cent. to 30 per cent. for the shade flora, and of from 50 per cent. to 80 per cent. for the sun species, probably have photosynthetic significance during the light phase.

To the same number (p. 517) Professor Rytz contributes an interesting account of the effects of various experimental treatments on alpine communities. The effect of the addition of chalk to Nardetum was in five years to produce a marked increase of Leguminosæ and diminution of such species as *Salix retusa, Sieversia montana, Viola calcarata, Campanula barbata*. Addition of a complete fertiliser without chalk had, however, similar effects and caused marked approach to the fresh-meadow type of vegetation.

H. Walter (p. 217) gives a useful summary of the water relations of Mangrove plants. Five zones are recognised. The outermost is occupied by *Sonneratia alba* growing in a soil solution with an osmotic value of nearly 26 atm. The transpiration from the leaves of *Sonneratia* when measured was found to be 8·8 mg. per sq. dm. per min. and the osmotic pressure of the leaf sap about 34 atm. In the landward direction corresponding to longer exposure at low tide the concentration of the soil solution rises and in the second zone dominated by *Rhizophora mucronata* the osmotic pressure attains 27 atm. Transpiration is 6·1 mg. per sq. dm. and the osmotic value of the leaves about 35 atm. In the third zone *Ceriops Candolleana* is dominant with osmotic pressures of between 32 and 39 atm. The fourth zone is characterised by *Avicennia marina*. Here the soil solution attains an osmotic pressure of 41 atm. and the leaves of the dominant attain osmotic pressures of between 36·4 and 65 atm. Beyond the Avicennia there is a zone bare of vegetation. A point to which the author calls attention, and which emphasises the importance of distinguishing between suction force and osmotic pressure, is that the viviparous seedlings which obtain their water supply from the parent plant have only about half the osmotic pressure and, whereas the mangroves all have a rich chloride content, that of the seedlings is low.

Bulletin No. 99 of the *Australian Council for Scientific and Industrial Research* is a survey of the pastures of the Commonwealth, accompanied by a coloured map showing the distribution of the major pasture types throughout Australia. The author, A. McTaggart, recognises 14 zones ranging from the coarse grasses

of the open forest grazing areas to the arid types with Spinifex (*Triodia*). A description is given of the chief species responsible for pastures in each of the vegetation zones.

The phenomenon of "internal cork" in apples is shown by H. O. Askew (*N.Z. Jour. Sci. and Tech.*, XVII, 1, p. 388, 1935) to be associated with a low boron content of both fruit and foliage, the former from affected trees containing about a third of the boron found in healthy fruits. These data, in conjunction with Atkinson's successful results from injection with boron solutions, render it highly probable that internal cork is a boron-deficiency disease.

The importance of plant cover in regulating wind action is clearly shown in the report on soil drift in the arid pastoral areas of South Australia by F. N. Ratcliffe (*Pamph.* 64, *Council Sci. and Indust. Res.*, 1936). Evidence is afforded that the serious soil erosion is an outcome of removal of the plant cover through overstocking. Stocking has not taken account of the rainfall fluctuation and the recent dry seasons have not enabled the vegetation to keep pace with the grazing. This absence of equilibrium is attributed to injudicious agricultural policy rather than to grazing by rabbits or secular climatic change which are held to have played minor rôles in bringing about the disharmony.

BIOLOGICAL.—Interesting data are furnished in the Annual Report of the Official Seed Testing Station (*Jour. Nat. Inst. of Agric. Botany*, IV, No. 1, 1935) for 1933 to 1934. This deals with the seeds harvested in the hot summer of 1933 and it is noticeable that in nearly every instance the percentage germination is higher than usual and in most species constitutes a record. As compared with the averages for 1917 to 1933, Parsnip seeds showed an increased germination of 14 per cent. and Runner Beans of 10·4 per cent. Dwarf Beans and Vetches were amongst the few which yielded diminished germinations. In this connection the higher percentages of "hard seeds" are noteworthy being 7·1 per cent. for Red Clover, as compared with an average of 4·4 per cent., and 14·9 per cent. for White Clover, as compared with an average of 10 per cent. Whilst, therefore, it is evident that Wild White Clover, with an average of 12·6 per cent. of hard seeds in over 800 samples, is hereditarily different in this respect from Red Clover with 4·4 per cent. as the average of 2269 samples, yet climatic conditions would appear to play a not inconsiderable part in determining this important biological feature.

The duration of life of plants is one aspect that, as the writer has pointed out elsewhere, has received too little attention despite

its biological importance. B. N. Singh (*Proc. Indian Acad. Sci.*, Vol. II, No. 4, 1935) from an examination of several species concludes that longevity is associated with the type of respiratory change and its vigour. Comparing annuals of short, medium and long duration the higher average rates of respiration are associated with the species of longer duration. One of the species, however, cited as of short duration is *Fœniculum vulgare* which, in this country, is a perennial species, a fact which suggests that if duration and type of respiratory activity are associated as suggested, this relationship may not be of a causal character.

Two strikingly different strains of *Deschampsia cœspitora* are reported by I. G. Lewis (*Jour. Board of Greenkeeping Research*, IV, No. 13, p. 112, 1936). The normal tall form has an average height to the tip of the panicles of 110 cm. and relatively light-coloured foliage. A dwarf strain cultivated alongside the type had an average height of only 70 cm. and possessed denser and darker foliage, in addition to which it flowered 14 days later.

A study of five grass species introduced to Australia (*Pamphlet 63 of the Council for Scientific and Industrial Research*), contains some useful biological data. The rooting depth of *Agropyron intermedium* and *Brachypodium phoenicoides*, is over 39 inches, of *A. cristatum* over 48 in. The roots of *Festuca Moirei* extend to a depth of 5 feet, whilst those of *Ehrharta erecta* barely reach $2\frac{1}{2}$ feet. The numerous tillers of the Festuca and its deep root system render it well suited to withstand drought, heavy grazing and severe competition.

Features in the biology of *Medicago sativa* are given by J. W. Hodfield and R. A. Calder (*N.Z. Jour. Sci. and Tech.*, XVII, No. 4, pp. 577–94). About a quarter of the flowers actually set pods, of which about 56 per cent. is due to self-pollination. With artificial self-pollination 100 flowers yielded 79 seeds, as compared with 480 seeds from 100 cross-pollinated flowers. Further the average number of seeds per pod was 6 in the cross-pollinated flowers, and only 1·8 in the selfed. Plants raised from seeds produced by selfing showed diminished vigour and yielded only about five-sevenths of the herbage produced by off-spring from cross-pollination.

The effect of soil conditions upon the root system of *Medicago sativa* has been studied by H. N. Watenpaugh (*Soil Science*, 41, 449–68, 1936). The experiments involved the growth of Alfalfa in columns of soil in which the upper portion and lower portions had various treatments with lime. In soil with a reaction of pH 4·7 the rooting depth was only 5 inches, whereas with liming that altered the reaction to 5·5–5·6 pH, the depth of rooting attained

to over 12 inches. The results of these experiments would seem to emphasise the pronounced effect of exchangeable calcium on rooth growth.

PLANT PHYSIOLOGY. By PROFESSOR W. STILES, Sc.D , F.R S , The University, Birmingham.

MINERAL CONSTITUENTS OF PLANTS.—A summary of recent advances in our knowledge of mineral nutrition of plants up to the end of 1934 has recently been published by F. C. Steward ("Mineral Nutrition of Plants," *Annual Review of Biochemistry*, 4, 519–44, 1935) ; consequently, only works published during the last 18 months are considered in this article.

The influence of potassium on the growth of *Lemna minor* has been investigated by H. L. White ("The Interaction of Factors in the Growth of Lemna. VII : The Effect of Potassium on Growth and Multiplication," *Ann. Bot.*, 50, 175–95, 1936). The concentration of potassium in the nutrient solutions used for the culture of the plant ranged from nothing to 300 mg. per litre. It was found that with a continuous light intensity of 450 foot candles and a temperature of 25° C. the optimum concentration of potassium for increase of leaf area was 200 mg. per litre, but for net assimilation rate and rate of increase in the *number* of fronds the optimum was about 20 mg. per litre. Shortage of potassium leads to low rate of assimilation, a high starch content and a high dry weight per unit area, and extracts from potassium-deficient colonies appeared to possess a low capacity for starch hydrolysis. The author thinks the results indicate that potassium regulates carbohydrate metabolism inasmuch as it brings about the activation of starch hydrolysing enzymes and so affects the starch-sugar balance in the plant.

In a second paper by the same author ("The Interaction of Factors in the Growth of Lemna. VIII : The Effect of Nitrogen on Growth and Multiplication " *Ann Bot.*, 50, 403–17, 1936) similar experiments are described, the varied nutrient being nitrogen instead of potassium. In this work the concentration of nitrogen, as calcium and magnesium nitrates, varied from zero to 100 mg. per litre. The optimum nitrogen concentration was found to be about 5 mg. per litre. Shortage of nitrogen was found to lead to a low rate of multiplication of fronds, small frond area, high net assimilation rate and high starch content. With high concentration of nitrogen, whereas the rate of multiplication, the net assimilation rate and the dry weight per unit area were the same as with the optimum concentration of nitrogen, the frond area was less and the chlorophyll content low. The accumulation of starch in plants

suffering from nitrogen starvation is thought to be related to the falling rate of multiplication which involves a lower rate of consumption of carbohydrate.

Nitrogen nutrition in barley has been examined by F. J. Richards and W. G. Templeman (" Physiological Studies in Plant Nutrition. IV : Nitrogen Metabolism in Relation to Nutrient Deficiency and Age in Leaves of Barley." *Ann Bot.*, **50**, 367–402, 1936). Plants of barley of the variety Plumage Archer were grown in sand cultures under a variety of conditions as regards mineral nutrition, namely complete nutrients and deficiency respectively of nitrogen, phosphorus and potassium. At a number of times during the period of vegetative development samples of the leaves were taken and analysed for various nitrogen fractions, namely, protein N, crystalloidal N, total amino N, amide N, nitrate N and residual crystalloidal N. It was found that the content of total nitrogen and most of the nitrogen fractions rose to a maximum at about the time of development of the second to fourth leaf, after which the content fell to a minimum with the production of the eighth or ninth leaf, after which the later formed leaves exhibited a rise in nitrogen content. In the individual leaf nitrogen content fell with age in the earlier-formed leaves, but in the later-formed ones rose at first. The nitrogen is recognised as derivable from two sources, freshly absorbed nitrogen and that from hydrolysis of protein in ageing leaves.

Although the quantity of nitrogen in nitrogen-starved plants, and particularly in the later-formed leaves, is less than in those provided adequately with that nutrient, the relative distribution of the nitrogen among the various fractions is much the same as in plants fully provided with nitrogen. The matter is, however, very different when there is a shortage of phosphorus. In this case protein formation is lessened even in the young leaf and falls off rapidly as the leaf grows older, whereas there is a greatly increased concentration of amide as well as some increase of nitrate and very definite accumulation of amino-nitrogen. With shortage of phosphorus there thus appears to be a definite inability for synthesis of protein beyond the stage of asparagine, and the conclusion is also drawn that the similarity observed in the symptoms of nitrogen and of phosphorus deficiency is due to this inability to synthesise protein which leads to low meristematic activity.

Shortage of potassium leads to a rapid disappearance of protein from the leaves as they grow older, and this is accompanied by increases in amino-nitrogen, amide nitrogen and nitrate. The view is expressed by the authors that potassium is not directly concerned with the synthesis of protein, but is related in some way to the

maintenance of the protoplasmic system. Hence in absence of potassium the protoplasm breaks down, the leaves die, protein becomes hydrolysed and the simpler nitrogenous substances accumulate.

The influence of phosphate supply on the development of a number of plants has also been examined recently by Anna L. Sommer (" The Relationship of the Phosphate Concentration of Solution Cultures to the Type and Size of Root Systems and the Time of Maturity of Certain Plants," (*Journ Agric Res.*, **52**, 133–48, 1936). The species investigated were peas, buckwheat, maize, tomatoes, wheat and cotton, and the plants were grown in water culture in 1000-litre tanks, the phosphate concentration ranging from 0 to 12 8 parts per million. The solutions were continuously aerated. It was found that plants with larger root systems, such as buckwheat and tomatoes, grew better in low phosphate concentrations and contained more phosphorus in the stem, than plants with smaller root systems such as cotton.

It was found that high concentrations of phosphate do not stimulate root development, while plants growing in higher concentrations of phosphate take longer to reach maturity than those growing in lower concentrations of phosphate.

The effect of adding different quantities of sodium nitrate to sand cultures of a leguminous plant, lucerne, has been investigated by H. G. Thornton and H. Nicol. " Reduction of Nodule Numbers and Growth, produced by the Addition of Sodium nitrate to Lucerne in Sand Culture," *Journ. Agri. Sci.*, **26**, 173–88, 1936.) The experiments were carried out in pots in which the amount of sodium nitrate ranged from 0 to 10 grams per pot containing 12 kilograms of sand. It was found that there was no correlation between the crop yield or nitrogen content of the crop and the amount of added nitrate. There was, however, a definite effect on the development of root nodules. With doses of nitrate above 1 gram the number of nodules was less the greater the amount of nitrate added, while the length of nodules was reduced about 30 per cent. by 1 gram of sodium nitrate and progressively more with larger quantities of nitrate. When the amount of bacterial tissue in the nodules was considered the effect of the nitrate was found to be even greater than that indicated by the reduction in the size of the nodules. Thus, with no added nitrate the bacterial tissue was found to occupy about 220 cubic millimetres per gram of root ; whereas with 1 gram of sodium nitrate per pot the amount was only about 75 cubic millimetres, and with 6 grams of added nitrate the amount of bacterial tissue was less than 10 cubic millimetres per gram of root.

The effect of adding sodium nitrate to cultures of lucerne is thus very greatly to diminish nodule formation.

In comparatively recent years much evidence has been produced suggesting the necessity for plant development of small quantities of certain elements which were previously thought to be unessential for plants, and which are certainly injurious in even moderate or low concentration. This has been indicated both for higher and lower plants. Among the latter *Aspergillus niger* has been a favourite for study, and evidence has been produced for the necessity of small quantities of zinc, copper and manganese for the growth of this fungus. R. A. Steinberg ("Some Effects of the Heavy Metals Essential for the Nutrition of Aspergillus niger upon its Growth," *Amer. Journ. Bot.*, **23**, 227–31, 1936) has now recorded the results of an investigation of the effects of iron, zinc, copper and manganese on the growth and spore production of *Aspergillus niger*. He finds that the ratio of the yield when the fungus is grown on a medium containing all these elements to the yield when one of them is omitted depends on the concentration of the various heavy metals present, but this optimum heavy metal concentration varies with the acidity of the solution, being 20 times as high in a solution of pH 8·01 as in a solution of pH 7·35. The work of the author confirms the view that these four elements are necessary for the growth and spore production of *Aspergillus*, for non-addition of iron or zinc may result in a lowering of the yield by 98 per cent. or more, while non-addition of copper or manganese may result in a lowering of the yield by 60 per cent. or more.

ZOOLOGY. By EMERITUS PROFESSOR W. GARSTANG, M.A., D.Sc.; E. B. FORD, M.A., B.Sc., and J. A. MOY-THOMAS, M.A., The University, Oxford.

ROBERT WEILL (*Thèse, Paris*, 1934; also *Trav. Zool. Stat. Wimereux*) has produced a monumental work on Nematocysts and their bearings on the Classification of Cnidaria, bringing together a mass of facts new and old on their morphology, physiology and evolution, the results of eleven years research in many waters (700 pp., 431 fig.). Indifferent to germ-layers as cnidoblasts show themselves, and specialised as they now are for the sole production of explosive organs, their pre-Cambrian prototypes were doubtless much more generalised, and must have closely recalled the special features of the particular Protozoan ancestors from which the Coelenterate body was first built up—as that of Sponges from Choanocytes. But whereas the collar-cell has retained both prehensile and digestive functions, the cnidoblast has long given up all digestive work and

comes down to us as one or another of an endless variety of purely prehensile progeny, fitted to cope with the ever-increasing complexity and diversity of their prey.

Weill distinguishes no less than 17 categories of nematocysts in a systematic hierarchy which descends from major differences of structure to minor and fluctuating features of size and proportions. He excludes the thin-walled Spirocysts of the higher Anthozoa, not so much from structural differences, since he confirms Will (1909) that their threads are really everted, but because of the unbridged gap between them and nematocysts in their reaction to dyes. The threads of spirocysts, but not the contents of their capsules, readily stain with acid dyes, while those of nematocysts can scarcely be stained at all and their capsular contents greedily absorb basic dyes—suggestive of a crystalloid content in one case, colloid in the other.

Of true nematocysts the first subdivision is based on the presence or absence of a terminal pore ("Astomo-" and "Stomo-cnide"). A familiar example of the former is seen in the cork-screw "Desmonemes" of *Hydra* (and of most Gymnoblasts and Siphonophores), which twine round the hairs of Crustacean prey. Others dilate as they evert into club-shaped sacs ("Rhopalonemes"), one type of which, with a small terminal appendage, characterises the Physophorid Siphonophores ("Acrophore"), another without it the Calycophorida ("Anacrophore"). Most nematocysts, however, have a terminal pore through which they eject the capsular fluid, often poisonous. These Stomocnides may be simple ("Haploneme") or differentiated ("Heteroneme"). Haplonemes with a uniformly cylindrical extrovert may be unarmed ("Atrich") as in *Plumularia*, Narco- and Scyphomedusæ, or spirally bristled throughout ("Holotrich") as in Hexacorallia, Zoantharia and Ceriantharia (but rarely Actiniaria); or they may be spined only near the base ("Basitrich") as in most Calyptoblasts and Actiniaria. A tapered form of Haploneme is found in *Tubularia* and many Siphonophores, thus heralding the Heteroneme, in which, on the analogy of a whip, the extrovert is sharply divided into a swollen stock (*hampe*) and a slender lash (*tube terminal*), with two main types, the "Rhabdoid" with cylindrical stock (forming the "axial body" of the unexploded cyst) and the "Rhopaloid," with the stock clavate or pyriform.

A special modification of Rhabdoid Heteronemes is the eventual atrophy of their terminal threads, thus calling for a distinction between "Mastigophores" and "Amastigophores." The former have a curious distribution: rare in Hydroids, common in Calycophores and Millepores; absent in Octanthids, widely distributed in

Actinians and Corals. Amastigophores constitute most of the " bottle-brush " nematocysts (*penicilli* of Stevenson, *Journ. M.B.A.*, XVI, 1, 1929), characteristic of Sagartiid, but not Actiniid anemones, both in tentacles and acontia. Rhopaloids always retain the thread, but their stock takes two forms, broader distally than proximally (" Eurytele ") or the reverse (" Stenotele "). Euryteles are characteristic of certain Gymnoblastea (*e.g. Clava*) and especially of Tracho- and Scyphomedusæ, Stenoteles of other Gymnoblasts (*Tubularia, Syncoryne*) and all Siphonophora except Calycophores. To the last category belongs the " typical " nematocyst of the text-books, exemplified in *Hydra*. It is really one of the most highly elaborated, the lower bristles being transformed into veritable stylets.

Different types of Cnidaria may have different combinations of these various types of nematocyst, usually 2–4 in number, the combination itself being termed a " cnidome." The bearings of all this careful discrimination upon problems of evolution and classification are obvious, and Weill gives numerous examples into which we have no space to enter. We will merely draw attention to the dominance of Rhabdoids and Spirocysts in Anthozoa, of Rhopaloids and Astomocnides in Hydrozoa and Scyphomedusæ ; to the lack of any bonds between Scyphomedusæ and Anthozoa ; and to the clear evidence of *Hydra's* true place among the Tubularian Gymnoblasts.

F. G. W. Smith (*Phil. Trans.*, B., **225,** 1935, 95–125) has reared the Common Limpet at Plymouth from artificially fertilised eggs to the early creeping adult stage, and describes clearly the cell-lineage, development of mesoderm bands, pericardium, and kidneys, origin of the nerve-centres, and the metamorphosis. Patten was wrong in ascribing a paired origin to the foot. Torsion takes place in two stages, a gradual tilt upwards of the hinder end of the shell from pressure of the growing foot below it, then a rapid twist of this end forwards up the right side of the body through contraction of a single oblique retractor running dorsally to the head on that side. A second short retractor arises behind and below this after torsion has begun (Smith might well have claimed this as the retarded and modified left retractor : it is hardly " ventral "). Both ultimately disappear, as also the velum and larval shell. The shell-muscle of the adult has no connection with the larval retractors and arises later. The young limpets were creeping actively in the third week, when the velum was cast by ejection of the trochoblasts.

Anne Hosker (*Phil. Trans.*, B., **226,** 1936, 43–188, 12 Pl.) has fully reinvestigated the development of nestling and adult

feathers in the duck and chick, with certain stages in the goose and starling, and confirms Davies's account of the formation of the rachis by "fusion of barbs" (rather of "barb-bases"). The rudiments of all nestling feathers arise as surface papillæ, which then sink into cylindrical pits ("follicles") of the skin As the ectodermal papilla sinks it encloses an axial "pulp" of mesoderm which carries an arterial loop and extends with its elongation. The ectoderm envelope thickens below the outer "epitrichium," and breaks up into longitudinal ridges fronting the pulp. These are continuously increased in length and number from a basal ring ("collar") of undifferentiated ectoderm, which contains a centre of proliferation near the middle of each side ("regions of plasmatic growth"). The primary ridges at the tip of the papilla give rise to parallel barb-rudiments (and their fringes of barbules), and are all alike except for a slight gradient in size. The pressure of new rudiments from the lateral centres crowds the bases of those above towards the mid-dorsal line, causing successive fusions of these bases with that of the most dorsal barb, which thus thickens as it lengthens, and becomes the rachis. The same process a little later is repeated in the ventral half of the lengthening feather-germ, and produces the hyporachis or aftershaft. Finally the bases of shaft and aftershaft occupy the whole circumference of the papilla (which is limited by a horny sheath within the follicle), and the two become continuous as the calamus or quill. As cellular alignment and differentiation extend down the papilla from tip to base, so the axial pulp with its ectodermal coat withdraws basewards, and cornification proceeds in the same order. Thus it comes about that at successive levels, and for brief periods, there are separate horny dorsal barbs without a rachis, then a barbed rachis with no aftershaft, then a free shaft and aftershaft with no uniting quill, and only at the finish a complete and typical feather.

Lillie and Juhn's recent theory of "concrescence" (1932)—an old friend in new guise (cf. Jeffries, 1883)—is effectively criticised by Dr. Hosker with the aid of some excellent diagrams, but she refrains from pointing out the bearings of her work on the evolutional questions raised in her first paragraph. Her results show, however, that the successive stages in the development of a feather, exemplified above, in no way repeat any possible sequence of ancestral adult characters. Furthermore the various types of nestling feather—with a stout rachis in the duck, a feeble one in the chick, and none at all in the pigeon—are seen to be just *adult feathers arrested in development* and closed off with a calamus at increasingly early stages, by reduction of yolk and shortened incubation, steps in the

evolution of chicks from no chicks, and of nidicolous from nidifugous young.

PALÆONTOLOGY.—Stensiö (*Medd. om Grønland*, XCVII, 1936) gives detailed evidence that *Phyllolepis* is an Arthrodire, not a Drepanaspid as was first thought. As it differs, however, in some respects from typical Arthrodires, this group should be divided into two sections, Euarthrodires and Phyllolepids.

Nielsen (*Medd. om Grønland*, CXII, 1936) gives a preliminary account of the wonderfully preserved new Triassic fishes from East Greenland. The Coelacanths are most fully discussed, and the course of the sensory canal of the head in *Whiteia*, which has been completely prepared, throws considerable light on the nomenclature of the dermal bones in these forms.

Brough (*Biol. Revs.*, XI, 1936) summarises the evolution of bony fishes during the Triassic. Emphasis is laid on the facts that Holostei were already in existence in the Permian, and that of the many Triassic families shown to have been independently derived from Palæoniscids, none has given rise to Holostei.

Moy-Thomas (*Proc. Zool. Soc.*, Part 2, 1935) has fully described the Carboniferous fish *Chondrenchelys problematica*. Its affinities are discussed, and its remarkable resemblances to Pleuracanths on the one hand and to Holocephali on the other are noted.

Westoll (*Geol. Mag.*, LXXIII, 1936) has described the dermal bones of the ethmoid region in *Osteolepis*. The most important result of Westoll's observations is that he has been led to uphold a theory of bone nomenclature very different from that of the Swedish investigators Stensiö and Säve-Söderbergh. His views may be summarised as follows : the bones ossifying round the sensory canal in *Osteolepis* are the earliest to be developed and the most constant, whereas the remaining bones fill up spaces in a more or less haphazard manner ; fusion between these should not be assumed without ample evidence ; in its absence the " unwieldy compound names " of some authors are dropped. The author promises a more elaborate discussion of bone nomenclature in a further work.

GENETICS.—K. Mather (*Proc. Roy. Soc. Lond.*, B., 1936, 208–27) has now clearly demonstrated that crossing-over in one bivalent is not independent of crossing-over in other bivalents in the same nucleus, and discusses this curious fact in detail. It will be realised that this phenomenon is of a different nature from the well-known " interference " effect, in which the occurrence of one cross-over reduces the likelihood of others taking place in the same chromosome. The correlation is generally negative ; a few instances of positive correlations between chiasma frequencies of bivalents were indeed

detected, but they appeared to be ascribable to environmental effects. In general, however, it seems that the total number of chiasmata in the nucleus is limited by some other condition than that controlling interference, so that above a certain frequency-level the bivalents must compete with one another in chiasma formation. It appears also that differences exist in the competitive power of the different bivalents. The precise nature of this chiasma-limiting agency requires further investigation.

PHYSICAL ANTHROPOLOGY. By L. H. DUDLEY BUXTON, M.A., D.Sc., Exeter College, Oxford.

THE ethnology of Mesopotamia has been a subject of study since the time of Herodotus, but so long as a man, even in the short journey from Jerusalem to Jericho, might fall among thieves, while the way from Europe to Baghdad lay either by Aleppo or by Bassorah, routes attended by danger not only from thieves, but from all the concomitants of desert travel, few except bold pioneers followed in the footsteps of the father of history. Since the Great War, however, and the advance in mechanical transport great advances have been made in studying the physical anthropology of this region. Two chief methods have been employed, happily linked together; first the careful preservation of ancient skulls found in the course of archæological investigations, no mean task in a land that has been alternately soaked and baked for a hundred generations since man first lived in the valley of the twin rivers, and secondly the measurement of the modern Bedouin. Unfortunately owing to the climatic difficulties we are never likely to have as many specimens from Ancient Iraq as we have from Egypt, but gradually more and more information is accumulating.

In the Field Museum's latest publication (*The Arabs of Central Iraq, their History, Ethnology, and Physical Characters*, by Henry Field, with an introduction by Sir Arthur Keith, F.R.S., *Field Museum of Natural History Anthropology Memoirs*, Vol. IV) we have first a collection of facts about the anthropometry of the modern Arab which will prove both a model for future workers in the field and a source of statistics for the laboratory student. Sir Arthur Keith's introduction is one of the first, if not the first modern statement of the physical anthropology of Mesopotamia. He has two alternative theories. One is that the peoples of Mesopotamia may represent a mixture of darker skinned Dravidians with people from the southern fringe of the Caucasians. Alternatively he suggests that the Arabs may be a pure evolutionary race and that the evolutionary centre of the Caucasians may have

extended into Mesopotamia. Not the least of the difficulties with which the anthropologist is faced is the perplexing matter of colour. Sir Arthur's two hypotheses suggest alternate solutions of this problem ; in the one case the darker colour is supposed to be due to a mixture of a fair and dark skinned race, in the other the darker colour has evolved, presumably in relation to environment. From the point of view of skeletal anatomy, however, Sir Arthur believes that no appreciable change has taken place in the population of Iraq since the fourth millenium B.C., except that possibly the brain was larger in ancient times.

A study on the same lines, but from a very different point of view, is that by Professor Sapper in the *Zeitschrift für Rassenkunde* (III, 3, 1936). In this case the author has concerned himself with the general rather than the particular problem and has attempted to throw more light on the vexed question of acclimatisation and race. He takes into special account the effect of air temperature and humidity, and believes that from the point of view of racial evolution the world may be divided into two main climatic types, *stenothermy*—the constant heat of the tropics—and *eurythermy*—the more variable conditions of the non-tropical lands. He considers that man may be divided into the races of warm and cold lands, but takes into account the effect of altitude. Unfortunately his map only gives Isotherms, and his point might have been made better if he had used, for example, one of the late Professor Herbertson's actual temperature maps. The division between the warm and cold land peoples cuts across the major racial divisions of man, since both the wavy haired and the Mongoloid peoples include tropical and even polar or sub-polar peoples, though the Negroid peoples are more restricted in their distribution. Professor Sapper shows further how the cold-land peoples acclimatise relatively easily in tropical highlands, but cannot survive in the damp tropical lowlands, while an intermediate zone of tropical arid lands is possible, but on the whole unfavourable, to cold-land folk. Exactly the reverse is true of the hot-land people. He suggests that the matter is of further importance, both nationally and politically, since the Spaniards can acclimatise in Spanish America, while the British only remain a fluctuating and thin upper stratum in India. He also draws attention to the position of white workers in Queensland and in the Panama zone. This particular problem is one of the most practical that the physical anthropologist can undertake. It has been discussed in various aspects since the time of Aristotle. In recent years most of the work done has been along the lines of physiology and preventive medicine. The general

position does now seem clear that certain races can only survive under certain conditions. If the matter of white settlement being possible in highlands can be proved it is of course most important. If, however, the suggestion that not only temperature and humidity are of importance, but also seasonal, as opposed possibly to diurnal, variations in temperature are matters in relation to which racial differentiation has taken place, then clearly the whole matter of white colonisation in tropical lands will have to be reviewed.

In addition to a general functional relation of man to his environment, a number of more detailed studies of particular parts of the human body are being undertaken by various workers. Almost every journal which publishes anthropological data devotes many pages and tables to regional studies. These papers, though individually of interest mainly either to those who are concerned with particular areas or as a source of exact information for comparative studies of the human race as a whole, hardly bear individual analysis. They are, however, of the greatest importance in the real development of physical anthropology, since they form a foundation for future work. A few years ago there was often a singular lack of data on the physical form of racial types from many areas. This gap is being rapidly filled up and the student of the relationship between man and his environment should in the future have little to complain of in the way of lack of co-ordinated data.

A particularly important paper of this type appears in *Biometrika*, XXVIII, June 1936, by G. M. Morant, assisted by Margot Collett and N. K. Adyanthāya, entitled a *Biometric Study of the Human Mandible*. The authors have explored the possible use of measurements of the mandible for estimating racial affinities. They find that the sexual differences appear to be relatively greater in the mandible than in the cranium and that within any given racial group the mandible is also more variable. On the other hand they believe that, with further data, measurements on the mandible may be of value in estimating racial differences. Not the least important part of the paper is the careful and exact account of the methods of making measurements, which have hitherto never been properly standardised. Comparatively few workers have directed much attention to the mandible, though the presence or absence of chin has been thoroughly discussed by students of prehistoric man. It is interesting to note that the present authors find that statistically there is a lack of association between the projection of the chin and other characters. The late Professor Arthur Thomson, who was concerned more with the general problems of human evolution than with racial studies in the narrower sense,

attempted to show in various papers the possible relationship between types of human mandible and functional activities. The present authors are mainly concerned with racial problems, but at the same time their paper should provide a starting point for other studies which older workers were prevented from undertaking from lack of comparative exact data. Their paper raises an interesting point. They have taken as their starting point a definitely physical horizon, the lower border of the mandible. Several anthropologists with whom the writer has had the opportunity of discussing this paper have felt that progress in anthropological work must lie to a greater extent along definitely functional lines and that therefore a better horizon would be, for example, the occlusal surface, in plain English the plane of the bite. This means rejecting any toothless mandibles, and possibly the authors tried this horizon and rejected it, though no mention is made of any such attempt. But, to show that he is fully alive to environmental problems, in the same number of *Biometrika* Dr. Morant, in collaboration with Otto Samson, discusses the very interesting and controversial point raised a good many years ago by Boas in his book *Descendants of Immigrants*. It will be remembered that Boas concluded that alien immigrants into the United States had children who differed from their parents in cephalic index, and that the cephalic index of American born descendants of immigrants tended to conform to an " American " type. Anthropologists have generally held that the cephalic index is of all characters the least open to environmental influence, and, while some were convinced by Boas, others frankly disbelieved him. While admitting the difficulties raised, the present authors, in a very careful and fully documented paper conclude that considerably larger divergencies would have to be found in order to establish the fact that head form as estimated by the cephalic index is directly modified by environment. That is the great trouble of physical anthropology, we want such a lot of data before we can establish anything, but the really important result of this paper is that, judged statistically, Boas's theory cannot be upheld, and as it has received a great deal of notice since it was originally published this conclusion, negative to a certain extent as it is, is of great importance.

NOTES

A remarkable feature of the volcanicity of Iceland, paralleled only in Kerguelen and perhaps in the Andes, is eruption under and through an ice-sheet. This type of eruption was necessarily much more frequent during the Glacial Epoch of the recent geological past, and in Iceland gave rise to an extraordinary formation—the Palagonite Formation—which is intercalated with the boulder clays that represent the normal products of glaciation. It is therefore of considerable interest to have an account of recent eruptions (1933 and 1934) in the Vatnajökull, the largest ice sheet in Iceland, by J. Áskelsson. Although authoritative notices are rare for obvious reasons, Dr. Áskelsson states that eruptions often occur in the Vatnajökull, and during recent times they have even been more frequent in this locality than in any other part of the island. The glacier-bursts (Jökulhlaup) which usually herald an eruption, and the volcanic exhalations that are emitted, produce effects that are perceptible for considerable distances and thus ensure attention and record. It would be an almost impossible coincidence for a qualified observer to be on the spot during such an occurrence in this remote and infrequently-visited region, and the facts have to be obtained by interpretation of the meagre accounts given by farmers who witnessed the eruptions or their effects, often from considerable distances, supplemented by later investigations in the theatre of eruption by a fully-equipped expedition.

There was a slight eruption in the Vatnajökull region at some time during December 1933, the position of which could not be definitely located. In March 1934 an immense glacier-burst caused floods in the Skeiðara river, bringing down vast quantities of boulders, sand, mud and icebergs. The icebergs are stated to have floated down the torrential main stream faster than a horse could gallop, and ships observed large ice-floes and pumice far out from

[1] Jóh. Áskelsson, " On the Last Eruptions in Vatnajokull." *Soc. Scient. Islandica*, XVIII, 1936, 55 pp. with 10 plates and a map.

land. The glacier-burst had fully subsided by April 3. The actual eruption of fiery lava and ash apparently began on March 30, when red glows and flames were seen as far west as Reykjavik and as far north as Akureyri. From nearer points of vantage huge tongues of flame and thick clouds of smoke were seen to rise to immense heights. Falls of ash were first noted on March 31 at places around the Vatnajökull, and spread to more remote regions during succeeding days. A second large ash fall began on April 3, after which the eruption ceased. A minor resumption of activity from the same centre was recorded as late as December 1934.

Áskelsson's investigations during the two subsequent expeditions showed that the eruptions came from the old centre in the western part of the Vatnajökull to which the name Grimsvötn has now been definitely attached. The volcanic focus lies on the floor of a fault-valley the tuff walls of which on the west are almost vertical. This valley is entirely surrounded by the Vatnajökull ice. The rift is about 10 kms. long by 6 kms. wide, its long direction is N. 35° E, and its height above sea level is 1300 m. The whole area constitutes a huge hole in the surface of the ice sheet which, in the intervals between eruptions, is again filled with ice, snow and water, forming the lake or lakes known as Grimsvötn. The ice streams into this depressed region from all sides, but the subterranean heat makes the depression permanent although at times it may be completely covered with a thin sheet of ice. Áskelsson states that in the south-western part of the Vatnajökull there are many such basin-like hollows separated by rounded ice-domes. The ice covering these features is thin, and is moulded on the contours of the underlying surface. The numerous glacier-bursts that occur in this region are caused by a sudden rise in the volcanic temperature, but are not all followed by actual eruption.

The N.E. to S.W. zone of volcanicity in which the Grimsvötn centres lie is continued beyond the margins of the Vatnajökull to the north-east in the well-known Kverkfjöll vents, and to the south-west in the famous Laki fissure from which was emitted the colossal eruption of 1783.

Hofmann Memorial Lecture [1] (F. P. D.)

It is a number of years since the inauguration of an annual Huxley Memorial Lecture at the Imperial College of Science and Technology. The decision has recently been made to have the Huxley Lecture in alternate years and in the intervening years to

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[1] Macmillan & Co., 1s. net.

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honour distinguished past Professors of the Royal College of Science by memorial lectures.

The Hofmann Memorial Lecture, delivered by Sir Gilbert Morgan, F.R.S., on Monday, May 4, was the first of the lectures under this new scheme. Its interest extends far beyond those who were privileged to hear it, and is not confined to old students of the College. The story of the genesis and early days of the Royal College of Chemistry is undoubtedly of great interest, but the account of Hofmann's researches and their influence on the British Dye Industry makes a unique contribution to the history of the Industry. We are reminded of the work of Hofmann on aniline, the amines and magenta, of that of Mansfield in obtaining pure benzene and toluene from Coal Tar, and that the material on which he was working at the time of his fatal accident—a still containing inflammable liquid overflowed and a fire ensued—was supplied by Messrs. Read, Holliday & Sons of Huddersfield. Sir Gilbert gives a vivid account of the early history of the firm and its subsequent development. He then turns to the Thames Valley Colour Works, "the birthplace of the coal-tar industry," for here W. H. Perkin senior—himself a student of the College and pupil of Hofmann—discovered aniline purple or mauve, and started its manufacture.

Altogether a fascinating sixty pages and well worth reading—fascinating because the story is told by one who is himself an expert in the technique of organic research and who has had close contact with the dyestuff industry.

Miscellanea

The honours list published on the occasion of the King's birthday included the names of the following :—*Knights* : Prof. C. S. Hicks, professor of human physiology and pharmacology in the University of Adelaide ; Dr. G. T. Morgan, director of chemical research, Department of Scientific and Industrial Research ; Dr. J. Morton, for services to the dye and colour industries ; Prof. A. C. Seward, professor of botany in the University of Cambridge ; Dr. E. O. Teale, mining consultant to the Government of Tanganyika Territory. *C.B.* : Dr. G. Rotter, director, Explosives Research Branch, Royal Arsenal, Woolwich ; *C.M.G.* : Dr. H. H. Scott, director of the Bureau of Hygiene and Tropical Diseases, London ; Dr. F. J. F. Shaw, director, Imperial Institute of Agricultural Research, India.

Sir Thomas Middleton has been elected to be a fellow of the Royal Society under the Statute which permits the election of

persons who "either have rendered conspicuous service in the cause of science or are such that their election would be of signal benefit to the Society." Prof. S. Freud of Vienna, Prof. Jost of Heidelberg, Dr. F. A. Vening Meinesz of Utrecht and Prof. H. Weyl of Princeton have been elected foreign members of the Society.

Sir C. V. Boys, Sir Henry Dale and Prof. F. G. Donnan have been elected honorary fellows of the Royal Society of Edinburgh.

His Majesty the King has approved the award of a Founder's medal of the Royal Geographical Society to Mr. G. W. Murray, director of desert surveys, Egypt and of a Patron's medal to Major R. E. Cheesman for his services to geography in Ethiopia, more especially in the Lake Tana region.

Sir Robert Hadfield and Dr. C. E. Guillaume, director of the International Bureau of Weights and Measures, have been elected honorary members of the Société de l'Industrie Minérale.

Dr. E. J. Allen is retiring from his post as director of the Marine Biological Association Station at Plymouth after forty-two years' service. He is succeeded by Dr. Stanley Kemp formerly director of the *Discovery* expeditions to the Antarctic.

We have noted with regret the announcements of the deaths of the following well known workers in science during the past quarter : Prof. M. Baratta of the University of Pavia, geographer and seismologist ; Prof. Margaret Brown, botanist ; M. Louis Bleriot ; Prof. F. Cavers, ecologist ; Prof. W. E. Dalby, F.R.S., engineer ; Sir George Hadcock, F.R.S., director of the Armstrong-Vickers Co., ballistics expert ; Sir William Hamer, epidemiologist ; Dr. H. J. Hansen of Copenhagen, zoologist ; Prof. A. P. Karpinsky, president of the Academy of Sciences, U.S.S.R., palaeontologist ; Lieut.-General Sir Alfred Keogh, rector of the Imperial College of Science, 1910–1922 ; Mr. A. H. S. Lucas of Sydney, naturalist and mathematician ; Prof. J. H. Müller of the University of Pennsylvania, chemist ; Sir Charles Nathan, member of the Executive Council of the Australian Council for Scientific and Industrial Research ; Mr. W. Newbold, statistician and biologist ; Prof. A. A. Noyes, physical chemist ; Dr. F. J. F. Shaw, whose name appears in the Honours list above ; Sir A. Theiler of South Africa, veterinary expert ; Dr. J. D. Unwin, anthropologist ; Sir Henry Wellcome, F.R.S., founder of the Wellcome Historial Medical Museum.

In a letter to *Nature* (June 20), Dr. W. N. Bond states that measurements of the viscosity of air by a capillary tube method have confirmed the value obtained by Kellström last year. Kellström's

result was obtained by a rotating cylinder method and Bond's confirmation of it clears up the mystery of the discrepancy between Millikan's value of e and that given by the X-ray crystal grating method. Millikan's data combined with Bond's value of the viscosity of air gives $e = 4\cdot816 \pm \cdot013 \times 10^{-10}$ e.s u. which agrees well with the X-ray value. In the same journal (June 13) I. Adamozewski of Warsaw, gives reasons for supposing that the residual electrical conductivity of carefully purified liquid dielectrics is due to ionisation produced by cosmic rays.

Miscellaneous Publication M122 of the National Bureau of Standards of the United States describes the early history of the standards of length and mass in that country. The need for such standards was considered by the Continental Congress in 1783. In 1790 Thomas Jefferson submitted a report to Washington's first Congress containing proposals for a decimal system based on the length of the seconds pendulum in latitude 45° and in 1792 a committee of the Senate recommended " That the standard for weights and measures . . . be a uniform cylindrical rod of iron of such length as in latitude 45° in the level of the ocean, and in a cellar of uniform natural temperature, shall perform its vibrations in small equal arcs, in one second of mean time." One-fifth of the length of this rod was to be called a foot and one-tenth of the foot was to be called an inch. The unit of weight was to be the ounce, the weight of a cubic inch of rain water measured and weighed in a cellar as before. Nothing was done however and the matter came before Congress again and again in the following years. In 1821 John Quincy Adams submitted a most comprehensive report and the committee which considered it recommended that the unit of lengths and mass should be the English yard of 1601, and the English pound avoirdupois of which copies in " platina " were to be made. Again no effective action was taken, but the Mint Act of 1828 decreed that the troy pound, designated as " the imperial troy pound of Great Britain " in the English Act of 1824, be used as the standard weight for gold and silver and a brass copy of the English standard verified by Captain Kater was taken as the primary standard. In 1832 by Order of the Secretary to the Treasury standards of length and mass were adopted for custom-house purposes. The unit of mass, the pound avoirdupois, was defined as 7000/5760 of the troy pound used by the Mint, and the unit of length, the yard, was taken as the distance, at 62° F, between the twenty-seventh and sixty-third inch marks on a brass bar 82 inches long made by Troughton of London for the Coast and Geodetic Survey, which had been sent to

the United States in 1813. The necessary copies of these standards were issued in 1839.

In the June number of the *School Science Review* Mr. J. A. Storer of Hemsworth Grammar School, writes in support of the " Real-is-Positive " sign convention in geometrical optics. The writer of this note has recently had an opportunity of looking over a large number of scripts from a school examination in which, in the course of one question, the candidates were asked to state the convention they were accustomed to use. The vast majority used the old-fashioned and awkward convention which attributes a negative sign to the focal lengths of convex lenses and concave mirrors ; an almost negligible number that favoured by Mr. Storer. This convention is the " non-Cartesian system for elementary work " recommended in the *Report* of the Physical Society on the *Teaching of Optics* where it is unfortunately complicated by measurement along a chief ray rather than along the axis. Its origin is unknown to the writer who first heard of it nearly thirty years ago from the late Prof. Trouton. Mr. Storer states that it is " used chiefly in this country by Dr. Searle " and it is becoming known as " Searle's convention." Its introduction follows naturally if the lens is introduced as a device for " bending " rays of light when " bending " towards the axis is associated with a positive sign and experience shows that students using it make far fewer mistakes than those who are taught the usual school text-book convention.

The *Report of the Government Grants Committee* for the period 1929–30 to 1934–35 constitutes a most valuable and sympathetic review of University activities in England, Wales and Scotland. The most important section is that which discusses Needs and Problems under the headings : Student Problems, Buildings, Staff Problems, Libraries and Other Questions. Only a very few of the points raised can be mentioned here. The advantages of community life in College or Hostel are properly stressed for there can be no doubt that the student of a non-residential University whose share in its life is limited to attendance in the lecture-room, the laboratory and library misses the most valuable things which it can give him. Unfortunately living in a hostel costs more than life in lodgings and, of course, much more than living at home so that endowments for hostels are perhaps the most pressing need of the modern Universities. There is a comment on Health Services which deals in particular with the advantages of schemes of health insurance. In fact, a large college has a real need for a health

officer whose services are available to every student without charge. Considerable space is devoted to a discussion of lectures and lecturers, a problem which is more serious than it would be if ability to lecture was regarded as a more important part of the equipment of a university teacher. Consideration of numbers and employment reveals the surprising fact that while in the ten years 1924–25 to 1934–35 the proportion of university students to the total population in England has increased slightly from 1 in 1,147 to 1 in 1,013 and in Scotland from 1 in 500 to 1 in 473, in Wales it has increased from 1 in 1,005 to 1 in 741 ! It is not surprising therefore to learn that in England the employment problem seems to be less serious than in either of the other two countries. Discussing employment the *Report* comments rather strongly on the fact that so little use is made of university graduates in the Local Government service. The position is in fact grotesque. The Local Authorities encourage the universities by grants and help their best pupils to enter them by scholarships, and then, as the Report says, make no " endeavour to reap a fair share of the harvest they have sown."

Dealing with staff matters the Committee give their approval to the Establishment System which has been adopted " in one or two of the larger London institutions." This involves a definite proportion between three grades of the non-professorial staff, and a definite fund to be used only for salary improvements. Of these grades the third or lowest is the normal grade of entry, appointments to it are temporary and promotion to the next grade depends both on merit *and* the existence of vacancies in the permanent establishment. It is here that difficulties arise. The scheme can be financially sound only if the number of persons in the first and second grades is fixed at an appropriate figure and it may happen that a teacher who would otherwise be retained has to give up a University career because there is no vacant place available. However, the advantages of the scheme from the point of view of the University or College as a whole are very great and, as the Committee point out, " it is not unduly harsh to the individual if the decision is made early enough in his career." The importance of research receives due recognition, but the paragraph in which it is discussed closes with this very significant sentence : " On the other hand, the number who are capable of delving into some abstruse, but not necessarily very significant aspect of learning is considerable, and we think it would be singularly unfortunate if research of this latter character, of which perhaps there is already too much being done all over the world, were to be rewarded in preference to work which

bears fruit, not in an accumulation of publications, but in the inspiration of teaching."

The *Report* closes with a detailed statistical analysis of students, staffs, income and expenditure. One item only can be noted here— that relating to the number of students in receipt of assistance at the various Universities.

The percentage of such students at Oxford and Cambridge increased from 38·2 to 43·4 during the period 1928–29 to 1934–35. At London, excluding the Medical Schools (at which very few students receive grants) the corresponding figures are 34·0 and 34·3 ; at the Provincial Universities 52·5 to 45·7, in Wales 62·2 and 57·0, and in Scotland 52·5 and 45·7. Thus only at Oxford and Cambridge has there been any considerable increase, an increase which becomes almost startling if the data are confined to postgraduate students alone for here there has been a rise from 39·8 to 66·0 !

ESSAY REVIEWS

THE PREHISTORY OF SCIENCE. By CHARLES SINGER, D Sc.
D.Litt., M.D., Professor of History of Medicine in the University of
London, University College. Being a Review of **Origins and
Development of Applied Chemistry,** by J. R. PARTINGTON, M B E.,
D.Sc. [Pp. xii + 598.] (London: Longmans, Green & Co., 1935.
45s. net.)

THE title of Professor Partington's book is misleading. It would
be very regrettable if, through his modesty, its full significance were
missed by working archæologists, the class that can most directly
profit by it. The range of the book extends far beyond that of
technical chemistry. Perhaps the title " Materials for a History
of the Technology of the Ancient Civilisations " with a subtitle
" with special consideration of the chemical nature of the processes
involved " would go nearer to describe this important work. The
volume includes, however, a vast mass of material quite outside even
these limits. Thus it contains much information on such subjects as
workshops, libraries, writing materials and instruments, textiles,
foods, labour conditions, fuels and production of fire, metrology,
medicine and medical practice, mining, building materials, pottery,
cosmetics, perfumes, incense, mummification, leather and its uses,
and a hundred other topics gathered together into a condensed
and easily accessible form. Professor Partington has, in fact, given
a systematic account of the sources, production and uses of materials
employed by the Civilisations of the Nile Valley, of Mesopotamia,
of Syria and Palestine, of Persia, of the Aegean (Minoan) area and
of Asia Minor, beginning with the earliest times and terminating
with the end of the Bronze Age which, in the areas under discussion,
was somewhere in the neighbourhood of 800 B.C.

It is a commonplace that the sheer accumulation of knowledge
in our age forces an increasing specialisation upon an unwilling
scientific public. The familiar distinction between the philosopher
and the man of science, that whereas the one by widening his field,
comes to know less and less about more and more, while the other,
constantly restricting and dividing the area of his investigations,
knows more and more about less and less, is a reflection of these

unhappy circumstances. It is difficult to believe that such a state is final, or that science is either a device for employing an ever-increasing number of professors or a means of making the world as a whole ever less intelligible. If science is really to provide a key to the sum of our thinking either about our world or about ourselves, a way of synthesis must, sooner or later, succeed to our present habit of scientific subdivision. For synthesis it is necessary to trace back the sciences to the stages at which they separated from each other and from the commoner kinds of organised knowledge that we term "technology." It is to this historical investigation that Professor Partington has been, for many years, applying his scientific and technical experience.

The study of the history of science, like every other study, has itself developed its own peculiar technique and there has thus been a tendency to entrust the history of science to its own special functionaries. While this process has not yet gone very far and while it is still necessary for historians of science to have had a training in scientific methods and principles, it is nevertheless true that most of them are men who have long ago abandoned the active pursuit of science for their own particular studies. On this ground —if on this ground alone—we should welcome the contribution of Professor Partington. His latest book is perhaps unique in our generation as a first class piece of historical investigation covering a wide field, produced by one who is actively engaged in the prosecution of scientific research and scientific teaching. Moreover, not only does it open out a vista which, if not entirely new to the archæologist, has never before been displayed with such range and clearness, but also, by combining the archæological and historical methods with those of the physical sciences, it is itself a liaison enterprise and therefore of a type greatly needed in an era of progressive —or at least advancing—specialisation.

All sciences pass through stages of great accumulation of data from which only very few important generalisations can be drawn. Chemistry, the very type of what we may call the 'progressively ordered' sciences, was in this stage about a hundred and fifty years ago. An immense experience of chemical processes and an endless number of seemingly unrelated data had accumulated.· They had become such a tax upon the memory that the other faculties of the chemist were liable to be in abeyance. From this slough the science was rescued, largely by the efforts of Lavoisier. Archæology is in somewhat the position now that Chemistry was before Lavoisier. Thus comprehensive attempts, such as those of Professor Partington, to apply exact experimental methods to the

correlation of archæological data, especially from widely separated fields, are of great significance. They are the more so in view of the fact that those equipped with the necessarily diverse kinds of knowledge to make such surveys are extremely few.

During the last twenty years an extension backward of our knowledge of the early civilisations of the Near and Middle East has become a matter of common knowledge. Many attempts have been made to establish definite relations and sequences among the archæological discoveries that are pouring in from a great variety of sites, stretching from Egypt eastward to Mohenjodaro in the Punjab and Anau in the Soviet Turkman areas of the Caucasus. These efforts have hitherto met with very little success. The rapid accumulation of archæological observations has given rise to a confused and changing picture in the minds of most readers. The line of attack adopted by Professor Partington is a comparison of the highly technical products of these ancient civilisations, based on the exact findings yielded by the application of scientific analytical methods. This has revealed that, even at the earliest date that can be safely ascribed to the civilisations that have yet been explored —say about 4000 B.C.—there was already, wide-spread over the Near East, a very highly developed technology that has seldom since been excelled. Such development implies a very long history anterior to the fourth millennium. It has been suggested from the pottery that the Anau civilisation, which may be as old as the Egyptian, was part of a great culture of a yet earlier period which spread from central Asia to the Iranian plateau and on to Syria and Egypt before 4000 B.C. For a verification of this conclusion we must await further archæological exploration of that difficult and inaccessible region. When this becomes possible, the well arranged mass of data set forth by Professor Partington will form one of the bases of comparison on which the verdict must be formed as to the dates and directions of movement of civilisation in the near East.

Two main positive conclusions emerge from Professor Partington's great work.

Firstly, there is an amazingly high standard and a remarkably close correlation—" similarity " is perhaps hardly the term—between the technical products of the divers bronze age and earlier cultures from the whole vast and varied area between the Mediterranean coast of Syria and Central Asia. However vague our knowledge of the ethnic movements in this quarter of the earth, there can now be but little doubt that all the ancient cultures there were related to each other. The doctrine of the 'indivisibility of civilisation' emerges reinforced from this great synthetic effort.

Secondly, it must now be realised that in Professor Partington's own words " the knowledge of materials in the Classical Period, which usually forms the starting point for the historian of science, is almost wholly derived from much older cultures. It represents, in many cases, not an original and vigorous development of national genius, but a decadent form of craftsmanship which had existed for a period often as long as that which now separates us from the best days of Greece and Rome. Just as the modern industrial period has ruined the traditions of craftsmanship, so the irruption of the people of the Iron Age broke the continuity in a traditional use of materials which had developed, almost without a break, from the period of the Stone Age."

This may seem to many a revolutionary doctrine. It is, however, established on a firm basis and the time has arrived when, in the reviewer's opinion, this new setting of the classical culture should be recognised in education. The methods and indeed the principles of classical education are based on the view that civilisation is a far more isolated phenomenon, is a trunk with far fewer roots, than the state of our knowledge now justifies. In the study of civilisation the past is living and there are no dead, nor can any age or literature or culture be treated as the sole source of our own civilisation. A real study of the 'humanities' would treat our civilisation as a part of universal history and in the study of the development of man " no part," as Professor Partington rightly says " is more significant than that concerned with the use of materials."

So monumental a work cannot be produced without some errors. Those that the reviewer has detected are few and unimportant. Attention should, perhaps, be called to the fact that the Aegean material discussed is almost entirely Minoan and refers very little to the numerous Cycladic and Helladic finds. On page 335 an ' Early Minoan ' dagger is described as having been found in an ' Etruscan ' grave. Can this grave have been really Etruscan ? For that civilisation is very much later than Early Minoan. On page 339 Troy II is described as a ' manufacturing town,' but Troy II was a small site and it is doubtful if any of the series of Troy towns was much more than a fortress or, at most, an emporium. On page 338 the ' arches ' of limestone described from Neolithic Malta were surely corbelled arches. On page 340 Troy II and Troy VI have somehow become confused. These are minute blemishes in a magnificently sustained effort.

The indexing is admirable but there are two additions that would greatly increase its usefulness. One of these is an adequate analysis of the headings. The design of the book is necessarily very intricate

and such an analysis would supplement the somewhat inadequate table of contents and would help the reader to find his way in the text. The other is a summary bibliography of the sources used. There cannot fail to be some omissions in a book which must have taken many years to write and many months to pass through the press. If Professor Partington could devise a method of summarily displaying the sources of his 25,000 references, the reader could ascertain at a glance whether any particular source had or had not been consulted. The usefulness of the book for reference purposes would thus be greatly increased. Since it is likely to remain for many years a standard reference work on the subjects of which it treats it may be hoped that its author will see his way to bringing out a short supplement containing these additions. The book itself will always remain as a classic of the prehistory of science.

VERSATILITY. By Sir Charles Martin, C.M.G., D.Sc., F R S. Being a Review of the **Collected Scientific Papers of Sir William Bate Hardy, F.R.S.** [Pp. xii + 922, with 15 plates and numerous figures] (Cambridge : at the University Press, 1936. 63s. net.)

" One man in his time plays many parts." William Hardy certainly did. The collection of his scientific papers which has recently been published under the auspices of the Colloid Committee of the Faraday Society bears witness to it.

What an astonishing range of subjects was covered by these researches ! The earlier of the 59 papers are studies in morphology and the later ones of the physics of lubrication. In the intervening period Hardy made researches in the domains of histology, physiology, biochemistry, physical chemistry and radiology. The connection between all these various enquiries is not obvious. What he worked at was determined by opportunity, the sympathetic influence of comrades and chance observations which set him wondering.

On re-reading these papers it is possible, however, for one who enjoyed his friendship for many years, to trace how he came to embark upon most of them. Early in his career the multiplicity of chemical transactions accomplished in an apparently formless cell-plasm, containing 80 per cent. of water, and the observation of a cell dividing, rooted his attention upon the nature of the material in which such arresting phenomena occurred. Later, the study of the coagulation of blood and a critical examination of the procedures he, and other histologists, were employing to fix tissues for microscopical examination also contributed to lead him to undertake the fundamental study of colloids and gel-formation. To do this

the methods of biological enquiry had to be put aside and those appropriate to physical chemistry learnt and practised.

Between the series of researches on ampholytic colloids and those on lubrication there is distinct relationship in that they are both concerned with the properties of matter at boundary surfaces. The jump from one to the other is not so wide as might appear, but whether Hardy would have made it, had it not been for two homely observations which excited his curiosity, may be doubted. The first was that some samples of oil which he used for his motor-bicycle extended over the surface of water whereas others remained heaped up, the second that a cup of tea with some of its contents spilt into the saucer possessed greater stability than when the surfaces in apposition were dry. As a deep-water yachtsman too, he was interested in the influence of a film of oil in preventing the breaking of waves and pondered over the way in which it produced " calms."

Hardy was a versatile genius ; his curiosity was catholic and no sort of intellectual adventure came amiss to him. Most men who spread their interest and attempt researches in so many directions jeopardise their careers. Not so Hardy, there is nothing amateurish in any of his work. He possessed extraordinary energy and much resourcefulness. He was loved by his scientific comrades whose criticism he invited and enjoyed and this kept him on the right paths when he advanced into territory with which he had had little previous acquaintance. Thus Hardy reached the growing edge of knowledge in several places, usually considered rather far apart, and extended it so notably that he gained the unique distinction of being invited by the Royal Society to give both the Croonian and the Bakerian lectures, honours bestowed by the society for important discoveries in Biology and Physics respectively. The principal papers in the collection can be grouped as follows : morphological studies, made between 1890 and 1892 ; observations on the nature and behaviour of the wandering mesoderm cells as seen in the different phyla of the animal kingdom, including the white blood corpuscles of vertebrates, 1892–1899 ; on electrolytic colloids and gels, 1899–1912, and a scientific study of lubrication and the phenomena exhibited by matter at the boundary surface between a solid and a liquid. In addition, nine lectures are included in which either work he has been doing is brought together and summarised, or the present state of knowledge on some general subject as " the physical basis of life," or " the problems of living matter," is philosophically reviewed.

The first scientific paper written by anyone who attains eminence is always interesting to the biographer and Hardy's first effort is particularly so because it had an influence in directing his subsequent

career. It was a straightforward piece of descriptive work about the development of a hydrozoan. It was a good performance in which he showed his mastery of recently introduced histological methods, and that he was an accomplished histologist. This led to his recruitment by Michael Foster as an assistant in the department of physiology at Cambridge which was then, and still is, responsible for instruction in histology. This was the time of the renaissance of physiology in Britain, and in Foster's laboratory he came into daily contact with a group of exceptionally talented experimental investigators whose influence in moulding his mind he was wont gratefully to acknowledge in after years.

The subject of the second of his morphological papers, the nerve cord of crustacea, was suggested by Gaskell, who had just afforded a rational interpretation of the sympathetic nervous system of mammalia, and was then puzzling over the origin of vertebrates, and particularly how the relative positions of the nervous system and of the alimentary tube in invertebrates became reversed in vertebrates. Hardy's observations and experiments showed that in Branchipus each ganglion was divided into anterior and posterior portions connecting respectively with motor and sensory nerves. Further, that the nerve cells supplying the motor fibres to the branchial appendages had a separate position in the ganglia, indicating a complete differentiation between somatic and visceral nerve supply in a primitive arthropod similar to that which Gaskell had discovered in vertebrates. A third piece of morphological work was the description of the minute structure of the alimentary canal of Daphnia and of the process of intracellular digestion in this water flea.

From 1892 to 1898 Hardy's published work was mostly histological. The seven papers on " Wandering cells " present together a comprehensive study of their character and behaviour in different classes of animals. The first paper deals with the crustacean blood corpuscles and records the interesting observation that, in these animals, clotting of the blood was initiated by a fibrin ferment set free from one type of wandering cell in which it is stored in the form of granules. When the blood is shed these cells explode as they come into contact with foreign matter.

The researches described in the subsequent papers were influenced by the technical advances in staining introduced by Ehrlich, which enabled a differentiation amongst wandering cells to be made with greater precision, and also by the discovery of their phagocytic activity by Metchnikoff. The significance of phagocytosis for the defence of higher organs against invasion by bacteria had recently

been proved by its discoverer, so that the wandering cells of the body had acquired additional importance. In these investigations, some of which were undertaken in collaboration, Metchnikoff's discoveries were confirmed and, in some directions, extended. The papers did not receive the attention they deserved. One reason for this may be that the fullest of them was honourably buried in the Transactions of the Royal Society and the funeral was postponed for two years. The work was more than a contribution to the theory of immunity ; it was an extension of knowledge of the different types of wandering cells as they occur throughout the animal kingdom. The papers are full of interesting observations but, curiously, owing to the technique employed and the animals used, the intense activity of the polymorphonuclear leucocytes, which constitute 75 per cent. of the " wandering cells " of the blood of man and other mammals, was missed.

The last paper of this histological series " on the structure of cell protoplasm " is a criticism of the extent to which the plasma of cells had been endowed by histologists with intricate structure from appearances seen in specimens fixed by protein precipitants. By operating upon albuminous solutions and jellies with a fixative, Hardy succeded in producing many of the appeareances supposed to indicate a definite structural arrangement. By placing the colloid or gel under stress at the time of fixation, pictures, some of which had a remarkable resemblance to those seen in cells, were presented.

These observations on the fixation of colloids diverted Hardy's attention for the time being from the cell, and a critical study of a piece of histological technique became the bridge across which he passed to his researches upon colloidal systems.

As material for his earliest study of colloidal solution, which bears the misleading title " On the coagulation of proteid by electricity ", he employed diluted egg-white in which the protein had been denaturated by heating the slightly alkaline solution. By simple experiments it was shown that the submicroscopic particles of protein remained in suspension owing to the electric charge upon them. In acid solution they were positively charged, in alkaline negatively. If the solution were neutralised the colloid suspension was destroyed and precipitation occurred, the precipitate being found devoid of charge. The full meaning of these experiments was not immediately apparent. They were, however, the beginnings of knowledge of amphoteric electrolytic colloids. In the next few years Hardy made many discoveries about colloidal solutions in general, and in the papers on globulins and on the conditions which

determine the stability of various hydrosols the significance of the early observations is explained.

Hardy's work on colloids between 1900 and 1905 was of fundamental importance and gained for him recognition as a pioneer in the development of this department of physical chemistry. It was to some extent summarised in his Croonian lecture to the Royal Society in 1907, and much better in his article on electrolytic colloids in the Van Bemmelen Memorial Volume in 1910.

Having become immersed in molecular physics he felt the want of more mathematics and worked hard to acquire them. Henceforth, as in his " General theory of colloidal solutions " (1912) and the paper on the application of dynamical similitude to molecular physics (1915), he made more use of forms of mathematical expression and became less intelligible to most of us.

The papers on the tension of composite fluid surfaces are studies of films of various oily substances upon water. The subject had been much worked at ; nevertheless, he succeeded in adding some new facts and put forth interesting suggestions to explain them.

By this time he had become interested in films of all kinds and the state of matter at boundary surfaces generally. It was curiosity as to the explanation of the greater stability of the aforementioned cup of tea on a wet saucer that set him on to the investigation of lubrication. The remaining papers in the volume are concerned with the properties of liquid films on solids and their effect on friction.

There are two kinds of lubrication. In one, the layer of lubricant is sufficiently thick to permit of movement between the particles of the liquid. In the other, as much as possible of the lubricant has been squeezed out and the layer is but a few molecules in thickness and attached to the faces of the metal. This Hardy called " boundary lubrication." In practice it is the more important, for pressure on a bearing expresses the excess lubricant from between the opposing surfaces. Nevertheless, until studied by Hardy this type of lubrication does not seem to have been investigated to any great extent. His researches showed that the lubricant adhered to the metal surfaces, thereby substituting the lesser cohesion of a compound film for that of metal to metal, that, as in films of one liquid upon another, the film was of the thickness of a few molecules only and that these were orientated in the film. Both adhesion and effect on friction were discovered to be dependent upon the chemical composition of the lubricant. By experimenting with a variety of substances and a series of different long chained carbon compounds the influence of chemical composition of the lubricant on static

friction was revealed. In the Bakerian lecture to the Royal Society, and that on problems of lubrication given at the Royal Institution, much of this work is summarised.

Having discovered the general principles governing boundary lubrication, Hardy was able to reverse the object of his experiments and, instead of directing them to ascertain the effect of the constitution of films on friction, to use measurements of friction to investigate the properties of matter at boundary surfaces between solids and liquids. In this way, he established the continuity between their properties and those of liquid films on liquids.

The philosophical lectures and addresses gathered into the volume, although some of them were published 30 years ago, make interesting reading. That on " the physical basis of life " addressed to a lay audience in 1906 brought Huxley's famous essay with the same title up to date and was published in an early number of SCIENCE PROGRESS. The Guthrie lecture on problems of living matter in 1916 explains to physicists some of the extraordinary accomplishments of the living cell and that, although more and more of them can be thought of in terms of physics and chemistry, most, for the time being, defy any such interpretation. There is still the tone of hopefulness that more knowledge of the colloid state will help, but in the Abraham Flexner lecture delivered in U.S.A. in 1931, in which the peculiar properties of protoplasm are again the subject, he is oppressed by the difficulties in interpreting its marvellous and apparently purposeful activities and arrives at the conclusion that the physical and chemical improbabilities are so great that the hypothesis of a special vital force and the search for it are as likely to lead to our goal as any other.

Vitalism, however, he considers to be a dangerous flag to fight under and thinks the biologist had best continue to take the findings of physics and chemistry and apply them to the riddle of this " impossible elusive living slime."

Posthumous collections of scientific papers are often but literary tombstones. They satisfy friendly sentiments and remind of achievements but, alas, soon are neglected. Some, as the present volume, serve a further and more practical purpose by bringing together the scattered papers of one who has made notable contributions. To the physical chemist who is interested in the peculiar properties of matter at boundary surfaces to have Hardy's papers on colloids and lubrication gathered together will be a boon. Professor Rideal who edits the volume and has written a short preface is to be thanked for providing a good index and a beautiful photograph of Hardy as we like to remember him.

A A

REVIEWS

MATHEMATICS

The Poetry of Mathematics and Other Essays. By DAVID EU-
GENE SMITH. The Scripta Mathematica Library, No. 1. [Pp. vi + 91.]
(Yeshiva College, New York: Scripta Mathematica, 1934. Cloth,
75 cents; paper, 50 cents.)

**Mathematics and the Question of Cosmic Mind, with Other
Essays.** By CASSIUS JACKSON KEYSER. The Scripta Mathematica
Library, No. 2. [Pp. vi + 121, with 1 plate.] (Yeshiva College, New
York: Scripta Mathematica, 1935. Cloth, 75 cents; paper, 50 cents.)

THESE two little "mathematical books for non-mathematicians" form the
first representatives of a series consisting of articles dealing with "the history
and philosophy of mathematics, and with its relations with the other great
activities of the human spirit." The series is designed "to furnish, at a
nominal price, material which will interest not only teachers of mathematics,
but all who recall their contact with the subject in their school or college
days. The articles are not themselves mathematical, but they relate to
lines of interest which mathematics suggests." "Neither extensive mathe-
matical knowledge nor a high degree of mathematical aptitude is required
for reading the essays understandingly. All that is required is a little logical
acumen, a reasonable facility in the reading of concatenated discourse, and
a fair measure of attention to what is said." The subjects dealt with in
the volumes before us include the relation of mathematics to poetry, religion,
law and general culture, and the mathematical interests of certain politicians.
The description of later volumes indicates that the series has a still wider
scope. Space forbids comment on the essays themselves: suffice it to say
that they are well worth reading, and that the project of issuing such volumes
at so low a cost is worthy of all praise.

H. D.

The Collected Works of George Abram Miller. Vol I. [Pp. xi +
475.] (Urbana: University of Illinois Press, 1935. $7·50.)

DURING the course of the forty years of his active scientific life Prof. G. A.
Miller produced some 400 original memoirs on the theory of finite groups.
When, in 1931, he retired from active teaching, their republication as a
collected whole was suggested, not merely to serve as a visible memorial of
Prof. Miller's contributions to science, but even more to perpetuate in future
generations that influence which he exercised over his contemporaries. The
size of the present volume, which contains some sixty items dating from
1894 to 1900 fills one with wonder at the energy of the man who, apart from

his teaching duties, his original work and the guidance of his many pupils, found time to write literally hundreds of reviews and expository articles, and even, as the present writer can testify, to pen a word of encouragement to anyone whose work touched even lightly upon the vast domain that he himself ruled.

Three of the articles in this volume were specially written to show how Miller's work falls in with the general advance of group theory at the end of the last century.   They are : (No. 1) " Historical Note on the Determination of all the Permutation Groups of Low Degrees " ; (No. 15) " Historical Note on the Determination of Abstract Groups of a Given Order " ; (No. 62) " History of the Theory of Groups to 1900."   The latter is a particularly valuable account in that it clears up the obscurities and ambiguities of earlier contributors to the theory.   Another set of three papers (Nos. 9, 10, 11), extracted from the *American Mathematical Monthly* of 1895–6 may justly be said to constitute the " most complete existent treatise on the determination of substitution groups, and the only one of any considerable degree of completeness, in the mathematical literature " (Preface).

The aim of the earlier papers was to continue the enumeration of permutation groups, intransitive, transitive and primitive begun by Kirkman thirty years earlier.   By 1897 the enumeration had made considerable progress, but the increasing difficulty of the problem with rising degree had demanded new lines of attack which in turn led to new general theorems, such as those on transitive groups whose orders are the products of three prime numbers.   Among the noteworthy papers in the volume are those on the limit of transitivity of the multiply transitive groups that do not contain the alternating group, on an extension of Sylow's theorem, on operation groups of order p^α (p a prime number), several papers on simple groups and on Hamiltonian and other special groups.

The period covered by these papers was one of rapid development, when the unbounded enthusiasm of its devotees inspired them with ambition to shift the whole basis of mathematics on to group theory.   But their expectations failed to be realized, and the nineteenth century closed with a saner outlook.   What the opening years of the present century were to reveal in group theory will be shown by the later volumes of this collection, whose appearance will be eagerly awaited.   The University of Illinois, and particularly the committee in charge of the publication, are most heartily to be congratulated, and thanked for the book which, bringing the works of Prof. Miller into a convenient form, will smooth the path of future workers in group theory.   There is but one small criticism that we have to make, that the pages were not headed by short titles of the papers.

<div align="right">E. L. I.</div>

ASTRONOMY AND METEOROLOGY

Worlds without End.   By H. SPENCER JONES, M.A., Sc.D., F.R.S., Astronomer Royal.   [Pp. xv + 262, with 32 plates.]   (London : The English Universities Press, Ltd., 1935.   5s. net.)

NOT every Astronomer-Royal has been endowed with the gift of popular exposition, and few have attempted to make use of it.   A hearty welcome is therefore assured to this attractively printed little book in which Dr. Spencer Jones has provided for the general public an interesting and authori-

tative account of the wonders of the Universe. Commencing with the Earth and its neighbours of the Solar System, the reader's horizon is gradually widened until it reaches the confines of telescopic vision where millions of stellar universes seem with one accord to be receding from us with almost incredible velocities. A short chapter on "Life in Other Worlds" makes an interesting digression from the general theme, and finally in "What was —What is to be" the author indulges himself and his readers in speculations concerning the remote past and remoter future of the Cosmos. Here possibly the layman may have a little difficulty in understanding why, when "all distances become doubled in about 1300 million years," the future history of the earth in relation to the sun is apparently unaffected, but happily his enjoyment does not necessitate that complete comprehension to which only few can attain. The illustrations are numerous and well chosen. A few slips will need correction in future editions; e.g. Whitakers Almanack is hardly so comprehensive as is suggested on p. 69, and the date 170 B.C. seems over a couple of centuries too early for the Fall of Jerusalem.

The Astronomer-Royal has accomplished a difficult task with great success, and his book, essentially up to date, will be read with pleasure and profit by anyone interested in astronomy, however shallow or profound his previous knowledge of that science may be. The price is very reasonable.

R. W. W.

The Solar System and its Origin. By HENRY NORRIS RUSSELL. [Pp. x + 144, with 13 plates and 1 figure.] (New York and London : Macmillan & Co., Ltd., 1935. 8s. 6d. net.)

PROF. RUSSELL's series of lectures delivered in 1934 at the University of Virginia on the Page-Barbour Foundation are here reproduced in book form, and give a clear and masterly summary of present-day knowledge about the various members of the Solar Family, together with the latest theories regarding their nature and origin.

Of the three chapters, the first describes the dynamical properties and probable age of the System, with the sizes, masses, periods and angular momenta of the different members. The knotty problem of the comets receives special attention. Chapter II treats of physical and chemical properties, temperatures, planetary atmospheres, and the composition of interiors. Chapter III, dealing with " Theories of its Origin," is unavoidably less satisfying in that its conclusions are destructive rather than constructive. Laplace's nebular hypothesis, which incidentally was first suggested by a theologian, Swedenborg, and a philosopher, Kant ; the planetesimal theory of Chamberlin and Moulton ; the tidal theory of Jeans and Jeffreys ; the collision theory of Bickerton ; the nuclear theory of Nolke ; all are weighed in the balance and found wanting. There remains the suggestion that the solar system was born some 2,000,000,000 years ago, at that fundamental date in the history of the material Universe when, according to the Theory of Relativity, all matter was closely packed together. Collisions between large masses were then presumably frequent and vigorous, and the births of planetary systems correspondingly numerous. In view of our present very limited knowledge, the only possible verdict on this suggested explanation is " Not Proven."

R. W. W.

An Introduction to Astronomy. By ROBERT H. BAKER, Ph.D. [Pp. viii + 312, with 280 figures.] (London : Macmillan & Co., Ltd., 1935. 12s. 6d. net.)

THE teaching of astronomy unfortunately seldom has a place in the ordinary curriculum of our secondary schools, but it is to be hoped that the importance of at least a nodding acquaintance with the oldest of the sciences will in time be recognized by the Educational Authorities. When such an awakening takes place, this latest book by Dr. Baker will be found a very suitable text-book for the higher forms. The author's *Astronomy* is well known as a standard and reliable compendium, and this *Introduction*, planned on similar but more elementary lines, is intended for students with no previous knowledge of the subject who wish, without the aid of mathematics, to acquire a knowledge of the sky and the various constellations, the individual members of the solar system, and the latest conclusions of astronomers regarding the stars and the universe. Dr. Baker is an experienced teacher and knows how to accentuate the essentials of his subject while securing the interest of the student by his popular style and apt illustration. Star maps are provided to show the face of the sky at 9 p.m. at different seasons of the year, and interesting descriptions of the more important constellations are added. All mathematical equations are rigorously excluded. The questions at the end of each chapter have been selected to aid and not to discourage the student, and the brief lists of references suggest further reading for those whose interest has been gripped. An adequate index completes the book, but, somewhat strangely, there is no list of the numerous and well chosen illustrations which add so greatly to its value.

For a teacher planning an introductory course in astronomy this book can be thoroughly recommended.

R. W. W.

Why the Weather ? By CHARLES FRANKLIN BROOKS, Ph.D. Second edition. [Pp. xviii + 295, with 52 figures, including 32 plates.] (London : Chapman & Hall, Ltd., 1935. 10s. 6d. net.)

THIS is a second, revised, edition of a book which at its first appearance in 1924 had a considerable success. Its author is Professor of Meteorology at Harvard University, and Director of the Blue Hill Observatory, and was at one time on the staff of the U.S. Weather Bureau. His wide experience of meteorology fits him to write a book on weather in a popular vein. The present book grew out of a series of daily explanations of the weather to classes in meteorology at Clark University. In its revised form it devotes a section to each season of the year, with descriptions of the common phenomena of each season. It does not follow the usual order of the text-book of meteorology, as is seen from the fact that the constitution of the atmosphere is not discussed until p. 195. There is no cause for regret at the inversion of the usual order. The author succeeds in giving clear and often lively pictures of the phenomena of weather, naturally with special reference to the United States. One finds very clear descriptions of such subjects as the formation of rain, of tornadoes, of ice-storms and thunderstorms, and of the special features of weather which affect aviation, agriculture and every-day life. The author finds time to discuss the difficulty which is often met, particularly in his own country, in finding the " cool dry place " in which

we are so often instructed to store things, and to give some useful advice as to where to park our car during a thunderstorm.

There are few aspects of weather which Prof. Brooks overlooks, and the book as a whole gives an impression of having been well planned and carefully written. The explanations are naturally simple, and on the whole fairly complete. When they are not complete, and the author does not tell us *why* certain things occur, he usually contrives at least to say *how* they occur. The nomenclature is American, and the English reader will perhaps feel some surprise at the use of the word " sleet " to denote hard pellets of ice, instead of the mixture of rain and snow to which we apply this term ; but even when the usage of the book is contrary to the normal usage in England, the reader will find no difficulty in understanding the author's meaning.

There are about 50 photographs of clouds, lightning, tornadoes, frost patterns, snow crystals, and other interesting phenomena, all excellently reproduced. The full index of contents adds enormously to the value of the book, by making it possible to refer back to any desired point, so that it is possible to use the book as a dictionary of weather. It is a valuable addition to the library of meteorological literature.

D. B.

PHYSICS

A Text-Book of Physics. Vol. V : Physics of the Atom. By E. GRIMSEHL. Edited by R. TOMASCHEK, D.Phil. Translated by L. A. WOODWARD, B.A., Ph.D. [Pp. xiii + 474, with 310 figures.] (London and Glasgow : Blackie & Son, Ltd., 1935. 17s. 6d. net.)

THIS is the last and undoubtedly the best part of the Grimsehl text-book. It is a translation of the seventh German edition prepared by Dr. Tomaschek in 1934 modified for English readers and brought further up to date in the process. Even so, however, there is no reference to induced radioactivity and no discussion of the proton-neutron structure of the atomic nucleus. The title does only bare justice to the contents for while it is true that three-quarters of the text is devoted to the multifarious aspects of atomic physics the remainder goes well outside this field, the last four chapters dealing respectively with Molecular Structure, the Structure of Solids and Liquids, the Electrodynamics of moving Media, and Matter and Energy in the Universe.

The treatment throughout is very clear and it is perhaps invidious to select any particular section for special praise. However, the twenty-page summary of the fundamental phenomena of radioactivity and the very simple account of Schrodinger's wave equation may be mentioned as examples of the author's excellent style. Dr. Woodward has translated the text into good idiomatic English ; indeed the only indication of its German origin lies in the frequent mention of Lenard. His name is followed by 33 page references in the index, Rutherford's by 16 and Millikan's by 3. In Richtmyer's *Introduction to Modern Physics.* Rutherford has 13, Millikan 7 and Lenard 5 !

The publisher's have done their share of the work admirably and considering the elaborate nature of the setting the price is most modest.

D. O. W.

A Treatise on Heat. By M. N. Saha, D.Sc., F.R.S., and B. N. Srivastava, M.Sc. Second edition. [Pp. xii + 815, with 257 figures, 2 plates and 76 tables.] (Allahabad : The Indian Press, Ltd., 1935. 30s. net.)

This book, which was first published in 1931 as *A Text Book of Heat*, now appears in a revised and extended form with the title *A Treatise*. The new name is certainly appropriate, for there are few parts of the subject that are not treated fairly extensively in this large volume. Besides describing much experimental work, the book treats of the Kinetic Theory, Radiation and Thermo-dynamics. Heat engines and steam turbines are discussed ; there are chapters on the application of Thermo-dynamics to Physical Chemistry ; and the book ends with an introduction to the Bose-Einstein and Fermi-Dirac Statistics.

A university student who is specializing in physics would do well to have some knowledge of most of the subjects treated ; but few, even of those who specialize in Heat, are likely to master all the topics. Anyone commencing research work in Heat will probably find useful guidance, with many references to original papers.

There is probably no other single volume that gives so extensive and so modern an account of the subject. Despite its length, this book is a joy to read ; it is clearly written, in a pleasant style, and the printing is good. We must be resigned to the volume being rather large and heavy.

The scope of the Treatise may be illustrated by a few details. An account is given of Knudsen's work on gas flow at low pressures. Callendar's work on steam near the critical point is described, and the low-temperature investigations of Onnes, Debye, Simon and De Haas. We find an account of the fresh verification of Planck's radiation law by Nernst and Wulf, and a brief excursion into the subject of infra-red band-spectra.

Joule's work on the mechanical equivalent of heat is treated very briefly, without a single diagram, whereas we are given diagrams of three forms of ice calorimeter. Larmor's deduction of the radiation pressure is given, though it assumes that velocities are directly additive and is therefore unsatisfactory when applied to waves of light. The book might be improved by extending the treatment of convection and giving some account of the thermal properties of heavy hydrogen and heavy water.

W. N. Bond.

Introduction to Quantum Mechanics. By Linus Pauling, Ph.D., Sc.D., and E. Bright Wilson, Jr., Ph.D. [Pp. xiv + 468, with 65 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1935. 30s. net.)

In this introduction to quantum mechanics the authors have set themselves the task of writing a text-book for the use of the chemist, the experimental physicist and the student who is beginning his study of theoretical physics. They state that they assume, on the part of the reader, no very advanced mathematics.

They have approached the subject by means of the analogy with classical physics which grew up round Schrödinger's original paper. This is the method often adopted in introductory works and is doubtless the easiest route for the readers the authors have in mind. It has also the merit of following the historical development of the subject.

The first two chapters are on classical mechanics and the old quantum theory. They are clear, but perhaps too concise—a criticism of brevity which applies especially to the treatment of the old quantum theory, which is important on account of its influence on the notation. In this part of the book the authors make an interesting remark. They point out when discussing the diffraction of particles by a crystal lattice that the de Broglie wave-length occurs in the appropriate formula, which might have led to an earlier discovery of the wave character of a particle.

The third chapter brings us to Schrödinger's equation and the new aspect of the subject is discussed. But we should have liked to have read something less formal about the operator methods. It would be more in keeping with the quantum description of nature, if operators were introduced, not as formal replacements of familiar quantities, but as actual representations of observables. Most beginners can appreciate this point and at once are brought into touch with quantum mechanical description. We do not " arbitrarily replace p_a by $\dfrac{h}{2\pi i}\dfrac{d}{da}$ . . .", we have developed a logical procedure which every beginner can and must understand.

The details of the work are admirable. There is an especially detailed account of the hydrogen atom, as important for any physicist now as the Bohr-Sommerfeld theory was but a short time ago. Many readers will discover at this point that the authors have been optimistic in the mathematical knowledge required. But this is of no consequence, since all must discover that there is no royal road to be found anywhere in physics.

Later on we come to the study of the helium atom, to some applications to molecular spectroscopy and to quantum statistics.

On reaching the end of the work we feel that it is a good book, but not altogether satisfactory as an introduction. It contains the most important facts for this stage and they are put in an attractive way. It is a work for a student who, after beginning, wishes to go on with the subject and requires guidance in the next stage amongst the maze of original work.

After warning us not to expect a discussion of " advanced topics ", including matrix mechanics, the authors insert a " brief mention " of it in the last chapter. It is difficult to appreciate what use this brief mention can be, and it is to be regretted that our authors have joined those who introduce this notation only to label it as difficult and so to misrepresent it. It is time this habit was broken and that a very elegant and simple mode of description of natural phenomena took its proper place in introductions to quantum mechanics.

H. T. F.

Infra-Red and Raman Spectra. By G. B. B. M. SUTHERLAND, M.A., Ph.D. Methuen's Monographs on Physical Subjects. [Pp. xii + 112, with 27 figures.] (London : Methuen & Co., Ltd., 1935. 3s. net.)

THE building of molecules has long been a fascinating and important part of chemistry, and every means that the physicist can provide, is used to elucidate the design. Infra-red and Raman spectra form one of the most important means of investigation ; for quite apart from their help in determining this design, they measure up the elasticity of interatomic linkages. There can be little surprise, then, that four considerable volumes have appeared since 1930, each dealing in detail with different aspects of the spectra.

Despite this formidable output, Dr. Sutherland's monograph will be welcomed by many : it rises above the bewildering array of publications, and surveys the subject in a manner most helpful to the student with research in view, and acceptable to those wishing to know the subject as part of modern chemistry or physics. The subject is treated in systematic fashion : experimental methods, the normal vibrations of a molecule, vibration-rotation spectra, long wave-length spectra, and correlation of infra-red and Raman spectra are the titles of the five chapters. Throughout, the significance of these spectra for molecular structure is kept well before the reader. Thus, brevity is obtained by the method of survey rather than by neglect of any aspect of the subject ; and in this the monograph fills a real deficiency in the literature. The standard of production now associated with Methuen's series is well maintained, and a bibliography and appendix of molecular constants complete a book that can be thoroughly recommended.

A. B. D. CASSIE.

The Brunner Focal Depth–Time–Distance Chart. By G. J. BRUNNER, S. J., and J. B. MACELWANE, S.J. (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1935. 10s. net.)

THE majority of earthquakes originate within a few tens of kilometres below the surface of the earth and the fact that the focus may not be quite at the surface can usually be ignored in the determination of epicentral distance and time of origin. In recent years, however, it has been discovered that some of the more violent earthquakes originate at depths as great as 600 km., and for such shocks the normal tables of times of transmission of the waves cannot be applied directly. Moreover, the records of such shocks are complicated by the appearance of additional pulses due to reflexion of waves from points near the epicentre. The Brunner Chart is designed to assist observers in the interpretation of such records and to enable estimates of the focal depth, epicentral distance and time of origin to be obtained from the records at a single station. Curves are given showing the times of travel to various distances of the more prominent phases which are likely to occur in a deep earthquake ; the curves are drawn at intervals of 100 km. of depth between zero and 700 km. Since complete agreement has not yet been reached as to the best average times for normal earthquakes, the times given on the Brunner Chart should not be regarded as absolutely correct ; they are, however, quite accurate enough for the routine analysis of seismograms.

The chart is well reproduced and is mounted on linen ; it is accompanied by an explanatory booklet of 12 pages, and it should be a very useful accessory in the practical seismologist's equipment.

F. J. S.

Mechanical Properties of Matter. By S. G. STARLING, B.Sc., A.R.C.Sc., F.Inst.P. [Pp. vi + 336, with 222 figures.] (London : Macmillan & Co., Ltd., 1935. 6s.)

MR. STARLING's book forms an important contribution to the literature dealing with that branch of Physics somewhat loosely summed up as Properties of Matter or General Physics. The aim of the book is to provide a course in the subject suited to students preparing for Intermediate Science and Higher School Examinations. A close perusal shows that its scope is more

extensive than this, and that students taking Physics in the General Honours Examination for B.Sc., will find their requirements fully met within its pages.

The book is a judicious blend of theory and practical work, and the author has drawn upon his long experience as a teacher of Physics, when dealing with the many points which present considerable difficulties to students.

The Calculus has been used throughout with consequent gains in the neatness and brevity of the mathematical treatment. Important points are illustrated by worked examples, and there is a large collection of graded exercises to be worked by the student. The book consists of thirteen chapters the first four of which deal with the fundamentals of Mechanics. The remaining chapters deal with gravitation, elasticity, viscosity, kinetic theory, surface tension, diffusion and wave motion. The treatment throughout is particularly fresh and clear, and there is a surprising amount of information contained within a relatively small compass. From a writer of Mr. Starling's reputation one is led to expect a text-book of distinctive character and clarity of treatment. In the opinion of the reviewer his new book attains to the high standard set by its predecessors and will prove to be a most useful and helpful text-book. The book has been excellently produced in every feature, and author and publishers are jointly to be congratulated on such a valuable addition to the literature.

S. Marsh.

A Comprehensive Treatise on Practical Mechanics. By J. M. Lacey, M.Inst.C.E. [Pp. viii + 320, with 102 figures.] (London : The Technical Press, Ltd., 1935. 18s. net.)

It is difficult to know exactly where to place this book ; perhaps it is best described in the words of the author, who states that the work " is intended to occupy a place between an elementary treatise on the science of mechanics and the more advanced works on particular branches of that science. The aim of the author has been to carry the reader from first principles to their practical applications. . . . Although not an elementary treatise, the author has endeavoured to present the subject in as simple a manner as possible . . .". On the one hand we have a detailed exposition of the parallelogram of forces, the polygon of forces, and so forth, quite suited to the beginner ; on the other hand the treatment of velocity and acceleration takes us almost at once into the methods of the calculus, which the author uses freely whenever he wishes. Having selected his method of treatment, the author carries it out well, but, for engineering students at least, we should have liked to see the inclusion of graphical methods applied to velocity and acceleration. It is surely a serious omission from " a comprehensive treatise " that there is no treatment of relative velocity and relative acceleration. In his definitions of mass and density, the author leaves us in Newton's logical circle with its philosophical difficulties. The book would be greatly improved by the addition of more worked problems and problems for the student. The subjects treated are those usually found in books on statics and dynamics, including the elastic deformation of beams. The reviewer hopes very much that the author will one day give us a second edition of this book with the additions indicated above.

John Case.

Strength of Materials. By EDWARD R. MAURER and MORTON O. WITHEY, Professors of Mechanics at the University of Wisconsin. Second edition. [Pp. xii + 382, with 333 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. 17s. 6d. net.)

THE first edition of this book appeared in 1925; the new edition contains an additional section on welded joints, whilst those sections which deal with axial impact, tubes under external pressure, and the chapter on columns, have been rewritten, the latter being brought into line with the recommendations of the Committee on Steel Column Research of the A.S.C.E. The book should be generally useful to draughtsmen and students of engineering.

In the first chapter the general ideas concerning stress and strain, tensile stress under shear, shear stress under tension, and combined stresses, are presented with great clearness. We should have liked to see a more thorough treatment of the failure of materials under combined stresses and the various theories of elastic breakdown under these conditions. To English readers, the use of λ instead of σ or $1/m$ for Poisson's Ratio may be irritating. The chapter on the behaviour of metals, and other materials, under test, and methods of testing, is on the whole good and should prove of great use, especially as it is supplemented by a set of tables of the physical properties of metals, timber, etc. It is a pity that no mention is made of the Lower Yield Point, seeing the increasing significance attached to this in Europe. Chapter III is adequate, but, seeing that the authors thought fit to add a section on welded joints, it seems remiss not to have included the results of Prof. Coker's work on this subject; we also think that the subject of bending moment diagrams might have been given more space on account of the difficulty which students always seem to find when approaching the subject for the first time. A novel feature, in dealing with beams, is the separation of $p/y = M/I$ from $M/I = E/R$, which probably has many advantages for the student. The torsion chapter contains some really excellent diagrams, and it is very pleasing to find them duplicated in such a way as avoid the necessity of turning over a page to look at a diagram while the accompanying text is on a different page. Throughout the book the diagrams are excellent. In addition to the text, the book contains many worked problems and others for the student to solve.

<div align="right">JOHN CASE.</div>

Electrical Measurements in Principle and Practice. By H. COBDEN TURNER, M.I.E.E., M.I.Mech.E., and E. H. W. BANNER, M.Sc., A.M.I.E.E., F.Inst.P. [Pp. xiv + 354, with 219 figures and 1 folding plate.] (London: Chapman & Hall, Ltd., 1935. 15s. net.)

THE wide variety of electrical measurements with which engineers are nowadays frequently called upon to deal, makes of great value readily accessible information regarding the possibilities and limitations of the different kinds of apparatus on the market. The authors of this book have set themselves the task of providing that information in as simple a form as possible and the results they have achieved are ample justification for their enterprise. Purely theoretical considerations have been completely excluded on the ground that in the first approach to a problem and even for the final measurement, it is frequently unnecessary to concern oneself

with theoretical aspects so long as their practical significance is fully appreciated. This dictum will no doubt receive much support, but in it there undoubtedly lies a very real danger that the limitations of a particular measurement are not properly realised and erroneous results may accrue. The authors have done their best in the limited space available to warn readers of the pitfalls of which they may become victims, but this warning can only be really effective if the person concerned is at the same time on the look out for such sources of error. In the case of apparatus applicable to high frequency measurements too much importance cannot be attached to this point and the reviewer is inclined to think that the information given here tends to inadequacy. The book does not, in fact, cater properly for those interested in high frequency technique, a branch which has now become of such wide scope that the authors would probably have been better advised not to attempt to enter its field. Many useful drawings are given of apparatus described, but there are also a number of photographs some of which, such as Fig. 22, give practically no information at all and are quite superfluous.

The book is divided into five parts. The first is concerned with units, standards and considerations of accuracy to be expected therefrom. The second part deals with instruments as the tools of measurement and after a preliminary classification the discussion proceeds under the headings of electromagnetic, electrostatic and thermal, miscellaneous such as thermionic voltmeters, testing sets, galvanometers and oscillographs, potentiometers and finally some general considerations applicable to the foregoing. Part three is devoted to the measurement of electrical quantities such as current, voltage, power, quantity of electricity, energy, frequency, wave-form, etc. Part four gives a description of methods available for the measurement of electrical properties of various apparatus as for example resistance, impedance, inductance, capacitance and magnetic phenomena. Lastly in part five indirect electrical measurements are specially considered, including such matters as the determination of temperature, light intensity, speed and some miscellaneous quantities employed in particular industries. A glossary is appended of terms used for describing parts of instruments and in measurements.

The book is not written for students or research workers, but it should appeal to many practising engineers who feel the need for some help either in finding a suitable method for the determination of a given quantity or in selecting the best type of instrument available for the measurement concerned.

H. M. BARLOW.

Industrial Electronics. By F. H. GULLIKSEN and E. H. VEDDER. [Pp. xiv + 245, with frontispiece and 245 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1935. 17s. 6d. net.)

THIS book describes a number of industrial applications of such electronic devices as light sensitive cells, thermionic valves of the vacuum or gaseous discharge type and mercury arc rectifiers. All of these have an important place in scientific apparatus applied to modern industry and are particularly valuable in promoting efficient organisation. The authors have selected for special consideration typical applications in each case and they have shown

how amazingly effective these electronic devices can be when suitably employed. Although the book makes interesting reading it is perhaps not so enlightening as some might wish when dealing with fundamental principles. The explanations given are rather superficial and in many cases insufficient to enable the reader who approaches the apparatus for the first time to get a proper understanding of its operating mechanism.

After a review of the outstanding characteristics of the principal types of electronic tube and their essential circuit arrangements, a variety of instruments and control systems are described. Appliances for regulating the operation of sorting, grading, matching, metering, signalling and protective equipments are dealt with first. This is followed by a discussion of rectification and frequency conversion by mercury arc equipment and finally a section is devoted to automatic regulators for voltage, speed, temperature, etc. As far as the writer is aware, the book is unique in that it is specifically concerned with the practical applications of electronic devices. The authors have done a very useful service in bringing together so much information on the subject and presenting it in a form which is readily assimilated.

H. M. BARLOW.

CHEMISTRY

General Chemistry. By HORACE G. DEMING. Fourth edition. [Pp. xiii + 774, with 170 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1935. 17s. 6d. net.)

THIS is the fourth edition of Prof. Deming's *General Chemistry* which first appeared in 1923. The object of the author is to present a wide but elementary survey of modern chemistry for the general student and not a descriptive inorganic text-book for students specialising in chemistry, and he expresses the hope that the book may be appreciated not only for the matter which it includes, but also for that which has been intentionally omitted.

The book is divided into four sections, (1) General Principles, (2) Non-Metals, (3) Organic Chemistry and (4) The Metals, occupying 223, 312, 68 and 146 pages respectively. It is obvious that the space allotted to Organic Chemistry is hardly sufficient to justify the title of the book, especially as 18 pages of this section are devoted to " How Chemical Formulas are Derived," a chapter which might well have been placed in Section 1. The rest of the section describes the Hydrocarbons, the Cellulose Industries, Other Industries based on Animal Products and Some Synthetic Products.

The general treatment is very modern. The student is early introduced to the electron in oxidation-reduction reactions, ionic instead of molecular equations are used wherever possible, and acidity is indicated in equations by the oxonium ion unless the properties of a specific acid are being discussed. By a system of cross-references, the general principles of Part 1 are linked with the descriptive matter in Part 2, and a special effort has been made to present the material in as flexible a way as possible. Each chapter ends with a number of revision exercises and a useful list of references to current literature. The book is well produced, very free from errors and will interest those who wish to experiment with newer methods of teaching, but one feels that the knowledge of students thus instructed will rest on a rather sandy foundation.

J. N. S.

The Structure of Metallic Coatings, Films, and Surfaces. A General Discussion held by The Faraday Society. Reprinted from *Trans. Faraday Soc.* [Pp. 1043–1290, with 77 plates and numerous figures.] (London and Edinburgh : Gurney & Jackson, for the Society. 21*s.* net.)

THIS discussion falls sharply into two parts. In the first part, the methods and results of electron diffraction by metallic surfaces are described in great detail. It is shown how the alterations in surface structure produced by polishing, or " running-in " of an internal-combustion engine, proceed with loss of the crystalline structure and production of the polished, amorphous " Beilby " layer ; how the presence of gases possibly as oxides, etc., in the surface layers produce rings indicative of structure not shown up by X-rays ; and some further evidence is given on the orientation of fatty substances with long chains on the surface of metals. The possibilities of this most important new tool for investigating the surface layers of any solid which can be placed in a high vacuum are well described.

In the second part, microscopic and optical methods of examination of films are described, and extremely interesting details of growth and crystallisation of films of metal deposited on glass are given ; there is much motion of atoms along the glass surface in these films. Finally, many important papers are included dealing with the structure of metallic coatings of industrial importance, particularly electrolytically deposited coatings.

The volume is a valuable collection of papers, of great interest both scientifically and industrially ; and there is a remarkably fine collection of photographs at the end.

N. K. A.

Physical Principles and Applications of Magnetochemistry. By S. S. BHATNAGAR, M.Sc. (Panjab), D.Sc. (Lond.), and K. N. MATHUR, M.Sc., D.Sc. (Allahabad). [Pp. xiv + 375, with 58 figures and 1 folding plate.] (London : Macmillan & Co., Ltd., 1935. 21*s.* net.)

IN recent years magnetic measurements have frequently been used to aid in the solution of chemical problems and there is room for a monograph which will bring together the theories and experimental results which are scattered through the literature. The volume under review is therefore opportune. It contains a clearly written account of modern mathematical theories of magnetism, descriptions of a number of experimental methods of measuring dia- and paramagnetic susceptibilities, and a detailed discussion of a large amount of experimental data with special reference to their chemical applications. The chapters on the " panmagnetism " of matter and on magnetism and valency are those to which the chemist will most frequently turn.

The value of magnetism in attacking chemical problems is largely due to the simplification introduced by modern quantum theory in the discussion of paramagnetism. As van Vleck has shown, the magnetic moment of a polyatomic molecule is determined to a first approximation by the net spin of the electrons and the contributions of the orbital motions can be neglected. Measurements of the magnetic moment can therefore be used to detect free radicals, to observe changes in valency, and are of particular value in the study of co-ordination compounds since Pauling's theory gives special significance to magnetism as an indicator of the type of valency linkage. These

modern developments are clearly described and many experimental results are quoted. In the reviewer's opinion it is a pity that the authors express results so frequently in Weiss magnetons. This unit has no physical significance and should disappear from modern literature.

The applications of diamagnetism to chemical problems are not so striking; since the pioneer work of Pascal in this field little work has been done which covers a sufficient range of organic compounds. The authors discuss a considerable amount of diamagnetic data in detail, but the reviewer feels that the discussion is not sufficiently critical both with respect to the accuracy of the experimental results and the validity of the numerous subsidiary hypotheses which are used to interpret them.

S. S.

Principles and Applications of Electrochemistry. Vol. I : Principles. By H. J. CREIGHTON. Third edition. [Pp. xviii + 502, with 84 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1935. 20s. net.)

HAVING passed through two editions before the appearance of Volume II— which was to have been written by Dr. Fink—Volume I has now reached a further edition simultaneously with the first publication of Volume II— which has actually been written by Prof. W. A. Koehler. It would seem, however, that the establishment of this new partnership has not called for any substantial modification of Volume I. Indeed, comparison with the second edition shows that hardly any important changes have been made in the text, apart from the addition of paragraphs dealing with a few special subjects in which notable advances have been made during the last few years, e.g. the glass electrode ; the electrolytic separation of light and heavy hydrogen ; and modern theories of overvoltage, of the influence of interionic forces on electrolytic conductivity, and of the true degree of dissociation of strong electrolytes. The additional matter is satisfactorily incorporated in the appropriate places in the text, and, although it increases the size of the book by only 14 pages, its inclusion should serve to enhance the usefulness of this now well-established work.

H. J. T. E.

Principles and Applications of Electrochemistry. Vol. II : Applications. By W. A. KOEHLER. [Pp. xiv + 545, with 245 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1935. 25s. net.)

IN this volume the author has endeavoured to deal with all industrial applications of electrochemistry. After a brief survey of some of the more relevant aspects of theoretical electrochemistry, primary and secondary cells and the various processes involving electrolysis are described. Separate chapters are devoted to electroplating, electrorefining, the extraction of metals by electrolysis of aqueous solutions and fused electrolytes, the electrolysis of alkali halide solutions, electrolytic oxidation and reduction processes, and the electrolytic production of oxygen and hydrogen. Problems of corrosion and methods of electroanalysis are also considered. On the electrothermal side the construction and operation of the various types of electric furnace used in metallurgical and non-metallurgical industries are briefly described, and processes for the fixation of nitrogen and the production of

ozone are discussed. The chapters entitled " Electronics "—including reference to thermionic valves, photoelectric and photovoltaic cells, and gaseous conduction lamps—and " Separation by Electrical Means "—comprising sections on magnetic separation, electrostatic separation, and applications of electrophoresis and electro-endosmosis—deal with several matters not usually considered in books on electrochemistry, but there is, nevertheless, a good deal to be said in favour of their inclusion. A final chapter deals with such miscellaneous processes as the electrolytic purification and sterilisation of water, electrolytic rectifiers, the anodic oxidation of aluminium, and the electrolytic concentration of heavy water.

Limitations of space have allowed only a general survey to be made of this very wide field, and the treatment throughout is somewhat sketchy. The essential features of the processes considered are, in general, clearly described, and the numerous diagrams and some unusually informative photographs serve to show many details of construction of the various types of plant actually used in industry. In most cases, however, little attempt is made to link theory with practice by eliciting the factors which determine the efficiency of a process and indicating how they may be controlled. There is, indeed, very little correlation with the general principles of electrochemistry as set forth in Volume I, and one cannot help feeling that a closer collaboration between the two authors would have greatly increased the usefulness of the work as a whole. Nevertheless, as a descriptive survey of the whole field of applied electrochemistry, with special reference to American practice, the present volume promises to be of considerable value. If it does not show how fundamental principles may be applied in the development of new processes, it does give a brief but up-to-date account of practically every application of electrochemistry in modern industry, and in many cases the information has been " vetted " by an expert in personal touch with the particular branch of industry concerned. The printing and general get-up of the book are very satisfactory.

H. J. T. E.

Modern Views of Atomic Structure. By DR. KARL RAST. Translated by DR. W. O. KERMACK. [Pp. ix + 155, with 25 figures.] (London : Frederick Muller, Ltd., 1935. 7s. 6d. net.)

THIS is a translation of two lectures delivered in 1932 and 1933 on the essential nature of the Periodic System of the elements. To these have been added brief accounts of neutrons, nuclear structure, artificial radioactivity, and other notable advances in nuclear chemistry and physics during the last few years. The lecture form has been retained and thanks to the excellent translation this little book gives a vivid and detailed exposition of the arrangement of the extranuclear electrons. The treatment involves little mathematics, it is enlivened by a number of homely and ingenious analogies and the copious use of original diagrams helps to make the book very clear and interesting. It forms an excellent introduction to modern treatises on atomic and molecular structure.

S. S.

The Quantum Theory of Valency. By W. G. Penney, A.R.C.S., M.A., Ph.D. Methuen's Monographs on Chemical Subjects. [Pp. vii + 95, with 14 figures.] (London : Methuen & Co., Ltd., 1935, 2s. 6d. net.)

To attempt to explain the Quantum Theory of Valency in 95 octavo pages requires courage : to succeed requires great skill. Dr. Penney has shown both. As is inevitable in the circumstances, the reader is expected to take much for granted. For example, he is told that a wave function is a description of a state of matter, possessing a certain physical significance and certain mathematical properties : he is expected to accept the statement and not to ask questions. Dr. Penney has, however, been careful to define the necessary concepts in such a way as not to arouse too many such questions in the readers' minds, and so this method is quite successful.

The book covers all the important topics : generalities, such as the meaning of quantum numbers, the Pauli principle, and wave functions, and the explanation of the Periodic System ; and their application to the problems of the hydrogen molecule, more complex molecules, the theory of directed valency, and the calculation of heats of activation and of resonance.

There is a misstatement in Chapter II, where, in discussing steric hindrance in ethane, Dr. Penney states that the potential energy is a minimum in the 0°, 120°, and 240° orientations : Eyring's original paper (correct reference, *Jour. Amer. Chem. Soc.*, **54**, 3191, 1932) confirms the obvious comment that these should be the 60°, 180°, and 300° orientations.

There are few general criticisms which may reasonably be made, and these are not very serious. It might have been better, in the introduction, to describe the hydrogen atom states first, and then to say how these are modified in poly-electronic atoms, rather than to discuss the general case immediately. Then, too, Dr. Penney sometimes uses hard words without explicit definition ; most chemists do not know the technical meaning of " degenerate " or " anti-symmetric." A glossary might be provided in a second edition. Finally, although an excellent set of references to original papers is given, no text-book is mentioned as suitable for those who want to know more about the foundations and details, but have only a humble and rusty mathematical equipment.

Within its inherent limitations this is a good book, and it should be read by all chemists.

L. E. S.

The Chemistry of Rubber. By H. Freundlich. Methuen's Monographs on Chemical Subjects. [Pp. xi + 72, with 13 figures, including 3 plates.] (London : Methuen & Co., Ltd., 1935. 2s. 6d. net.)

The title is completely misleading as only scanty references to chemistry, as such, occur in the book. In fact the author infers that where colloidal properties are not predominant the problems need not, therefore, be discussed extensively (p. 52). Actually the book deals in an inadequate manner, as is inevitable in a book of only 72 small pages, with the colloidal properties of latex and raw rubber, and then proceeds to deal very briefly with such topics as vulcanisation, oxidation of rubber, botany and the history of the industry. Why such varied topics are introduced it is difficult to understand.

No doubt due to the width of the subject which is attempted and the very limited space available, the treatment is so compressed that the book

must certainly prove unintelligible to the uninitiated and merely a source of annoyance to the expert.

The English is clumsy, and it would appear that the general editor (of the series of monographs of which the volume is one) might have put the author's phraseology into a more reasonable form.

It is impossible to understand upon what plan the bibliography has been compiled, as many references seem ill-chosen.

It is to be regretted that the book cannot be commended in any way.

T. J. D.

The Theory of Emulsions and their Technical Treatment. By WILLIAM CLAYTON, D.Sc., F.I.C. Third edition. [Pp. ix + 458, with 91 figures.] (London : J. & A. Churchill, Ltd., 1935. 25s. net.)

THE second edition of this indispensable book appeared seven years ago and ran to 283 pages. The increase of size is fully justified by the growing importance of the subject which, incidentally, hardly appears to be realised even by institutions which include colloid chemistry in their curriculum. Lack of academic interest may indeed be one of the reasons why practice in this field is very considerably in advance of theory ; in fact, many of the theories of emulsions, of which the author gives a very complete survey, were inadequate at the time they were formulated and have become more so in the light of new facts. Nor has criticism spared even the more recent " orientated wedge " theory which appeared to offer an attractive explanation why, according to the nature of the emulsifying agent one or the other type of emulsion was formed. The theory is supported by drawings—reproduced here, as they have been many times—in which the length of the oleate molecules is about the same as the radius of the globules they surround. As the length of the oleic acid molecule is 11×10^{-8} cm. and even a small globule say 2μ dia. has a radius of 1×10^{-4} cm., these diagrams are decidedly misleading.

Quite apart from the question of emulsion type two factors complicate all theoretical treatment : the difficulty of defining stability in quantitative terms, and the impossibility of describing a given emulsion fully by anything short of a complete size distribution analysis. These points are fully faced by the author, and all the methods available for size-frequency determinations are described in ample detail in the chapter on Physical Measurements in Emulsions.

The chapters on : Emulsions in Biological Investigations (devoted largely to milk) ; Miscellaneous Emulsions ; the Preparation of Emulsions, and Deemulsification are mines of information drawn from the most varied sources. The material has been selected with judgment and is presented lucidly. The more academic reader will perhaps be struck most by the numerous types of " mills " and homogenisers which have been developed empirically to meet the demands of industry. A good deal of theoretical investigation will be necessary to elucidate fully the mechanism of dispersion in these appliances. Their development has been due to the increasing use of emulsions in all those cases in which an " oily " liquid cannot be applied to its purpose either in concentrated form or diluted by a completely miscible liquid—a simple instance is the use of paraffin emulsions as insecticidal plant sprays.

Dr. Clayton presents this vast amount of matter with great clearness

and with a detachment which, perhaps, may cause the reader to wish occasionally that the author had given him a little more help in choosing between conflicting theories or statements. Proof reading has been done with great care; the statement on p. 3 that colloidal suspensions consist of particles of 10^{-3} cm. is so obvious an error as to be harmless.

The book is excellently printed and illustrated, and the price is decidedly moderate.

One point, which can hardly fail to strike the attentive reader, may deserve mention; about 90 per cent. of the work, and practically all that is fundamental, has been done by British and American authors. This ratio is so unusual in the field of colloid chemistry that it may provide an interesting problem to those who indulge in ethnological speculations.

E. H.

The Systematic Identification of Organic Compounds. By RALPH L. SHRINER and REYNOLD C. FUSON. [Pp. ix + 195, with 9 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. 11s. net.)

THE preface to this work immediately creates a favourable impression, as it points out that the exercise given to students in the identification of organic compounds, owing to lack of a rigid system, is an invaluable training in care of observation and use of judgment and is the best kind of experience for those preparing for research. The reviewer would go as far as to say that, provided derivatives are prepared conscientiously, more of this type of training could be given in many institutions in place of much of the usual preparative work which involves little more than carrying out of precise instructions from a " cookery " book. If a student has a practical knowledge of the common operations of organic chemistry, distillation, crystallisation, etc., he would gain more advantage from attempting to make a few compounds direct from the often meagre instructions in the original literature than from carrying out a large number of the standard preparations.

The system adopted by the authors classifies organic compounds according to their solubilities in water, ether, 5 per cent. sodium hydroxide, 5 per cent. hydrochloric acid, 5 per cent. sodium bicarbonate, cold concentrated sulphuric acid and 85 per cent. phosphoric acid. Sub-classification is achieved by twenty-one classifying reagents, e.g. bromine in carbon tetrachloride, alkaline permanganate, nitrous acid, phenylhydrazine, ferric chloride, etc., and final identification is made by use of physical constants and the preparation of derivatives. The determination of physical properties and the choice and preparation of derivatives are carefully dealt with though the procedures given for the last are sometimes open to criticism; the methods suggested for oximes and 2 : 4-dinitrophenylhydrazones are not, in the opinion of the reviewer, the most satisfactory. There is a short section dealing with the separation of mixtures and the usual tables of melting-points of compounds and their important derivatives which have the advantage of being unusually clear.

The method of procedure may not be particularly new, but the great virtue of the book is its clearness, its scientific outlook, its freedom from muddled and inaccurate statements and the care with which the pitfalls of the subject are indicated. The writer has seldom been able to recommend so warmly a students' text-book.

O. L. B.

Fluorescence Analysis in Ultra-Violet Light. By J. A. RADLEY, M.Sc., A.I.C., and JULIUS GRANT, Ph.D., M.Sc., F.I.C. Second edition. [Pp. x + 326, with 28 plates and 17 figures.] (London : Chapman & Hall, Ltd., 1935. 21s. net.)

THE first edition of this work was reviewed in these columns some two years ago and the main differences between the edition under review and that reviewed earlier, are that a certain amount of new material has been added, and that the number of references has been increased to over 1,500. In view of the fact that a new edition (and not merely a reprint) had been decided upon it is a pity that a more careful scrutiny of the original edition was not made. Certain errors and omissions have been attended to, but others still remain, whilst the authors still discuss certain subjects in a very loose fashion, e.g. the transmission of glasses (p. 25) and the transmission of silver-coated quartz lenses (p. 30).

The volume is very well produced and, as a compendium, would appear to be serving a useful purpose.

A. H.

Analytical Chemistry. Vol. II : Quantitative Analysis. By F. P. TREADWELL and WILLIAM T. HALL. Eighth edition. [Pp. xiii + 858, with 153 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1935. 30s. net.)

" TREADWELL " is one of the few text-books with an international reputation. Compiled originally by the late F. P. Treadwell, Professor of Analytical Chemistry at Zurich and translated into English by W. T. Hall of Massachusetts Institute of Technology, it rapidly established itself on both sides of the Atlantic as the leading text-book of analytical chemistry. In 1924 the sixth and completely revised English edition appeared, which differed considerably from the German text, changes having been made to conform with the best American practice, and once again, in the eighth edition the entire book has been re-set. Many changes have been made ; some of the older methods have been dropped and the " Outline of a Course of Instruction " which was introduced into the previous edition has been omitted. New and well-tested methods for the determination of columbium, tantalum and several other elements have been introduced. To recommend " Treadwell " is superfluous. It is sufficient to say that the new edition fully maintains the tradition of accuracy and comprehensiveness so long associated with this well-known work.

J. N. S.

A Laboratory Course in Elementary Chemistry. By E. B. R. PRIDEAUX, M.A., B.Sc.(N.Z.), D.Sc.(Lond.), F.I.C., and F. C. LAXTON, B.Sc.(Lond.), A.I.C. [Pp. xiv + 258, with 37 figures.] (London : William Heinemann, Ltd., 1935. 3s.)

THIS is a beginner's course of practical chemistry. The elements of chemical manipulation are described, the laws of chemistry demonstrated, the properties of simple substances examined and the elements of volumetric analysis explained. The material is well chosen and graded, but it is to be deplored that so many rules of grammar should be broken, that the laws of chemistry may be established. The definite article is often omitted, prepositions mis-

used and a loose phraseology sometimes distorts and occasionally obscures the information which the authors wish to convey.

A few examples may be quoted : " Burn in jar sulphur or phosphorus " (p. 73) ; " after boiling water, the tube is connected " (p. 135) ; " take 50 (or two 25 c.c.) by a pipette " (p. 156) ; " discovered from 1890 " (p. 192) ; " a weighed basin, which is evaporated " (p. 21) ; " dissolve in half a test-tube full of water " (p. 74) ; " bromates may be made by bromine water as chlorates by action of chlorine water on alkali " (p. 66) ; " crumple a roll of tin foil and drop it in, then fill it with hydrogen sulphide " (p. 85) ; " make 250 c.c. of silver nitrate by weighing 1·20 g. and dissolving 500 c.c. in distilled water " (p. 166). The following misprints need correcting :—" thin ammonia " (p. 68) ; " by means of a glass tube reading to the bottom " (p. 117) ; the molecular weight of silver nitrate (p. 164). The calculation on p. 161 is so obviously incorrect that it is difficult to imagine how it escaped the eye of the proof-reader. These and other similar points all need attention before the book can be recommended.

J. N. S.

Exercises in General Chemistry and Qualitative Analysis. By HORACE G. DEMING and SAUL B. ARENSON. Fourth edition. [Pp. xv + 326, with 25 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1935. 9s. net.)

THIS is a course of practical chemistry arranged for students using Deming's *General Chemistry* as a text-book, cross-references to the fourth edition of this work being freely given. The course, the object of which is not so much to impart chemical information as to develop a chemical point of view, consists of seventy-five experiments, followed by a short course of qualitative analysis. It is obvious that much time and labour have been spent on arranging the work. Minute details are given for performing the experiments and for making the elaborate records which the student is encouraged to keep, and each exercise is supplemented with numerous " quiz " questions which must be answered (sometimes in writing) as they arise. The experiments chosen cover a wide ground and a student who worked through them diligently would accumulate a considerable store of chemical information, but this very mechanical type of instruction has not yet found much favour in this country.

J. N. S.

GEOLOGY

Tectonic Essays : Mainly Alpine. By E. B. BAILEY, M.A., F.R.S.L. & E., Professor of Geology, Glasgow University. [Pp. xii + 200, with 5 plates and 49 figures.] (Oxford : at the Clarendon Press ; London : Humphrey Milford, 1935. 12s. 6d. net.)

To understand fully the importance of Prof. Bailey's book, one must bear in mind that the results of the exploration of the Alps play a rôle of paramount importance in *modern Geology* and hold a prominent position among the advances of modern Science.

Prof. Bailey who is a pupil of Ben. N. Peach, the great Scottish tectonician, follows the investigations, during the last thirty years, of the structure of the Alps, in writings and on the field.

His book is the story of past achievements, the story of the development of ideas and of their verification in the field. All British students in structural Geology know the " Nappe theory " and the synthesis of the Alps, and Prof. Bailey, who is at present the undisputed Master of tectonics in Great Britain, knows how much geologists may profit by studying the history of these great discoveries. Indeed, for geologists engaged in oil or mining prospecting, and for students of Geology and Geography, it is valuable to study how results have been arrived at. Thus they will learn the mistakes that must be avoided ; the danger in simply copying the Alps ; the value of imagination based on knowledge ; that a fold, though interesting in itself, must not obscure the view of the whole structure to which it belongs as a detail.

With the Glarus discovery, in which the name of Marcel Bertrand is famous, we pass gradually to the interpretation of the Prealps, produced by Schardt, who thus heralded a revolution in Alpine Tectonics.

In a capital chapter on the High Limestone Alps of the Rhone sector, the author admires, rightly, Lugeon's leading work and the wonderful synthesis, of 1902, to which he was led. Then we wander from Argand's field, the Pennine Alps, to Termier's triumph in the Hohe Tauern, and coming back to France we hear again of Marcel Bertrand, in Provence.

In the last chapter we learn that the Alps must be fully understood, for the case of the Pyrenees has shown how dangerous may be a mere copying based on superficials.

Prof. Bailey in writing this book also pays a tribute of admiration to the work of the masters to whom we owe the foundation and development of Alpine Tectonics. I am sure to be the interpreter of all Alpine geologists in thanking him for it.

<div style="text-align: right">Léon W. Collet.</div>

Historical Geology of the Antillean-Caribbean Region. By C. Schuchert, D.Sc., LL.D. [Pp. xxvi + 811, with frontispiece, 16 plates and 107 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1935. 50s. net.)

This magnificent monograph is the first of a projected series of three on the Historical Geology of North America. It represents the culmination of thirty years of stratigraphical and palæogeographical research and reading by Professor Schuchert, who is regarded by all as the foremost American authority on these branches of geological science. The work has been built around the well-known series of palæogeographical maps of North America compiled by the author. At first sight it seems strange that the initial volume of the projected series should deal with the Antillean-Caribbean Region, the least well-known part of the continent ; but Professor Schuchert explains that it is the logical beginning since the majority of the North American marine faunas came from the south, either from the Atlantic or the Pacific regions, migrating along the geosynclinal zones. The necessity for establishing these routes led finally to an extended review of the geology of the areas bordering the whole of the Gulf of Mexico and the Caribbean seas. This study in turn reacted upon and improved knowledge of the Pacific, Cordilleran, Ouachita and Appalachian troughs of the United States and Canada, and has helped to fix the ancient shorelines of the southern third of the North American continent.

The book is thus " a first attempt to bring together the geologic history of the Antillean-Caribbean region, a vast crustal unit that has evolved structurally under the influence of the periodically dynamic marine basins within and around it." This region—a " framed mediterranean " lying between two continents—is, with its bordering lands, one of the most mobile and unstable of earth zones.

The book is divided into Sections. Section 1—Introduction and Summary—deals with the greater geological features, the diastrophism, and the palæogeography of the region. In Section 2 are described the three Middle American Basins—the ancient Caribbean mediterranean, the Antillean Sea, and the young Gulf of Mexico. The biogeographical evidence bearing on the physical evolution of the region forms the subject of Section 3. Section 4, which occupies the whole of the book from p. 114 onward, deals with the stratigraphy, structure and palæogeography of the lands within and forming the frame of the described region. Seven main regions are delimited : 1, Mexico : 2, the Gulf Coastal Plain : 3, Nuclear Central America (Chiapas, Guatemala, Honduras, and North Nicaragua with its young foreland ; Tehuantepec, Tabasco, Campeche and Yucatan) : 4, the Antilles and their Bahaman Foreland : 5, the Young Isthmian Link connecting Central and South America (Panama, Costa Rica and Southern Nicaragua) : 6, Northern South America (Columbia, Venezuela, Trinidad, Barbados and Tobago) : and 7, the Oceanic Islands. For the geology of the regions represented by this great array of romantic names Prof. Schuchert's book will long remain the main reservoir of facts and references. Very full bibliographies are appended to each section and sub-section, and the work is illustrated by a large number of excellent geological maps and sections interspersed with photographs of the pioneers in the study of the region, and by a series of 16 palæogeographical maps depicting the relations of land and sea from Middle Pennsylvanian to Upper Pliocene-Pleistocene times.

It is, of course, impossible in a short review to discuss Prof. Schuchert's main conclusions on the geological history of this critical region of the earth's crust. It will be sufficient to say that he decisively rejects the Antillean Arc of E. Suess, which was supposed to unite the Andes by means of a great eastwardly-directed loop of fold-structures with the Rocky Mountains cordillera. In Schuchert's view this continuous linking of fold mountains does not exist in actual fact. He holds that the east-west Venezuelan chains, the Lesser Antilles, and the folds of the Greater Antilles and Central America, are separate and distinct features due to different causes, quite independent of the Andes and the Rocky Mountains.

The book possesses a very full index and has been finely produced by the publishers. One important mistake occurs in the heading to Region 3 (Contents, p. xvii) where " Southern Nicaragua " should read " Northern Nicaragua." In this feast of good things it is perhaps ungrateful to lament somewhat scanty treatment of igneous events, although full references to the literature are given. Geologists will await the remaining volumes of Prof. Schuchert's *magnum opus* with impatience.

G. W. T.

BOTANY

The Story of the Plant Kingdom. By MERLE C. COULTER, Professor of Botany, The University of Chicago. [Pp. vii + 270, with 119 figures.] (U.S.A.: University of Chicago Press; Great Britain and Ireland: Cambridge University Press, 1935. 13s. 6d. net.)

THIS book is specially written for a course in Botany or for part of the Biology course in a new four-year college curriculum introduced into the University of Chicago. Apart from this, it is hoped that the book will prove of interest to the general reader.

The main features used to string together the groups is the possible phylogenetic relationships. It might be doubted if to-day this is the best means of approach. There is now much less certainty than formerly about these relationships and in any case it seems scarcely possible to regard the scope of the book as being sufficiently wide to give the reader matter to enable him to critically examine the views put forward. The study of functions and of environmental relationships are very slightly touched upon. Transpiration is dealt with in footnotes, in one of which it is mentioned that evaporation is commonly called " transpiration."

The drawings are clear but are often not exact enough. For example; Fig. 5 does not clearly show either the chloroplast or the " cap-cells " of Oedogonium; in Fig. 7 of Euglena there is no gullet or eye spot; Fig. 47 of a stomate seems to have three intercellular spaces and the walls of each guard-cell are thickened equally. The ovule in Fig. 107 shows a one layered nucellus while the slightly older ovule in Fig. 108 has no nucellus but a single massive integument. The description of the tissues of the higher plant is very inadequate and not very helpful in the elucidation of detailed drawings such as Figs. 71 and 83.

The general " get up " is very good and the book is very readable.

E. M. C.

A Textbook of Practical Botany for Intermediate and Higher School Certificate Students. By WILLIAM LEACH, D.Sc., lecturer in Botany in the University of Birmingham. [Pp. viii + 160, with 23 figures.] (London: Methuen & Co., Ltd., 1935. 4s.)

THIS little book will serve a useful purpose for students studying this subject for the suggested stages. The technical advice given is sound and it is clearly stated while the amount of descriptive statement is strictly limited to the matter in hand.

It would seem scarcely desirable, however, to have so many photomicrographs of sections or preparations of the plants described. It is usually preferable for the student to be able to have an opportunity of interpreting his preparation for himself. The figure of the transverse section of the marram grass leaf is given upside down, and so is the figure of a longitudinal section of a root apex.

The experiments in the Chapter on plant physiology are very good ones and here the illustrations serve a useful purpose.

E. M. C.

Praktikum der Zell-und Gewebephysiologie der Pflanze. By
Dr. Siegfried Strugger. [Pp. xii + 181, with 103 figures.] (Berlin:
Gebrüder Borntraeger, 1935. RM. 8.50.)

Within its limited range this handbook will prove a useful guide to practical
work on the physiology of plant cells and tissues. The ninety-four experi-
ments described deal mainly with the cytoplasm, nucleus and plastids and
with cell permeability and related phenomena. The descriptions of the
experiments are clear, the illustrations are good and the production in general
leaves nothing to be desired. Numerous citations to literature are given at
the end of each of the eleven chapters.

<div align="right">W. S.</div>

AGRICULTURE AND FORESTRY

Problems in Soil Microbiology. By D. W. Cutler and L. M. Crump.
[Pp. vii + 104, with 18 figures and 1 map.] (London : Longmans,
Green & Co., 1935. 9s. net.)

This little book incorporates the substance of a series of lectures given in
the University College of Wales, Aberystwyth, and the contents are largely
the result of work carried out in the General Microbiology Department of
the Rothamsted Experimental Station by the authors and their colleagues.
The book is in no sense a text-book of soil microbiology since only certain
research aspects of the field are considered, and the lectures need to be read
in the perspective of a more general background provided by a more com-
prehensive work.

The authors introduce their subject by showing the suitability of soil
as a home for microbic life, and then proceed to a discussion of the activities
and relationships of bacteria and protozoa under field conditions and those
of pure culture *in vitro*. In the final chapter the interactions between the
soil organisms are considered, and the book closes with a selected bibliography
and an index. The problems dealt with are well illustrated by clear diagrams
and numerous tables. The writing is clear and concise and the authors'
general presentation of their work excellent.

To anyone already possessing an elementary general knowledge of soil
microbiology the book makes excellent further reading since it serves as a good
introduction to the modern research aspects of certain important problems,
especially the rhythmic interrelationships of bacteria and protozoa, and the
interdependence of laboratory and field study.

<div align="right">W. B. B.</div>

A Text-Book on Forest Management. By M. R. K. Jerram, M.C.
[Pp. vi + 156, with 17 figures.] (London : Chapman & Hall, Ltd.,
1935. 10s. 6d. net.)

Between the elementary and advanced texts on Forest Management there
is room for a book of this kind. The elementary book conveys the general
idea : the advanced provides fuller treatment, so full indeed that the wood
is sometimes lost among the trees. The student whose particular need at
a certain stage in his career is to get an adequate but simple framework,
round which he can later build experience, will find this book particularly
useful.

The book presents the subject in three parts. Part I deals with the

Foundations of Forest Management—the regulation of the yield, the relations between growing stock, increments and yield, and silvicultural systems in relation to yield regulation. Part II, incorporating recent work, deals with the Preparation and Control of a Working Plan, and Part III with Forest Finance.

The precise and categorical statement ministers to the needs of beginners and the student is expected from the enunciation of principles to add the qualifications essential for successful practice. If we take the book by itself it may be doubted whether the author's aim is fully met. It seems to the reviewer that too little information about the sets of conditions which influence choice of method has been given. But as the author implies in his preface, the book is to be regarded as a companion volume, dealing with elements, to a qualifying and amplifying lecture course.

<div align="right">A. S. W.</div>

ZOOLOGY

How Animals Develop. By C. H. WADDINGTON, M.A. [Pp. 128, with frontispiece and 30 figures, including 6 plates.] (London : George Allen & Unwin, Ltd., 1935. Cloth, 4s. 6d. ; paper, 3s. 6d.)

THIS little book, shortly and simply written, is the work of one of the leading experimental embryologists in the world. No greater mistake could be made than to think that because it was " popular " it lacked authoritativeness. Many of the facts described are the outcome of Dr. Waddington's own experiments, and many of the ideas are his own contributions towards the theoretical interpretation of our Nature's most important and baffling phenomena—development. Particularly felicitous is the author's gift of lucid explanation with the help of carefully selected analogies, and from all points of view the work deserves the warmest commendation. A subsequent edition will doubtless see to a misprint in Fig. 5, and perhaps to one or two morphological terms used in a slightly heterodox sense.

<div align="right">G. R. DE B.</div>

Tissue Culture : The Growth and Differentiation of Normal Tissues in Artificial Media. By E. N. WILLMER, M.A. Methuen's Monographs on Biological Subjects. [Pp. xvi + 126, with 2 plates and 9 figures.] (London : Methuen & Co., Ltd., 1935. 4s. net.)

" IN this book an attempt is made to give as fair an account as possible of the part played by the method of tissue culture, for it must not be regarded as anything more than a method, in helping to elucidate some of the problems of normal growth and differentiation, and in furthering the knowledge of the processes involved in the normal development of the animal organism." Thus in the Preface the author sets out the aim and scope of the book. He has adopted throughout a critical attitude towards the problems of growth in tissue cultures, pointing out the extensive lacunæ in our knowledge of the mechanism of growth, and emphasizing the necessity for further research.

A brief account is given of the technique for cultivating tissues, and the characteristic types of growth of fibroblasts, epithelial cells, and wandering cells (macrophages) are described. The influence of different media upon growth is discussed, and also the present position of our knowledge of the metabolism of cells growing in cultures. The importance of embryo extract for the maintenance of continuous growth of cells *in vitro* is emphasized,

but the author points out that its mode of action still remains obscure. The latter part of the book is devoted to a review of researches concerned with the differentiation of normal tissues, and the assumption and performance of the functions of various types of cells.

For the student, this book provides a stimulating introduction to the subject of tissue culture, and the author's critical comments on current theories should prove of interest to those who are specially concerned with the problems of cellular physiology.

R. J. LUDFORD.

Genetic Variations in Relation to Evolution. By H. S. JENNINGS. [Pp. viii + 139, with 21 figures.] (Princeton, N.J.: Princeton University Press; London: Humphrey Milford, 1935. 9s. net.)

THIS book is a reprint of a series of lectures given by Prof. Jennings at Princeton University. It begins with an account of genetics which, though very clear, is perhaps over-simplified, and certainly contains some very controversial statements. For example, we read that " The variety among organisms is multiplied a thousandfold—perhaps a millionfold—by reproduction from two parents instead of one." And yet the apomictic *Taraxacum* contrives to be a good deal more variable than related plants with functional pollen.

The main question which the book answers is posed on p. 58. " Do the observed genetic mutations constitute the type of change which has resulted in progressive evolution ? " The author gives a negative answer. " The agents known to produce gene mutations " he writes " are definitely destructive agents. . . . By an astounding paradox, we are asked to believe that they also produce changes on which evolutionary progress is based." Perhaps the paradox is no more astounding than Darwin's theory that the destruction of the unfit is the main cause of evolutionary progress. It is then pointed out that most mutations produce a loss of fitness. They certainly do so under normal conditions. But under abnormally good or bad conditions they often do not. Thus Sax found that white-flowered beans were less fertile than the wild coloured type in bad years, but more so in good years. In other words, the mutation to white is an advantage under more favourable conditions than are found in nature. Similarly, Timoéeff-Ressowsky found that the " bobbed " or short bristled *Drosophila funebris*, though much less viable than the normal in good conditions, was almost equally fit under conditions of overcrowding or heat.

But the most fundamental question of all is this. Are the differences between naturally occurring groups which are rated as species or sub-species of the same character as those produced by X-rays ? The author summarizes Sumner's work on *Peromyscus* and shows that in this case there is no reason to give an affirmative answer. Curiously enough he omits any mention of other work on related species, for example that of Detlefsen on *Cavia* and of Crew and Feldman on *Rattus rattus*, where crosses between different species or sub-species give clearcut Mendelian results. Further, the genes which differ in the two species or sub-species are genes which have mutated under laboratory conditions.

Had our author been willing to consider these cases he could hardly have doubted that *some* evolutionary changes have been due to mutation. The interesting and important question is whether it can be shown that

some changes have been due to other causes. This may well be the case, but it cannot be proved by ignoring all evidence to the contrary. Another example of the same controversial method is to be found in the discussion of chromosome rearrangements following radiation. Here it is stated that chromosome breaks always occur in pairs. The fact is that they are detected in pairs. If a chromosome is broken at only one point there is reason to believe that the break usually joins up again. If there are two breaks a new arrangement of chromatin is possible; and so the effects of two simultaneous breaks are detected, while that of one is not, except by immediate and careful cytological observation.

The last two chapters deal with the genetics of *Protozoa*, and can be wholeheartedly commended, though even here a reference to Raffel's work might have led to a slightly different emphasis.

While I cannot help feeling that the book as a whole is rather one-sided, which is perhaps an inevitable consequence of its shortness, I would cordially recommend it to anyone who has been convinced by such books as R. A. Fisher's or my own that evolution can be explained in terms of modern genetics. Here the contrary thesis is stated with vigour and moderation. I believe that Prof. Jennings' case can be answered, but it is a good case, and no serious student of the theory of evolution can afford to neglect it.

J. B. S. HALDANE.

Heredity and Evolution. By A. E. WATKINS, M.A. [Pp. viii + 243, with 2 plates and 25 figures.] (London : John Murray, 1935. 7s. 6d. net.)

THIS book provides a very helpful introduction to the genetics both of plants and animals. It is written in a form which will be intelligible to students who have no previous knowledge of the subject. The account of the Mendelian theory here given is clear and well developed. Some of its extensions are also explained in a very helpful manner. Thus the subject of chromosome variation, of more importance to botanists than to zoologists, is handled in a thorough and interesting way. Indeed, it occupies perhaps a somewhat disproportionate amount of space.

Certain other sections, however, receive rather insufficient treatment. Thus in the discussion of the *Lymantria* work in the chapter on sex, mention should have been made of the remarkable discovery that the female-determinant is carried in the cytoplasm. In fact no reference is given to Goldschmidt's work later than the now superseded text-book of 1923. More serious, however, is the inadequate discussion of the modification of the phenotypic effects of the genes. No attempt has been made to demonstrate the reality of such changes, well substantiated as they are by experimental evidence, nor to discuss the theory of dominance. Instances should have been given to show that the dominance relations of a gene may be altered by placing it in a new internal environment.

Such omissions have considerably hampered the evolutionary discussion. Thus in order to explain mimicry, the author has to fall back upon the old and untenable hypothesis of parallel variation, due to Punnett. Such a theory will not survive examination. For one of the most striking and frequent features of the resemblance between mimic and model is its complete superficiality. Far from parallel mutations having produced the same effects in each, the apparent similarities are commonly attained by funda-

mentally different means. Once this is realised, other facts relating to mimicry become explicable only when it is assumed that the effects of the genes have been modified by selection. For mimetic resemblances often involve adaptations along several distinct lines, as colour, pattern, and habit, all of which may be under the control of a single factor-pair (*e.g. Hypolimnas mima—wahlbergi*). It cannot be held that evolution has had to wait upon the fortuitous occurrence of mutations such as these. But such instances are easily explained when it is remembered that the effects of genes are subject to genotypic modification, and that this has been demonstrated in mimetic forms.

The important passages in which it is argued that the inheritance of continuous variation is Mendelian, would be strengthened if it were pointed out that the F2 generation has proved more variable than the F1, and the implication of this fact explained. The discussion of fluctuations would have been assisted by introducing the concept of genotype and phenotype. These two words should appear in the Glossary. This is a helpful addition to the book, but the definition of " homozygote " is misleading.

Several rather complex subjects are very well explained. Notably Stern's cytological proof of genetic crossing-over, and the mechanism of ring-formation in chromosomes. On the whole, this is a valuable book and deserves success. It has a useful historical introduction, but Mendel's famous work was not conducted on sweet peas.

E. B. F.

Final Report (July 1935) of the Furunculosis Committee. [Pp. 67, with 2 maps and 2 charts.] (Edinburgh : H.M. Stationery Office, 1935. 2s. 6d. net.)

THIS Committee has existed for six years and two interim reports have been published. In this final account of the bacteriology and the experimental investigations, the whole field is reviewed and the findings of the Committee summarised.

The origin and the nature of the disease have been clearly established, and, although young fish are less susceptible than adults, Furunculosis has spread in an alarming manner and has been most virulent amongst salmon and sea trout fresh in from the sea.

The investigations establish the fact that *Bacillus salmonicida* is of freshwater origin, that it can survive only a short time in sea water, that epizootics do not occur in lakes, but in pools of rivers, and that the water temperatures usually found in the summer months are favourable to its development. There appears to be no proof that the disease has ever occurred in salmon taken in sea nets, and it has been found that smolts do not carry the disease to the sea. The undoubted fact that salmon fresh in from the sea and not yet out of tidal water may die of the disease has seemed to many to be an indication that the disease comes from the sea; the investigations here recorded find no proof of this, but they find that salmon are peculiarly susceptible to the epizootic condition just as they enter fresh water in summer. The higher water temperature may then be the factor which stimulates the organism to activity, since it is found that the accession of a temperature of from 55 to 66° F. gives the optimum development of the disease. The transition from the sporadic to the epizootic state may take place in a few hours or a few days, and in three or four days fish may die.

The peculiar manner in which completely separated river systems have suffered from the spread of the disease has been somewhat puzzling and, again, has induced the belief, or supported the belief, that the origin is in the sea. But very early in the investigations it was found that some apparently healthy fish are carriers, and that the means by which the disease cropped up summer after summer in the same river was by the carriers. It is found also that the carriers themselves ultimately develop the disease and die—as every infected fish does. Up to the conclusion of the enquiries in July of this year, thirty-one English and Welsh rivers and twenty-eight Scottish rivers were infected. When we look at the map indicating these rivers, it is at once apparent how widely separated from others many of them are, and how frequently the disease seems to occur only in upper tributaries. That trout, whether brown trout or sea trout, are delinquents seems to be clear, and, in the spread to separate systems, the ascertained habit of young sea trout to enter rivers for a time and later to return to the sea, move along the coast, and enter another river, may account for much of the spreading. It is ascertained that the organism may lie quiescent for many months, and in interesting tank experiments with a known population and a known population-density to the given water volume, it was found that over 50 per cent. of the trout stock were resistent to the disease even after two annual epizootics. Mr. Hewitt's iodised food specific was considered but not experimented with. Dr. Todd's view of a useful bacteriophage was more carefully followed up, but, so far as the report shows, prophylactic treatment of the disease in any attempted line does little or nothing to check the spread of the disease when an epizootic has started.

This result is a little disappointing, and the curative side distinctly referred to in the remit to the Committee by the two Ministers of the Crown does not bulk at all prominently in the report under review. One may conclude that if a series of preventive measures by treatment of trout in the ponds and the tank employed had been tried, even negative results would have been recorded. The disinfection of fish ova with acriflavine was most useful, although the evidence shows that ova *per se* do not carry the bacillus, but with the ascertained retarding of development in sea water, it would appear as if some experimentation might have been allowed along lines admitting of practical application. As it is, the only recommendations of the Committee are along the lines of prohibition of importation of fish from abroad, and of transference of fish from infected areas. These recommendations were made in an interim report and have not yet been acted upon. Nothing could be better than the bacteriological facts ascertained with regard to *B. salmonicida*, but many will share the disappointment of the Committee that the "methods of combating disease", referred to in the remit, have proved so elusive.

W. L. C.

Cold Spring Harbor Symposia on Quantitative Biology. Vol. I 1933. [Pp. xii + 239, illustrated.] Vol. II, 1934. [Pp. xii + 284, illustrated.] Vol. III, 1935. [Pp. xvi + 359, illustrated.] Published at the Biological Laboratory, Cold Spring Harbor, L.I., New York.

THE practice of holding an annual Symposium at Cold Spring Harbor on topics related to Biology and to the light thrown on some of its problems

by the mathematical, physical and chemical methods of approach, is an interesting experiment in itself, which the publication of the proceedings (including full reports of the questions and answers following each paper) enables all biologists to follow. In many cases the discussions have been carried on through the post with persons who were unable to attend the meetings.

Volume I contains twenty-eight papers, relating to bioelectric phenomena, osmosis, and oxidation-reduction indicators.

Volume II contains thirty-one papers, of which about half are devoted to various aspects of growth, four deal with the gene, and the remainder treat of such varied subjects as biological radiations, X-ray studies of proteins, deuterium, and ultra-violet microscopy.

Volume III contains thirty-five papers, devoted mostly to the various aspects of photosynthesis and other biological effects of the action of light.

Space forbids any detailed reference to these papers, but mention may perhaps be made of Chambers's study of oxidation and reduction indicators, Astbury's work on fibre-structure, Newton Harvey's account of bioluminescence, Gudernatch's most suggestive essay on the phylogenetic distribution of specific chemical factors influencing growth and differentiation, Sewall Wright's correlation of growth and genetics and Rahn's study of mitogenetic rays.

The organisers of the symposia are to be congratuated on their achievement.

G. R. DE B.

MEDICINE

Fundamentals of Biochemistry in relation to Human Physiology. By T. R. PARSONS, M.A., B.Sc. Fifth edition. [Pp. x + 453, with 26 figures, including 1 coloured plate.] (Cambridge: W. Heffer & Sons, Ltd., 1935. 10s. 6d. net.)

THE usefulness and popularity of this well-written text-book is shown by the fact that a new edition has already been called for although the last appeared only two years ago. The opportunity has been taken of bringing the book up to date, and so additional matter has been added, more particularly to the sections on muscle metabolism, tissue oxidations, sex hormones and certain of the vitamins. These are the branches of the subject in which progress has been most rapid and spectacular. Special mention should be made of the well-chosen bibliography, including references both to books and original papers, at the end of each chapter. The author and the publisher are to be congratulated for producing a very useful text-book at a very reasonable price.

W. O. K.

The Structure and Composition of Foods. Vol. II : Vegetables, Legumes, Fruits. By ANDREW L. WINTON, Ph.D., and KATE BARBER WINTON, Ph.D. [Pp. xiv + 904, with 303 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd, 1935. 75s. net.)

THIS volume is one of a trilogy dealing with the structure and composition of food. It is encyclopædic in size and weight and will be an essential on

the shelves of any reference library of botany, physiology, biochemistry or domestic science. Naturally the outlook of the writers is predominently botanical, but none the less they have included a great deal of chemical data and there is an up-to-date introduction dealing with the complicated chemistry of the vitamins and plant pigments. The publication of this book will be welcomed by all who are interested in nutrition.

V. H. M.

Destiny and Disease in Mental Disorders. By C. MACFIE CAMPBELL, M.D., Professor of Psychiatry, Harvard University. [Pp. 207.] (London : Chapman & Hall, Ltd., 1935. 10s. 6d. net.)

THE above book inaugurates a series of volumes, which are to be based upon the Thomas W. Salmon Memorial Lectures delivered under the auspices of the New York Academy of Medicine ; and it is interesting to find that this first volume is devoted to the study of the schizophrenic psychoses, which constitute a sort of " no man's land " between the provinces of internal medicine and psychopathology. While it is no longer in dispute that the organic, toxic and symptomatic psychoses fall essentially within the former province, the claims of the psycho-pathologist to regard the psycho-neuroses as his special concern and even to offer a psychological explanation of the problems of the manic-depressive or " affective " psychoses is generally conceded. The etiology of the schizophrenic psychoses is, however, more obscure ; and this fact has tempted Dr. Campbell to review a considerable amount of relevant case material with the object of illustrating the extent to which schizophrenic symptoms may be interpreted as attempts on the part of their victims to adapt to the situations of life. His thesis is that, whereas bio-physical investigations are not without significance, these must be evaluated in terms of the individual personality and needs of the patient in relation to the demands made upon him by his environment conceived in the widest sense. While this thesis cannot be regarded as constituting any new development in psychiatry, Dr. Campbell's book is representative of a dynamic point of view, which is steadily gaining ground in the psychiatric world, and which promises to yield ever-increasing insight into the obscure problems of mental disease.

W. R. D. F.

Outlines of General Psychopathology. By WILLIAM MALAMUD, M.D., Professor of Psychiatry, State University of Iowa. [Pp. xiv + 462.] (London : Chapman & Hall, Ltd., 1935. 21s. net.)

IN recent years psychopathology has undergone such rapid developments along so many divergent lines of thought that a broad grasp of its funda-mental principles is difficult to attain ; and it is with the object of presenting these fundamental principles in systematic form that Dr. Malamud has undertaken the present work. Throughout he expressly recognises the limitations imposed by the claim of psychopathology to be an independent science. In virtue of this claim, he points out, psychopathology must restrict itself to promoting an understanding of abnormal mental phenomena and must avoid encroaching either upon the field of medicine, which is therapeutic in aim, or upon that of psychology, which seeks an understanding of mental activity in general irrespective of its normality or abnormality. At the same time the author deprecates the view that psychopathological

phenomena simply represent deviations from normality and insists upon regarding them as products of an attempt to reach an adaptation to life in terms of the individual's congenital endowment, his past experience and factors inherent in the situations he is called upon to meet. This point of view involves a rejection of the growing tendency to regard " normal " as equivalent to " average " and implies the acceptance of an ideal (in the Platonic sense) of normality as a state of perfect adjustment, which not even the most " normal " person completely attains. In the light of such an ideal of normality, what distinguishes the mentally abnormal from the mentally normal person is that the former falls short of average adjustment to an incapacitating degree. While the explicit adoption of this conception is perhaps the most significant feature of the present work, the author's clear statement of basic problems and painstaking effort to isolate fundamental principles merit the attention of every worker in the field of psychopathology. At the same time the reader who desires to acquire a working knowledge of the origin and development of abnormal mental states will doubtless find himself compelled to turn to the writings of one of the schools of psychopathological thought, from which Dr. Malamud deliberately holds himself aloof.

<div align="right">W. R. D. F.</div>

HISTORY OF SCIENCE AND BIOGRAPHY

Antoine Lavoisier, the Father of Modern Chemistry. By D.McKIE, Ph.D., B.Sc., A.I.C. With an introduction by F. G. DONNAN, C.B.E., LL.D., D.Sc., F.R.S. [Pp. 303, with 3 plates and 4 figures.] (London: Victor Gollancz, Ltd., 1935. 10s. 6d. net.)

THE period of the history of Chemistry from Boyle to Lavoisier, inclusive, seems to have had a peculiar attraction for English authors and in his new book Dr. McKie has largely followed traditional lines. As he says, between Boyle and Lavoisier there stand Black, Priestley, Cavendish and Scheele. He says practically nothing of Scheele, whose work was of great importance, and he has only a passing word for Mayow, the recent harsh judgment of whom has apparently scared off many students who could with advantage have reconsidered the position. He gives a very able account of Black. The period under review includes the era of phlogiston. Nothing significant has been written about this since Kopp and nothing is added by Dr. McKie, since it is in a way out of the main line of his study. The title of the book is a challenge, and it is surely unjustified to call (p. 154) Priestley " the first pneumatic chemist." Even if we do not wish to consider Mayow and Hales, Cavendish's paper of 1766 cannot well be left out of account. It has recently been the fashion to criticise sharply the view, which curiously enough is Priestley's own, that the latter was an unsystematic worker, and Dr. McKie is in the fashion. Nothing, however, can hide the random collection of materials used in Priestley's experiments, nor, if the usual English authors and Dr. McKie had favoured us with an account of Scheele's investigations, could the great difference in method have escaped our attention. Too much attention has perhaps been given by historians of chemistry to combustion ; there are other branches of the science, and the idea of equivalents, for example, was known before Lavoisier's work.

Dr. McKie's book is one which will add to his already excellent reputation as a serious and well-informed writer on the history of Chemistry, and it shows

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clearly that its author has made genuine researches on his subject.　It gives an interesting and accurate account of what is significant in Lavoisier's progress to that great culmination which so narrowly escaped the sharp edge of the guillotine, and it sets out the contributions of some of his predecessors who probably influenced him.　If the book follows rather closely the familiar tradition of English authors, this will probably be regarded by most of its readers as calling for praise rather than criticism.　Prof. Donnan's introduction contains some well-put general observations on the value of the history of Chemistry which deserve to be read and considered ; he speaks with authority, since his edition of Mayow in Ostwald's *Klassiker* is an example of the best type of work in this field.　Dr. McKie's book is one which can be strongly recommended.

J. R. P.

Sir Donald MacAlister of Tarbert.　By his WIFE.　With chapters by SIR ROBERT RAIT and SIR NORMAN WALKER.　[Pp. viii + 392, with 1 plate.]　(London : Macmillan & Co., Ltd., 1935.　12*s.* 6*d.* net.)

THIS biography is more than a record of achievement : it is a portrait, drawn to the life, of a truly remarkable man.　Successive chapters depict Sir Donald MacAlister's schooldays, first in Scotland and later in Liverpool, where he gained scholarships which enabled him to proceed to the University ; his career at Cambridge, where before entering the course of training for his chosen profession, medicine, he devoted himself to the study of mathematics and gained the distinctions of Senior Wrangler and First Smith's Prizeman ; the completion of his medical curriculum at St. Bartholomew's Hospital, followed by a year of research in Physiology at Leipzig ; his return to Cambridge for some years of great activity as Fellow and Tutor of St. John's College ; his invaluable service to the University of Glasgow during his twenty-two years' tenure of the office of Principal and Vice-chancellor, followed in 1929 by his election as Chancellor in succession to the Earl of Rosebery.

It was during this last part of his life's work that his genius as an administrator and his qualities as an inspiring leader found full scope, to the lasting benefit of the University, but his arduous duties in Glasgow did not exhaust his energy, for he devoted much time and thought to the affairs of the General Medical Council, of which he was President for twenty-seven years.　A most interesting account of the unprecedented development in every direction of the University of Glasgow during his Principalship is given in a chapter contributed by his successor, Sir Robert Rait, and an additional chapter by Sir Norman Walker contains an appreciation of his services to the General Medical Council.

Other chapters of the book describe vacations spent in extensive travels on the Continent and America, which afforded opportunities for the exercise of Sir Donald's exceptional gifts as a linguist, and after the War in his own country, where as a trustee for Iona he took a very active interest in the restoration of the ancient cathedral.

In his earlier years Sir Donald was rather delicate and the strain of constant overwork left its marks upon him.　Towards the end of 1914 he was so seriously ill that his life hung in the balance, and this illness left a legacy of precarious health which added to his burden during the rest of his life but which, through the exercise of an indomitable will, was not

permitted to prevent him from undertaking everything which he felt to be a duty.

Lady MacAlister's delightful biography of her husband is sure of a warm welcome from a wide circle of friends.

G. G. H.

Great Doctors of the Nineteenth Century. By SIR WILLIAM HALE-WHITE, K.B.E., M.D., LL.D.(Hon.), F.R.C.P. [Pp. vii + 325.] (London : Edward Arnold & Co., 1935. 15s. net.)

IN this charming collection of short biographies of physicians and surgeons who have contributed so much to the honour and progress of medicine and surgery in this country during the nineteenth century, Sir William Hale-White has supplied a volume of much value which will give great pleasure to members of the medical profession and will surely be of interest to many non-medical readers.

The dominant notes which link these great lives are intense energy, unremitting labour ; in many cases early struggles with adversity ; but in all an insistent personality so different and peculiar to each. The author has successfully conveyed this " character atmosphere," forcing the reader to feel the country naturalist in Jenner, the unresting hurry of Bell, the calm kind sympathy of Paget, the careful " seeing " of Bright, the dominant " Doctor of fashion " in Gull, the majestic deliberation of Lister, the lovable Wilks, and the emotional Ross. In spite of the statement on p. 56 " the unusual combination of artist and scientist " it is remarkable that in this small sample there are seven (nearly 40 per cent.) who were both scientific men and artists, viz.: Bell, Bright, Stokes, Graves, Paget, Bowman and Ross.

It is evident that the prearranged size of the book has often handicapped the freedom of its literary style. In places the reduction and omission of paragraphs has produced a sense of breathlessness, and suggests the catalogue manner of *Who's Who*. At the risk of being greedy after receiving so much that is good we plead for portrait illustrations which would not only add to the value (and perhaps price) of the collection, but make our acquaintance with these great men more intimate and real.

P. J.

MISCELLANEOUS

Prehistoric Man in Ireland. By CECIL P. MARTIN, M.B., M.Sc. [Pp. xii + 184, with 11 plates, 6 figures and 17 tables.] (London : Macmillan & Co., Ltd., 1935. 21s. net.)

DR. MARTIN has performed a most notable service in writing this book. He has collected together and detailed in an exact and critical form the material available for the study of the inhabitants of Ireland from the earliest known remains down to those of the Norsemen. This survey is preceded by a general discussion of archæological and anthropological methods, some of which is no doubt somewhat elementary, but will probably be useful to the reader who approaches the subject without any previous knowledge. He also publishes tables of measurements of all known ancient skulls which can be dated and of some whose date is uncertain. The anthropology of Ireland is particularly interesting in relation to that of England because, though the earlier

people of the Neolithic and Bronze Ages are practically similar, Ireland was not troubled either by a Roman or a Saxon invasion. The Norsemen played terrible havoc.

The population of Ireland Dr. Martin believes to be derived from three sources, the river-bed men, the Iberian race of the Megalithic monuments and the roundheaded people of the Middle Bronze Age, to which he considers were added some other strain from Early Iron Age invaders and considerable Nordic elements from the Norse, Anglo-Saxon and Norman stocks. He concludes that the present population of Ireland differs from that of most of Great Britain in having a larger Iberian element and a smaller Nordic one.

The general question is one of great interest. There can be little doubt that the Megalithic culture came from the South-western shores of Europe. So there is good reason to call it Iberian. But the race is another question. Generally speaking, the shape of the Nordic and the Mediterranean skulls is very similar, but their size is very different. The Mediterranean people from Spain to Egypt and beyond have small gracile skulls, the Nordic have large skulls. It is true that most of the Nordic peoples, ancient and modern, tend to be mesocephalic rather than dolichocephalic. It is by no means certain, however, whether these peoples are not the result of crosses between various early races. The balance of evidence which we have at present suggests that all the early peoples of the British Isles, including the Megalith builders but excluding the Beaker Folk of the Bronze Age, are really Nordic physically whatever the origin of their culture. The whole matter is of great difficulty and needs a good deal more research. Dr. Martin's account therefore of the Irish people is particularly valuable both because of the careful way in which scattered material has been collected and critically examined and because of the restrained and balanced way in which he has put forward his conclusions.

L. H. D. B.

Race, Sex and Environment : A Study of Mineral Deficiency in Human Evolution. By J. R. DE LA H. MARETT, B.Sc. [Pp. 342, with 12 figures and 1 folding plate.] (London : Hutchinson's Scientific and Technical Publications, 1935. 21s. net.)

MR. MARETT's book might not inaptly be described as a study in the mechanism of evolution. Impressed by study of the effects which have been observed in animal husbandry to follow from mineral deficiencies in the soil and the resulting defects in nutritive values of the foods produced from it, he has applied the principle thereby suggested to the study of human evolution, extending it to cover not merely the evolution of physical characters, but also the development of the social activities and institutions which are the outcome of man's mental constitution. The correlation of human types and human activities with geographical conditions has long been a subject of observation in anthropogeographic studies. Dr. Marett's hypothesis bridges the gap between organism and environment by suggesting that the mineral constitution of the soil is the causal factor in their interaction, functioning as the modifying force in the evolutionary process through its action on endocrines. This is here exemplified in the evolution of the big brain, the upright posture and hairlessness in man. Mr. Marett also indicates how climatic change in pleistocene times may so have affected the chemical constitution of the soil as to have caused the extinction of the big-boned races

known to us as fossil forms of man, leaving the stage clear for "modern" man.

As already mentioned, Mr. Marett does not confine his attention to the evolution of man's physical characters, but extends it to social institutions, such as, for example, marriage in its diverse forms, shows how it may produce varied types of society and sounds a warning against certain trends in modern civilisation.

It will be appreciated that much of Mr. Marett's book is of a highly speculative character. None the less it is a valuable and stimulating piece of work which opens up a wide and varied field for further research.

E. N. F.

Ubena of the Rivers. By A. T. and G. M. CULWICK. With a Chapter by MTEMA TOWEGALE KIWANGA and an Introduction by Dr. L. H. DUDLEY BUXTON. [Pp. 444, with 5 plates, 2 maps, 1 figure and 1 chart.] (London: George Allen & Unwin, Ltd., 1935. 16s. net.)

THIS book is an attempt to present a balanced picture of the life and outlook of a typical Bantu tribe. The Ubena of the Rivers—in contrast to the Ubena of the Hills—inhabit the upper part of the Ulanga Valley in Tanganyika Territory and the authors have not only lived among the people and gained their confidence, but they have enjoyed the particular good will of the royal house. Indeed Mtema Towegale Kiwanga had written a history of his tribe which was unfortunately lost when his canoe was attacked by a hippopotamus in 1930 and he, with his family, eagerly prompted the authors to set down the record which remained only in their memories. All phases of life have been considered—history, religion, tribal organisation, social organisation, war, hunting, crime and law, land rights, tenure, and production, family life, medicine and recreation. This book, as Dr. Dudley Buxton points out in his introduction, belongs to the class of studies made at leisure and it has clearly benefited in many ways. The investigators are husband and wife and they have been able to investigate singly aspects of tribal life which would have remained unknown to a lone student of either sex. Thus Mrs. Culwick was able to witness the two initiation ceremonies of the girls on reaching the age of puberty. Although the second is made the occasion of a picnic for the older women, one is struck by the clever way in which the matters of chief importance in sexual instruction are imparted to the girls, each backed up by some little object lesson to press home its meaning. It is almost a model for some civilised races ! Africa changes fast and this record of the present is made all the more valuable on that account.

L. D. S.

Soviet Geography : The New Industrial and Economic Distributions of the U.S.S.R. By N. MIKHAYLOV. [Pp. xviii + 232, with 38 maps.] (London: Methuen & Co., Ltd., 1935. 10s. 6d. net.)

THIS is a remarkable book. It deals with the economic geography of the U.S.S.R. in terms of a political and social philosophy, and in a style more rhetorical than scientific. The author bases his work on official sources of information which, in many cases, are not accessible to outsiders. He gives a very vivid and challenging account of the progress made in the exploration and utilisation of the natural resources of the enormous territory of the Soviet Union, one continuous land area extending from latitude 36° N. to the North Polar Seas with an extent equal to two-thirds of that of the British

Empire. This is the second in area and the third in population among the World Powers; and it may be the first in mineral resources.

The chief topics are, in order of treatment, the exploration of the land and its natural resources, the new distributions of manufacturing industries, of agriculture, of transport, and of population. In all of these there is a record of achievement which has no parallel elsewhere in the world. But when the author tells us, on pp. 58 and 115, that by 1937 the U.S.S.R. will be first among the industrial and agricultural producers of the world in each of a wide range of industries and products, it seems that prophetic fervour is taking the place of sober statement of fact.

There are 38 map-diagrams. The first compares the U.S.S.R. with the rest of the world on the basis of a mercator net, not of an equal-area net, and so gains emphasis by the sacrifice of accuracy. The second bears a network of lines which suggest parallels and meridians; but these lines do not form a regular mesh, nor do they mark accurately the latitudes or longitudes of many of the physical features shown. Some of the later diagrams of production are too complicated to be easily read. And many of the diagrams seem to subordinate accuracy to impressionism.

Yet, though the text and diagrams would be more convincing if they were consistent and accurate where we can test them by relation to one another or to external facts, the book is of real importance. It gives a convincing picture of the power of a unified control in dealing with the resources of the land, and of the great progress made under the Soviet regime. It pictures the scientists' dream of the rational study and utilisation of natural resources; though it can hardly be said to reduce the dream to *cold* fact. If the success achieved is even a fourth of that claimed, then the U.S.S.R. will soon be the greatest of the economic Powers and a dominating force in World Politics. This book deals with only one aspect, though a fundamental one, of the Soviet Union; it goes to confirm the view that to-day the U.S.S.R. is, in more senses than one, pre-eminently a Land of Hope.

C. B. F.
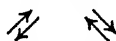
Human Ecology. By J. W. BEWS, M.A., D.Sc. [Pp. xii + 312.] (London : Oxford University Press, 1935. 15s. net.)

THIS work is an attempt to apply lessons learned from plant ecology to the study of some of the human sciences. It is systematically arranged, so as to present clearly the author's outlook on a very complex group of studies. The introductory chapter begins by asserting the essential unity of the " biological triad " of " organism-function-environment," no one part of which can be conceived (*i.e.* for the human mind, exist) without the other two. Ecology here denotes not a branch of science, but an attitude of mind toward all the biological and social sciences which regards this unity as a fundamental truth. The next chapter is devoted to a sketch of the environment—physical, geographical, biological, social, and internal. Then follow three chapters on Heredity, Response to Environment, and Psychology. These sections, in which the author, to use his own words, looks at the triad first from the end of the environment and then from that of the organism, occupy the first third of the book. In the rest he approaches the subjects from the aspect of function, of man's adaptation of his mind and habits and society, of his material and moral equipment, to the environment, and his modifications of and influence on several aspects of that environment. This occupies more than half of the book. It includes chapters on Anthropology,

the culture of Ancient Man, the Food-Gatherers, Plant Cultivators, and Herdsmen, and on Social Differentiation. Last is a brief " General Discussion."

Such a summary of contents indicates the enormously wide range of the work. Yet every section is treated in relation to the fundamental unity of the triad, a concept which gives a real unity to all these varied topics. Each chapter is followed by a select bibliography, which indicates a wide range of reading. It is improbable that any author, or any reviewer, can be expert in all the branches of knowledge here treated. But Dr. Bews has made good use of his reading in those parts which the present reviewer is competent to criticise. The ecological method involves regarding the triad as a whole ; and therefore it produces a synthetic study. But actual detailed research must normally be directed to some specific problem in some particular part of the borderlands of knowledge ; and it is necessary to isolate, at least mentally, the objects of investigation, though it is also vital to remember that in the world of fact they are but parts of one complex whole. Dr. Bew's work reminds one of attempts at a similar task which occupied the late Sir Patrick Geddes, especially in his last years. He had experimented with statements of the problems by increasingly elaborate symbols and diagrams, of which his " river-valley section " is probably the best known. The Geddesian

diagram of $\begin{smallmatrix} & \text{PLACE} & \\ \nearrow & & \nwarrow \\ \text{WORK} & \rightleftharpoons & \text{FOLK} \end{smallmatrix}$ may be taken as nearly equivalent to Dr. Bew's

ecological triad of " environment-function-organism " ; though in Geddes' sociological view the FOLK is a human society and not an individual human being. Denoting these by the symbols p, w, f, we may indicate the various fields of investigation by the possible mathematical combinations and permutations among these symbols, e.g. " fw "≡the work (occupations) as influenced by the society (its size, composition, ability, organisation, level of culture, needs and desires, and so on), for the six pairs and six trios derived from the triad. If we consider as a fourth factor the individual human being " h " the combinations become more numerous and complex. Also each of these single factors is, in itself, complex. But this method of lay-out seems able to ensure that no part of the resulting field of study is missed ; while it recognises the omnipresent interweaving of all the factors.

This book has two claims to notice : (1) as a concise summary of a wide field in the sciences of life, and (2) as a serious attempt towards the unification of these sciences in the light of the holistic philosophy of the Rt. Hon. J. C. Smuts, who contributes an introduction. In both aspects the book is a stimulus to further thought and investigation on which the author is to be congratulated.

C. B. F.

Psychology in Business and Industry : An Introduction to Psychotechnology. By JOHN G. JENKINS. [Pp. xii + 388, with 20 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1935. 12s. 6d. net.)

THIS book is designed for the use of undergraduate students who wish to get a general introduction to the psychological problems of business and industry. The writer points out that applied psychology is really two disciplines, viz. research for the purpose of reaching generalisations, and the arts of practice. The two aspects are often confused with serious consequences.

In six sections there are considered problems concerned with Industrial Selection, Industrial Production, Personnel, Industrial Distribution, and Market Research.

The student who has read this book will be left with no delusions that psychology will be able to provide short cuts to the prediction of industrial success. In every section there is issued the warning that the laws of Logic and Statistics must be fulfilled if valid results are to be obtained.

As a sketch the book fulfils its purpose. How much the undergraduate reader will understand the statistical discussion is doubtful, and there is a lack of balance in a book of this nature that devotes almost the same space to the Effects of Narcotics as to Problems of Personnel.

M. S.

Training in Industry : A Report embodying the results of Inquiries conducted between 1931 and 1934 by the Association for Education in Industry and Commerce. Edited by R. W. FERGUSON, B.Sc., A.R.C.S. [Pp. xi + 156.] (London : Sir Isaac Pitman & Sons, Ltd., 1935. 6s. net.)

THIS book is a general summary of inquiries that have been made during three years from about forty firms and industrial concerns. It is edited by R. W. Ferguson whose work as Hon. Secretary for the Association for Education in Industry, and his experience with Messrs. Cadbury are well known and contains 87 pages of Report and 69 pages of Appendices of schemes of educational training adopted by fourteen large industrial concerns. The Appendices giving particulars about what is being done both for the selection of entrants to the firms and for their training afterwards, are instructive and interesting reading.

In the general introduction the following extract from a minute passed by the Council is given : " The adaptation of entrants to business ways, effort and enterprise is the crucial process in determining their suitability for business, and this process should be carried out by business in business."

It is pointed out that although the Council passed this minute they fully realised and appreciated the importance of commercial and technical education obtainable in institutions conducted by public education authorities, and as the Council proceeded with the Inquiry it became evident that no strict differentiation is practicable between training within the industry itself and the education provided by the technical or commercial college, and that any attempt to separate them into watertight compartments would be unwise.

The first four chapters are of special interest, and there is general agreement about the importance of Day Continuation Schools, of continued education in evening classes and of incentives to attend special business courses arranged by the firms. Opportunity for promotion for workers to rise from the bottom are shown in the Appendices. The remaining chapters deal largely with the consideration of training courses for Foremanship, Salesmanship, Management Executives and Administrative Officers, which are arranged by the firms themselves.

An addition to Appendix XV of important books and other publications, and some mention even incidentally, of Associations and Institutions that have been doing kindred work would have been an advantage to those members of the Association and to general readers who may be unacquainted with what has been done outside the Association.

The book is a valuable addition to previous Reports and is of great interest to those engaged in Education in Industry and Commerce especially to commercial management executives. A useful reference book to Local Education Authorities.

<div align="right">C. T. MILLIS.</div>

An Examination of Examinations. By SIR PHILIP HARTOG, K.B.E., C.I.E., and E. C. RHODES, D.Sc. [Pp. 81, including two Appendices.] (London : Macmillan & Co., Ltd., 1935. 1s.)

ALL who are interested in the hazards of examinations (and who is not ?), will read this remarkable Report summarising the prolonged investigations into the variations disclosed upon the scrutiny of examiners' markings from the vast number of scripts from candidates for Special Places in Secondary schools to the comparatively small number of candidates for University Scholarships of great importance, with profit, some real misgivings, some hope of the possibility of better methods yet to be devised, and with great gratitude to Sir Philip Hartog, Dr. Rhodes and the Committees of Investigation behind them.

The First School Certificate Examination in English, French, Latin, History and Chemistry is first considered and reveals divergences of the most astonishing kind. The only consolation that can be derived from the situation is that in the case of a small-scale examination there will be an internal logic and consistency in the examiner or few examiners concerned, but in the case of examinations taken by a very large number of candidates the necessarily larger team of examiners makes the divergencies much more pronounced and difficult to evaluate. This is seen on p. 20 where it is stated that in a certain examination one examiner ploughed nineteen candidates, no other examiner ploughed more than seven, and two ploughed none. The results disclosed in the Special Places examinations are somewhat less disquieting, but it is not difficult to guess that the scripts scrutinised came from only one or at most two large-scale L.E.A.s. The investigation needs to be extended to the One Examiner type of Special Places Examination. The most astounding results, however, are those of the two parallel Examining Boards for University Honours in History. These deserve the most careful and repeated reading and reflection.

It is a great pity that so far there does not appear to have been carried out the same scrutiny of the results of the newly established Joint Boards for the examination of the Two Year students in the training colleges of the country. These Boards are numerous, highly organised and conducted by Examiners of great experience and ability. But some uneasiness may not unfairly arise as to the comparative worth of the Certificates issued by these Boards, in view of the fact that upon their results the Board of Education does or does not issue its licence to teach as a recognised certificated teacher in Approved schools.

It is clear that if examinations are a terror and a nightmare to the candidates, they are not less so to any conscientious examiner.

<div align="right">A. A. C.</div>

Tools of To-morrow. By JONATHAN NORTON LEONARD. [Pp. vii + 303, with 16 plates.] (London : George Routledge & Sons, Ltd., 1935. 12s. 6d. net.)

THE development of art has been a long, slow and continuous process, that of science short, rapid and discontinuous. Only when man had learned

the secret of mass-transportation—of people, goods and power, did civilisa-
tion assume its present aspect. The steam and petrol engines, the Bes-
semer converter and the dynamo have made this possible and transformed
the face of the earth. What of the future ? This book gives an account
of the problem and indicates the direction of future developments. Many
of the tools of to-morrow are already in our hands and need only to be
developed and perfected. The Diesel engine may soon bring about changes
in our transport system comparable only to the achievements of the petrol
engine in the past ; metallography and the invention of new alloys, par-
ticularly of the rarer metals of which beryllium is the forerunner, may rival
in importance the changes caused by Bessemer steel ; to-morrow the hydro-
genation of coal will contrast with the fixation of nitrogen of yesterday ;
geophysical prospecting will extend our knowledge of the distribution and
extent of raw materials in the earth ; the problems of the distribution of
power from super-power stations may be solved ; television has enormous
potentialities ; machines, which to-day have hands, may to-morrow have
eyes as the use of photo-electric cells develops, and mass production will
raise still more formidable economic problems as some of the newer ways of
handling metals, die-casting, extrusion and broaching are more and more
widely applied. The chapter on transportation is specially interesting. Few
can have realised how the value of our social and economic life depends upon
the relative ease with which passengers and freight can be transported, and
the enormous changes which result from a disturbance of this equilibrium.
These are only some of the points raised and discussed in this book. It
is written in popular style, the language is lively and the contents informative
and often entertaining.

<div align="right">J. N. S.</div>

The Subject Index to Periodicals, 1935. [Pp. xii + 570 columns.]
    (London : The Library Association, 1936. £3 10s. net.)

It is now twenty years since the Library Association first started to issue
its valuable subject index. In that time the work has grown in size, accuracy,
and value to an extent which may well entitle the general editor and his
team of voluntary contributors, as well as the Association itself, to a feeling
of no little pride in their achievement.

The present volume, covering articles published during the year 1935
to the number of approximately 26,000, is the result of the examination
of 539 English and American periodicals and of 50 printed in foreign languages.
It has as special features not only the fact that the journals surveyed are
in large measure of a kind not included in other, more specialised, indexes
to periodical literature (indeed, the Library Association deliberately excludes
those periodicals which are covered by the recognised indexing publications,
e.g. Engineering Abstracts, Index Medicus, Science Abstracts), but also a
most praiseworthy rapidity of publication, as a result of which it has on
this occasion been issued within five months of the close of the year under
review. For these reasons the *Subject Index to Periodicals* may be warmly
recommended to all scientific workers who desire to be satisfied that they
do not miss articles in their subjects, more particularly when printed in
unexpected journals, as well as to those in charge of libraries or information
bureaux who are frequently required to answer questions at short notice
or to compile exhaustive bibliographies.

<div align="right">J. W.</div>

BOOKS RECEIVED

(Publishers are requested to notify prices.)

Theoretical Mechanics. Dynamics of Rigid Bodies. By William Duncan MacMillan, A.M., Ph.D., Sc.D., Professor of Astronomy, the University of Chicago. New York and London : McGraw-Hill Publishing Co., Ltd., 1936. (Pp. xiv + 478, with 82 figures.) 36s. net.

Theoretical Astrophysics. Atomic Theory and the Analysis of Stellar Atmospheres and Envelopes. By S. Rosseland. The International Series of Monographs on Physics. Oxford : at the Clarendon Press ; London : Humphrey Milford, 1936. (Pp. xx + 355, with 44 figures and 17 tables.) 25s. net.

You and the Universe. Modern Physics for Everybody. By Paul Karlson. Translated by Bernard Miall. London : George Allen & Unwin, Ltd., 1936. (Pp. 325, with 165 sketches and 8 plates.) 12s. 6d. net.

Heat for Advanced Students. By the late Edwin Edser, A.R.C.Sc. Revised edition by N. M. Bligh, A.R.C.Sc., A.I.C. London : Macmillan & Co., Ltd., 1936. (Pp. x + 487, with 204 figures.) 6s.

Geometrical Optics. By H. T. Flint, Ph.D., D.Sc., Reader in Physics in the University of London, King's College. London : Methuen & Co., Ltd., 1936. (Pp. x + 266, with 128 figures.) 7s. 6d.

Elements of Practical Aerodynamics. By Bradley Jones, M.S., Professor of Aeronautics, University of Cincinnati. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1936. (Pp. vi + 398, with 123 figures and 17 tables.) 18s. 6d. net.

Principles of Electric and Magnetic Measurements. Part I : Electricity. By P. Vigoureux, of the National Physical Laboratory. Part II : Magnetism. By C. E. Webb, of the National Physical Laboratory. The Student's Physics, Vol. VII. London and Glasgow : Blackie & Son, Ltd., 1936. (Pp. xii + 392, with 197 figures, including 2 plates.) 20s. net.

The Earth's Magnetism. By S. Chapman, M.A., D.Sc., F.R.S., Professor of Mathematics, Imperial College of Science and Technology, University of London. Methuen's Monographs on Physical Subjects. London : Methuen & Co., Ltd., 1936. (Pp. xii + 116, with 35 figures.) 3s. 6d. net.

Cathode Ray Oscillography. By J. T. MacGregor Morris, M.I.E.E., University Professor of Electrical Engineering, Queen Mary College, University of London, and J. A, Henley, M.Sc.(Eng.). Vol. II of a Series of Monographs on Electrical Engineering under the Editorship of H. P.

Young. London: Chapman & Hall, Ltd., 1936. (Pp. xiv + 247, with frontispiece and 151 figures, including 20 plates.) 21s. net.

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SCIENCE PROGRESS

THE REGULATION OF THE HÆMOGLOBIN IN THE BLOOD OF MAMMALS

By A. E. BOYCOTT, M.A., D.M., LL.D., F.R.S.

1. HÆMOGLOBIN must have been invented by animals a good many times in the course of their evolution. Invertebrates have tried a variety of respiratory pigments to carry oxygen from the environment to the tissues and hæmoglobin occurs scattered about in various unrelated groups as in earthworms, insects (*e.g.* bloodworms, the larvæ of midges) and mollusca (*e.g.* the ram's-horn snail, *Planorbis*). Experience seems to have shown what modern experiment confirms, that it is the most useful oxygen carrier yet discovered, and it becomes universal in vertebrates, enclosed in special cells to prevent waste. Its unique efficiency culminates in its use by birds and mammals in which, in combination with a rapid circulation, it has become essential. As warm-blooded animals they live fast and have active vivacious lives, needing a constant supply of abundant oxygen. There is no store of oxygen in the body and though the activity of the tissues may overrun the supply for a short time their needs in general have to be met from minute to minute. The strenuous lives of the higher animals depend, therefore, on hæmoglobin, which is for them a necessity, though this is far from saying that they could not have been developed without it. Animals generally have several alternative modes of achieving their ends and it does not seem impossible that something like the tracheal system of insects (which also live very fast) could have been adapted to supply the incessant stream of oxygen which mammals require. As things have happened, however, they have used hæmoglobin and, as might be expected, have developed arrangements to adjust its quality and distribution to their requirements.

2. The red corpuscles of the blood, which contain the hæmoglobin, together with the cells in the bone-marrow from which they arise, constitute a tissue or organ comparable to the other

organs of the body.   By a tissue or organ I mean a mass of cells
and intercellular substance which has a specific structure and a
specific chemical composition and which, more important still,
resists any qualitative or quantitative variations which are imposed
upon it and tends to return to the normal state.   Owing to the
facility with which it may be examined without serious disturbance
to the live animal, we probably know more about the principles
which govern its behaviour and the mechanisms by which its
quantity and quality are regulated than we do for any other tissue,
and while the mechanisms may be special to the blood the principles
are of wider application.   And in considering what happens under
different conditions it must be remembered that the carrying of red
corpuscles round the body is not the only function for which a
circulating blood is needed : it has to bring other food than oxygen
to the tissues and carry away their waste products for excretion,
and it has also to distribute the internal secretions on which much
of the harmonious working of the body depends.   If therefore,
thinking particularly of hæmoglobin and oxygen supply, it some-
times seems to us that the behaviour of the circulating blood is
not as perfect as it might be, it is as well to see whether these
other considerations may not be having their influence before we
impugn the good sense of what is being done.

3. Within the ordinary limits of biological variation, the volume
of the blood, the quantity of hæmoglobin and in consequence the
concentration of hæmoglobin are constant in any one species of
mammal under ordinary conditions of life : the variation is about
$\pm$ 10 per cent. and in most mammals the blood forms about 5 per
cent. of the body weight and contains about as much hæmoglobin
per kilo body weight as will carry 7 to 10 c.c. of oxygen, i.e. about
as much oxygen as will with moderate activity be used in a minute.
Under unusual conditions, after experimental procedures and in
disease, the total quantity and concentration of hæmoglobin may
be varied a good deal either upwards or downwards for long periods.
The volume of the blood is, however, much more constant.   It is
impossible to alter it either way experimentally for more than a
few hours : in disease the body cannot survive a diminution which
is more than temporary and only in rare conditions is the volume
persistently too large.   This is due to the necessity of the blood
fitting snugly into the vascular system.   The body evidently
objects to too much blood, for, if it can, it always brings the volume
back to normal after it has been increased though the few examples
which are known of continuous excess show that it is possible for
the circulation to go on adequately under these circumstances.

Too small a volume is, however, impossible : the heart cannot fill properly and the circulation soon fails altogether. This is the immediate danger of a considerable hæmorrhage rather than the loss of hæmoglobin, and animals are facile to meet it by taking in water and salts from the tissues into the blood. This process begins before the hæmorrhage has ended and is naturally more effective if there is plenty of water available in the tissue spaces. If it fails, the animal dies. If it succeeds, the volume is restored, the salts of the plasma are adjusted by exchange with the tissues and the characteristic proteins soon replaced.

4. But except for a small quantity kept in the spleen, the body has no store of ready-made red corpuscles on which it can draw. Hence when the volume of the blood comes back to normal after a hæmorrhage, the quantity and concentration of red cells and hæmoglobin are diminished and the animal is anæmic in proportion to the amount of blood lost. The bone-marrow, however, soon responds to the deficiency and makes the necessary number of red cells in a time which varies with the degree of defect and other circumstances, but which is in any case not very long. The actual intermediary stimulus is without much doubt the imperfect supply of oxygen to the tissues, bone-marrow included : a similar growth can be induced by breathing air with less than the normal pressure of oxygen and contrariwise an atmosphere enriched with oxygen can produce a condition of anæmia. A large loss of blood gives a large stimulus but the resultant regeneration is not necessarily quicker than after a smaller hæmorrhage because a limit is set by the amount of active marrow present, so that generally speaking the time taken to replace the loss is proportionate to the amount lost. As the normal level is reached, the stimulus dies away and growth stops when the natural quantity and concentration of hæmoglobin have been regained.

5. The red corpuscles are indeed among the tissues which have a high capacity for regeneration, and it is natural to associate this with the fact that they are continually being destroyed and replaced in normal life. Their destruction is vouched for by the continual production of bile pigment in the body and its excretion by the liver, their regeneration by the maintenance of the normal level of hæmoglobin and by finding in the circulation young red cells (" reticulocytes ") just passed out from the marrow which develop into the mature form after about 24 hours in the blood. Exactly how many red cells are thus destroyed every day has at present defied exact measurement and the proportion of the whole apparently differs a good deal in different animals, but in rabbits

it is likely that it is about 1 or 2 per cent. of those in circulation. Nor is the reason for it quite clear. It may be that the cells become senile and are worn out, it may be that they are injured in the rough and tumble of the circulating blood, it may be that bile pigment is necessary to play some part in the economy. Be that as it may, the result is that the bone-marrow is always kept more or less in training and in a position to respond to the call of an unpremeditated loss of blood. Other tissues which are continually kept active in ordinary life have a similar capacity, which probably reaches its highest level in leucocytes which are produced in great numbers every day and correspondingly destroyed in combating injuries and infections of the respiratory tract and alimentary canal : those lost in a large hæmorrhage are replaced in a few hours and any call for them in a local inflammation is met at once. The skin too is constantly being worn off the surface of the body and replaced ; hence its familiar ability to grow quickly and repair surface wounds.

6. The habit which the marrow has of extruding immature red corpuscles into the circulation is a curious one and also very convenient, for it enables us by quite simple methods to get a good idea of marrow activity. Whether all red cells are in this state when they first come into the circulation is a little uncertain, but it seems likely that, when blood production is going on quietly, a good number are fully mature when they are put into use. Directly, however, the marrow becomes more active these reticulocytes became more abundant and with great activity very abundant. It looks as if the marrow knew that any hæmoglobin in the blood would be useful so long as it was contained in a cell which could hold together in the circulation, and the more urgent the demand the more immature are the cells which are put out : even the nucleated precursors of the red cells are sometimes used in which hæmoglobin is present only in small amount. The blood of embryos and of new-born young, in which red cells are being made very fast, have the same character and all the circulating red cells may be in various stages of immaturity. Quantitatively it may be shown that the amount of hæmoglobin in each red cell is less than normal and by Price-Jones' statistical measurements that the red cells are smaller than usual and more variable in size. These immature cells also differ from normal red cells in consuming a certain amount of oxygen themselves, which to some extent undoes the advantage which mammals gained over birds by inventing non-nucleated corpuscles which could carry oxygen without using any of it *en route*. The marrow in short is acting in a hurry to meet an emergency which

in this particular instance seems a good thing to do : the body needs oxygen and any cell with hæmoglobin is better than none.

7. The influence of experience and practice is also illustrated by the regeneration of repeated hæmorrhages. A rabbit after the loss of about one-third of its blood restores its hæmoglobin in about three weeks ; if it is then subjected to a second similar bleeding it regains its normal level in a week or ten days, and frequent bleedings can exalt the animal's ability to make fresh red cells to a remarkable degree so long as the body can supply or take in the necessary quantities of iron and other materials. Concurrently with this improvement, the bulk of active marrow increases until it may occupy the whole of the available space within the bones, most of which in a normal animal is filled with fat. It is evident therefore that if a loss of blood or any unusual demand for red corpuscles could be foreseen, it would be rational practice to take a course of bleedings and so hypertrophy the marrow. Hæmorrhages do not, however, come by the clock and the only obvious practical application of the principle seems to be in those who contemplate high mountain climbing. Here, too, it is very doubtful whether the acclimatisation which recent expeditions in the Himalaya have shown to be necessary for successful work at over about 20,000 feet is due in any considerable degree to an increase in the circulating hæmoglobin : preparatory bleedings were, I believe, tried by one of the Austrian expeditions, but without any clear benefit. The method would, however, surely shorten the period of disability experienced by those who go to heights of about 15,000 feet and stop there for a long time, conditions under which an increase of hæmoglobin seems to be requisite for comfort and activity.

8. Another consideration which has a great effect on the rate of regeneration is the species of animal concerned. The loss of 30 or 40 per cent. of its hæmoglobin is replaced by a rat in a week, by a rabbit in 3 weeks, and, from the much less complete data available, we may surmise that a man takes 2 or 3 months. The capacity to produce fresh cells, the rate of metabolism and the duration of life all vary with size or perhaps more accurately the ratio between bulk and surface. The rate of living, measured by the oxygen consumed and the CO_2 produced per unit of weight, varies inversely with the size, and the duration of life inversely with the rate of living. A small rat, therefore, lives much faster than a large man and lasts at the most 3 years instead of 60. Correspondingly his normal turnover of red cells is relatively larger, he has more active marrow and more immature red cells in his

blood, and his ability to generate blood after a hæmorrhage or an exposure to a low oxygen pressure is substantially greater. So too young growing animals live faster than their adults and regenerate lost red cells quicker. It is another illustration of the results of practice. It is as well that small animals have this better equipment to meet one of the accidents of their natural life in which blood is apt to be lost by external injuries, scratches, the bites of other animals and so on. The size of a wound so inflicted is not proportioned to the size of the animal which receives it, but to that of the agent which causes it. Other things being equal, the same sized wound will let out the same amount of blood from a small as from a large animal, but the importance of the loss is quite different in the two cases. One c.c. is about all the blood a small mouse has, 2 c.c. is a serious loss for a moderate rat, 5 c.c. would be fatal. But these amounts are negligible for a rabbit or any larger animal, just as a loss of 50 c.c. would kill a rabbit and not be noticed by a man. While therefore a small animal runs a larger risk of immediate death from hæmorrhage than a large one, he is in a much better position to replace any blood which has escaped. In fact, he has other defences, for the blood of rats and mice clots quicker in a wound than that of, for example, man, so that with equal injury the small beasts actually lose less blood.

9. If, on the other hand, we try to increase the circulating hæmoglobin by injecting the blood of one rabbit into another, we find that within 24 hours the volume of the blood has returned to normal, that the injected plasma, together with an amount of the animal's own plasma equivalent to the volume of the injected red corpuscles, has been turned out of the circulation into the tissue spaces, that with the plasma have gone its characteristic proteins to which the capillary wall is normally impermeable and that the blood is abnormal only in its greater content of hæmoglobin : the injected red corpuscles cannot readily escape from the vessels and hence the total quantity and concentration of hæmoglobin are increased in proportion to the amount injected. Subsequently the hæmoglobin falls slowly and after a few days rather faster, so that if the amount injected is equal to about 40 per cent. of the quantity already present in the recipient the normal level is reached in about a fortnight. It was at one time supposed that this disappearance was due to the natural wearing out of the red cells and that such experiments threw light on their normal length of life. We now know, however, that this is not the explanation, for repeated transfusions show clearly that an active process of destruction is involved. If after the first injection has gone the same animal is transfused

again in a similar way, the extra hæmoglobin disappears in a few days, and after several such injections it is difficult to keep the hæmoglobin above the normal level for more than about 24 hours, be the injections as large as they can be. It is the converse of the experiments on repeated hæmorrhages : practice or training
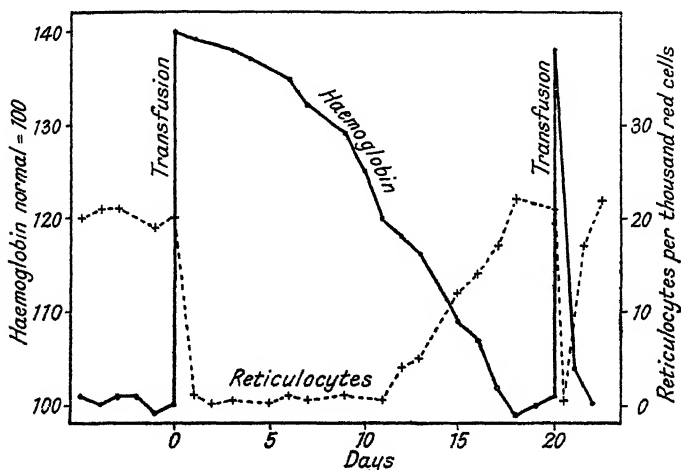
teaches the animal in the one instance to make blood faster and in the other to destroy it more quickly. It seems indeed to have as much objection to having too much blood as to having too little, which at first knowing seems rather strange to our traditional belief that it is always a good thing to have plenty of good red blood.

10. That practice should lead to this kind of result is of course really nothing but common sense : the body will always do a thing better if it has done it before. Thus the first dose of the toxin of a bacillus may lead to very little in the way of the production of antitoxin, the second dose leads to a larger response and so on, though not *ad infinitum* : the capacity of the animal in the end puts a limit to the improvement which can be effected by training. Or, if bacteria are injected into the circulation these are fairly soon removed by sticking to the walls of the capillaries and by phagocytosis : a second similar dose given to the same animal disappears a good deal faster. The two instances from the blood which we are particularly considering are not altogether in parallel. Animals may be presumed to have had some experience of dealing with hæmorrhage in their ancestral or individual lives, and natural selection would presumably tend to exalt any quality with such an obvious survival value : being bled by the experi-

menter is not therefore an entirely fresh incident for them. But we know of no circumstance of natural life which leads to an animal having too much or too concentrated blood and transfusion must be something quite outside its experience. Yet it learns at least equally quickly to deal with the abnormal condition. That it can in fact bring itself back to normal after a large transfusion more quickly than after an equivalent hæmorrhage is due to the commonplace fact that destruction is easier than growth.

11. The mechanism by which blood is regenerated after a hæmorrhage is qualitatively the same as that by which the small normal daily losses of red cells are replaced. Nor is any fresh mechanism brought into play to get rid of large amounts of red cells in a day or two beyond what is always in operation on a small scale. If a rabbit is injected with the blood, not of another rabbit but of another species of animal, its serum acquires the property of dissolving the foreign red cells, and if these are injected into the blood of a trained rabbit they break up in the circulating blood. And the serum of the trained rabbit will do the same if it is mixed with the foreign red corpuscles in a test tube. Nothing of this kind happens if the rabbit is injected with rabbit blood, and the excess cells after transfusion are taken up by phagocytic cells, mostly in the lymphatic glands, where they are disintegrated. The only constituent which can be traced is the iron : this is partly excreted by the kidneys and large intestine and partly stored in the spleen and to some extent in the liver, whence it can be retrieved if necessary (*e.g.* after a hæmorrhage) and used to make fresh hæmoglobin.

12. What is the stimulus which rouses this mechanism of destruction to action ? At first sight it might seem to be nothing more than an example of the response which the body nearly always makes to a foreign protein, demonstrated most clearly in the production of antibodies which react with the antigen coming from outside. It is true that in the great majority of instances a natural protein is treated by an animal as foreign and an antigen only if it comes from another species. Ordinarily rabbit proteins are not antigenic to rabbits which do not produce antibodies which react with them. But instances to the contrary are known. Thus a rabbit may produce antibodies to the proteins of the lens of its own eye which lives its life secluded from the free play of the general circulating fluids of the body. Cattle and hens are also known in some cases to respond to cattle and hen proteins respectively, and a few other examples of intraspecific reactions have been found. It is, however, on general grounds improbable that the transfusion

reaction is of this type and such experiments as can be made to test the point seem to exclude the possibility of this explanation, at any rate in any simple form. If the fact that the injected blood comes from another individual is important, it is likely the reaction would be more marked if the blood were taken from an animal of a race different from that of the recipient. Such, however, is not the case. Using more or less pure-bred strains of rabbits widely different in their general make-up and constitution (e.g. big lop-eared brown belgians and the small white and black himalayan strain) we find that after transfusion strain A disposes of A blood just as quickly as it gets rid of B blood, and vice versa : to the recipient rabbit the two sorts of blood appear to be indistinguishable. More conclusively, take a rabbit and train it well with half a dozen transfusions : when it has learned to destroy blood really quickly bleed it well (say 40 c.c. out of a total of about 100 c.c.) and immediately transfuse it with the same amount of blood from another rabbit. An animal without any previous experience of considerable hæmorrhage would take at least 3 weeks to regenerate the amount of blood lost : a well-trained rabbit would destroy the amount injected in 2 days or less, and if it behaved according to this plan would become distinctly short of red corpuscles and be substantially anæmic for a fortnight or so. In fact, it does not destroy the transfused blood and the total quantity and concentration of hæmoglobin continue at the normal level. Evidently, therefore, the fact that the added blood comes from another individual is not under all circumstances an effective stimulus for its destruction. If the rabbit has no particular use for the blood, it destroys it : if it needs it, it keeps it : the response is conditioned by the requirements of the body as a whole to have a good supply of oxygen.

These results are analogous with Rous' great experiments on the liver. If the right lobe of the liver is removed, the left quickly hypertrophies and the animal soon has the normal amount of liver substance. The right lobe can also be got rid of in a more leisurely way by tying the branch of the portal vein which brings blood to it from the viscera : the cells do not die suddenly, for they still receive blood *via* the hepatic artery, but their normal functions being obstructed they waste away and as the right lobe atrophies so the left lobe hypertrophies, the animal having always something near the normal amount of liver and ending up with it all in the left lobe. But if the bile duct coming from the left lobe is tied, the corresponding liver cells are poisoned by the accumulation of bile and their capacity for growth is much deteriorated. And if then the portal vein to the right lobe is tied, the right lobe does

not atrophy because (there is really no other way of stating the conclusion) the left lobe cannot hypertrophy, and so the animal would be left with too little liver.

13. The viscosity of the blood depends mostly on its content in red cells and even at the body temperature of warm-blooded animals it is high. After transfusion when the added fluid has been got rid of and the volume of the blood brought back to normal, it is increased in proportion to the increase in the concentration of red corpuscles and it is attractively simple to suppose that this increase of viscosity is the stimulus which leads to the destruction of the excess of red cells. More sticky blood is certainly a fact of which an animal should be readily aware for it much increases the work of the heart in sending the blood round the body. As regards the supply of oxygen, it is of course unnecessary to circulate blood with a high concentration of hæmoglobin as quickly as a more dilute suspension, but, as far as the facts are known, other considerations besides oxygen come in and polycythæmic blood appears to be circulated at about the normal rate. Viscosity, therefore, seems a likely kind of stimulus and the idea that it is important is compatible with the bleeding-transfusion experiments already mentioned, when transfused blood did not increase the viscosity and was not destroyed. There is, however, an impressive mass of evidence available showing that increased viscosity is not *per se* an infallible stimulus to the destruction of red cells. The blood of men who live their lives in the high settlements in the Andes at about 14,000 feet contains on the average an amount of hæmoglobin in every 100 c.c. which will carry about 27 c.c. of oxygen from the lungs to the tissues instead of the 19 c.c. which is the normal figure for people living at sea-level : the increased number of red cells—from 5 to about 7 millions per cubic millimetre—makes their blood much more viscous than usual, but there is no indication that any destruction of red cells goes on more than the ordinary small daily quantum. Persons who go and stay in these places for a time increase the quantity and concentration of their hæmoglobin in the same way, and when they return to sea-level and the stimulus of a reduced oxygen pressure is no longer present they soon lose the excess and their blood returns to normal : they are in the position of the transfused rabbit which does not need the extra hæmoglobin and gets rid of it. This polycythæmia of high altitudes is indeed the regular response which the body makes to any defect in the supply of oxygen to the tissues, no matter how it is caused. The air breathed may be defective either in its total pressure or in its percentage of oxygen at normal pressure ; there

may be a difficulty in getting adequate contact between the air breathed and the blood in the lungs as in partial obstruction of the trachea or in a unilateral pneumothorax; there may be a difficulty in the blood getting round the body quickly enough as in partial obstruction of the inferior vena cava; some of the circulating hæmoglobin may be useless for carrying oxygen as in chronic poisoning with carbon monoxide. All these diverse conditions have this in common, that the tissues get less oxygen than they would like to have, and the bone-marrow responds by producing fresh red cells and raising the concentration of hæmoglobin in the circulating blood : the increase does not lead to any destruction of red cells and is maintained as long as the cause of and need for it persist. Even this reaction is not, however, invariable, for it does not occur in the commonest instance of defective oxygenation seen in man, which is due to disease of the heart muscle. A polycythæmia here would no doubt be useful and would help to compensate for the unnatural slowness of the circulation, but the increased viscosity would throw more work on a heart which already has as much as it can do and the body strikes a balance between the advantages and disadvantages of thicker blood by leaving the concentration of hæmoglobin somewhere near the normal level and demanding a quiet life. This method of compensation for a shortage of oxygen also has its limits, for if the concentration of red cells is increased indefinitely the blood would in the end become solid. The tolerable limit of viscosity which is compatible with the maintenance of the circulation seems to be reached when the concentration of red cells is increased to about 150 per cent. of the normal or a little more, and this presumably fixes the greatest height at which it is possible for man to live permanently : curiously enough this seems to correspond with the height at which the necessities of food and shelter begin to make life impracticable.

14. It seems, therefore, that the concentration of hæmoglobin in the blood is fixed by the needs of the animal for oxygen in such circumstances as happen to prevail. What we call the " normal " level in man (*i.e.* an oxygen-carrying capacity of about 19 c.c. per 100 c.c. of blood) or in the rabbit (about 13 c.c.) depends on their living somewhere about sea-level in air containing 21 per cent. of oxygen: any decrease in pressure or percentage leads to an increase, and vice versa. The point of equilibrium is fixed as the best compromise between having an easy circulation and a good oxygen supply, and as in other organs this " normal " level provides a good margin for emergencies. With the body at rest, only about a third of the charge of oxygen taken up in the lungs is used by

the tissues and some such reserve capacity is obviously needed to meet the changes and chances of life. We know from diseased anæmic people that no particular symptoms arise with a concentration of hæmoglobin considerably below the normal level so long as they stay quiet, but they cannot take any violent exertion. This might perhaps do in the protected seclusion of civilised life, but it would be a hopeless arrangement under more natural conditions. The curious thing about this reserve power is that it is apparently innate to the organism and not the direct result of individual experience : with a sufficient stimulus a man can run like mad when he has never done so before though we may reasonably surmise that his ancestors have had to do it often enough to save their lives. Natural selection operates through the extreme demands for vitality rather than through the average.

15. The difficulties of a higher viscosity give us a ready explanation of why the body dislikes having too much hæmoglobin, but it is rather doubtful whether this is really the whole and fundamental explanation because other tissues show the same phenomenon. Thus the quantity of liver substance varies with the intake of food with which it has to deal ; there is a wide margin of safety and an animal can get along very well with less than half the usual amount. But if the supply of food is persistently diminished, the liver becomes smaller, and there is evidently some objection to having more of it than corresponds to current needs. So, too, even with bones when it is rather difficult to see why too much is not approved. Their size depends a good deal on the size and activity of the muscles which are attached to them, and if these muscles are paralysed the bones become smaller. They do not as it were just waste away, but are taken to pieces and, with all their elaborate architecture, reconstructed on a smaller scale, which must give the animal more trouble than leaving them alone. The truth of the matter is probably best appreciated by considering the familiar facts of the waxing and waning of voluntary muscle where a certain amount of activity is plainly necessary to keep a given bulk of tissue in existence. The effects of exercise in this instance are so well known that it is rather depressing to have to say that the detailed explanation of them is very far from clear. The general fact is that quantity of tissue is related to quantity of function and the ups and downs of the hæmoglobin in the blood seem to be an example.

16. The readiness with which the bone-marrow responds to a loss of red cells is, as we have seen, to be connected with its continuous activity on a small scale in daily life. The necessity for

this daily regeneration might presumably be abolished if the daily loss were provided for by putting extra red cells into the circulating blood. This can be done either by transfusing large quantities of blood every few days or by daily injections approximately equal to the normal daily loss (1 to 5 c.c. in an ordinary rabbit). Fortunately the activity of the marrow in experiments of this type can be ascertained at frequent intervals by counting the immature red cells in a drop of blood. After a large transfusion the number of reticulocytes is reduced within 24 hours, e.g. from 2 per cent. of the red cells to less than 0·1 per cent. : the marrow avails itself at once of the chance of doing less work. And the number remains low as long as the excess of hæmoglobin persists, returning to the original level as the surplus is destroyed. If transfusions are continued, the reticulocytes can be kept at a low level for a long time and the efficiency of the marrow be so reduced that it may have some difficulty in coping promptly with a large bleeding. But they can never be got rid of altogether and they return to their normal level as soon as the extra hæmoglobin is disposed of. The marrow still insists on keeping up a small amount of growth though it is, to the experimenter's mind, relieved of all necessity for it. The matter no doubt looks different from the rabbit's point of view : his thoughts are fixed on keeping his body nicely adjusted to its surroundings and as ready as may be to cope with anything which may happen. One wonders whether it would be possible to abolish any cells by doing their work for them. Probably not, and it is legitimate to guess that the reason is that the quantity of tissue is always related to the reserve capacity which tissues have as well as to their normal quantity of function.

COHESIVE FORCES IN METALS

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In the last few years great advances have been made from the theoretical side in our understanding of the metallic state. Outside a limited circle very little is known about these advances; in the literature references are frequently made to our complete ignorance of the nature of the metallic bond, or of the rôle of the conduction electrons. Nevertheless, the theoretical description which can now be given of monovalent metals such as silver or the alkalis is almost as complete as that which Born and his co-workers have developed for simple polar crystals such as rock salt; and for some of the more complicated metals and alloys a qualitative discussion is possible which enables many of their properties to be understood.

The advances to which we refer have been achieved by applying quantum mechanics to the behaviour of the free electrons in metals. Historically the first applications of the new theories were to the problems presented by the heat capacity and magnetic properties of these electrons. The work of Sommerfeld [1], Pauli [2] and Heisenberg [3] in this field is well known. In the field of electrical conductivity the theory has been successful in accounting for the order of magnitude of the resistance of normal metals and for the high resistance and abnormal behaviour of certain alloys; the major phenomenon of supraconductivity, however, still remains unexplained in principle.

In this article we shall confine ourselves to the more chemical problems presented by the nature of the metallic bond, the crystal structure of metals and alloys and the theoretical calculation of the cohesive forces. These problems are of great interest in themselves, and, moreover, it is likely that their complete solution will be of assistance in the attack on the problems of the strength of metal crystals, recently described in this journal by Professor Andrade.

A theory of the metallic bond must make possible the calcula-

tion of quantities which can be compared with experiment. Of these one of the most important is the energy of the metal. From this can be deduced at once the heat of sublimation; if the energy can be obtained for different values of the interatomic distance the compressibility may be deduced, and also the specific volume under zero pressure. If the energy can be calculated for a series of different crystal structures, the structure for which the energy is lowest should be the one which the metal is actually found to have. We shall therefore begin by showing how the energy has been calculated.

A monovalent metal, for instance sodium, may be thought of as an array of positive ions (Na^+) and an equal number of free electrons which are moving about in the lattice in some way which we must investigate. At the absolute zero of temperature the ions may be considered to be at rest [1] in their positions of equilibrium, and arranged therefore on a body-centred cubic lattice. In the alkali metals we may assume that the ions are not in contact with one another, so that the electrons in the ions, which form closed shells as in a rare gas, do not influence one another. The energy of the crystal will then be made up of the following terms:

(1) The mutual electrostatic potential energy of the positive ions.

(2) The electrostatic energy of the free electrons in the field of the ions.

(3) The mutual electrostatic potential energy of the electrons with one another.

(4) The kinetic energy of the electrons.

The assumption that, even at the absolute zero of temperature, electrons have kinetic energy is of course a familiar one even in the earliest forms of the quantum theory, as for instance in Bohr's theory of the hydrogen atom, in which the orbital electrons were assumed to be in continual motion. The kinetic energy of the conduction electrons in metals plays, as we shall see, an important part in metallic cohesion.

First, however, we must consider the potential energy. Obviously it will not be possible to calculate the interaction energy of the electrons and the ions unless we have some information about where the electrons are. But the methods of quantum mechanics never lead to precise information about the positions of the electrons either in the atoms of a gas or in solids. They only give the *probability* of an electron being at any given place. This,

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[1] For our purpose the small " zero-point energy " may be neglected.

however, is sufficient for the calculation of the energy of the crystal. If we denote by $P(r)\ 4\pi r^2 dr$ the probability that between the distances r and $r + dr$ from a given ion a free electron will be found, the energy of the electrons in the field of that ion may be taken to be

$$- \int_0^\infty \frac{e^2}{r}\ P(r)\ 4\pi r^2 dr \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The potential energy term $- e^2/r$ is thus *averaged* over all possible positions of the electrons. It should be noted that the integration in (1) extends over the whole metal, and gives the energy of *all* the free electrons in the field of the given ion.

The quantity $eP(r)$ thus gives the mean charge density in the metal at any distance r from a given ion, excluding the charge contributed by the electrons in the closed shells of the ions themselves. The calculations of the mean charge density in atoms by Hartree [4] and his co-workers are well known. The methods of obtaining the charge density in a metal are very similar.
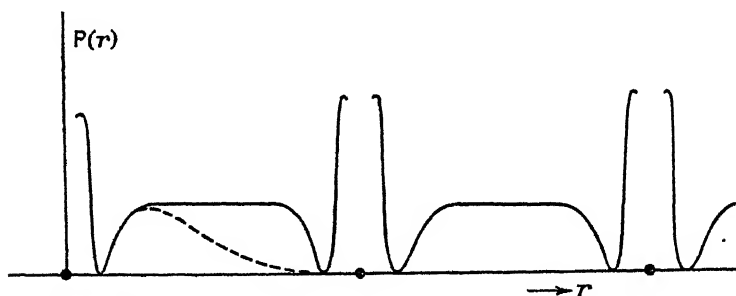
Fig. 1.—Charge density in metallic sodium (excluding the charge on the ions). The dotted line shows the density in a free atom.

Fig. 1 shows the calculated [5] charge density in metallic sodium, plotted along a line passing through adjacent ions. It will be seen that, except in the neighbourhood of each ion, the charge density is almost constant. We thus have for a monovalent metal the very simple model of an array of positive ions embedded in a nearly uniform distribution of negative electricity.

We have now to estimate the interaction energy of the electrons with one another. For this purpose we must know something about the *relative* positions of the electrons. The work of Wigner [6] has shown that the electrons tend to keep away from each other. Again, we are only allowed to speak in terms of probabilities, but in these terms we may say that as each electron moves through

the lattice, it carries with it a sphere in which any other electron is unlikely to be.

At first sight it seems as if it would be a very complicated problem to sum all these potential energy terms ; it is, however, as Wigner and Seitz [7] have shown, extremely simple. If one takes, for instance, the body-centred cubic lattice, which the alkalis have, and if one draws planes bisecting the lines joining each atom to its nearest and next nearest neighbours, one divides up the lattice into a series of polyhedra, one surrounding each atom. Such a polyhedron is shown in Fig. 2. Now the potential energy of the lattice may obviously be divided up into the potential energy of the charges within any one polyhedron, and the mutual energy of the polyhedra themselves. The first term is easy to calculate.

As we have seen, each electron is surrounded by a sphere in which another electron is unlikely to be, so that it is improbable that two electrons will at a given moment be in the same polyhedron; the energy arising from their interaction is thus small. Thus for the contribution to the potential energy made by the charges within a polyhedron we have only to consider the energy of an electron in the field of the ion ; this is easily evaluated by formula (1).
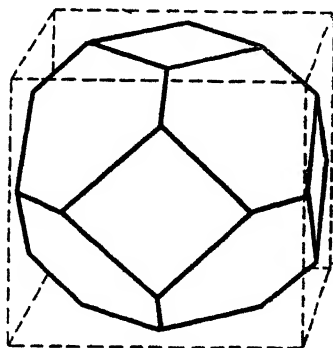
FIG. 2.

The interaction energy between the polyhedra themselves is negligible ; for, according to (1), the energy must be calculated as though the electronic charge were distributed throughout the polyhedra instead of localised in point electrons, and since the polyhedra are electrically neutral and very nearly spherical, the field outside any one of them is small. It would of course vanish exactly if the polyhedron were replaced by a sphere ; actually it gives not more than 1 per cent. of the energy of binding of the metal.

The force which keeps the metal together, therefore, is simply the attraction between each positive ion and the electron which happens to be in its polyhedron ; all the other terms in the potential energy cancel out approximately. At first sight it is a little difficult to understand how these electrically neutral polyhedra stick together ; it must be realised that the electrons, owing to their rapid motion through the metal, will remain roughly uniformly distributed ; so that if the crystal is expanded in any way the electrons, on the average, will move further away from their ions.

We have now to ask why it is that the ions of the metal are kept apart. The answer is that the kinetic energy of the electrons increases as the metal is compressed. Experimental evidence that the conduction electrons in metals have kinetic energy extending over a range much greater than gas-kinetic values is provided by the work of O'Bryan and Skinner [8] on ultra-soft X-ray emission bands ; they find that for lithium K-emission, for instance, a band of breadth 4·2 e.V is obtained instead of a sharp line.

From the theoretical point an electron moving with velocity v is associated with a wave (the de Broglie wave) of wave-length h/mv. Now if the electron is shut up in a block of metal of length, say, L, the de Broglie wave will be a standing wave and must have one of the wave-lengths 2L, 2L/2, 2L/3 . . . 2L/n . . . and so on. The corresponding velocities are $nh/2mL$ and the energies

$$n^2h^2/8mL^2 \qquad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (2)$$

Now according to the Exclusion Principle, due originally to Pauli, not more than two electrons can ever be in the same quantum state. The principle, as is well known, has proved of the greatest value in explaining the periodic table and the X-ray levels of atoms. In our case it means that in the whole of our block of metal of side L only two electrons can have energy $1^2h^2/8mL^2$, two more the energy $2^2h^2/8mL^2$, and so on. The kinetic energy of each electron, and hence their total kinetic energy, are inversely proportional to L^2, and hence vary as $V^{-\frac{2}{3}}$, where V is the volume of the metal.

To obtain an exact formula it is of course necessary to take account of the fact that in a metal an electron can move in any direction. We consider for simplicity N electrons moving in a cubic box of side L ; the wave representing an electron is then taken to be of the form

$$\sin \frac{\pi}{L}(n_1x + n_2y + n_3x) \cos 2\pi\nu t$$

and the corresponding energy, similar to (2), is

$$E = \frac{h^2}{8mL^2}(n_1{}^2 + n_2{}^2 + n_3{}^2)$$

where n_1, n_2, n_3 are integers. If now we take Cartesian axes, measuring n_1 along the x axis, n_2 along the y axis, and so on, then clearly the number of quantum states with energies less than E is one-eighth part of the volume of a sphere of radius $\sqrt{8mL^2E/h^2}$, which is equal to

$$\frac{\pi}{6}\left(\frac{8mL^2E}{h^2}\right)^{\frac{3}{2}} .$$

Since *two* electrons can be in any state, the maximum energy $E_{max.}$ that any electron will have is to be found by equating this to $\frac{1}{2}N$, *viz.*

$$\tfrac{1}{2}N = \frac{\pi}{6}\left(\frac{8mL^2E_{max.}}{h^2}\right)^{\frac{3}{2}}$$

Putting L^3 equal to the volume V this gives

$$E_{max.} = \frac{h^2}{8m}\left(\frac{3N}{\pi V}\right)^{\frac{2}{3}} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

a formula due originally to Sommerfeld [1]. The corresponding wave-length—the smallest which any electron has—is

$$\lambda_{min.} = 2(\pi V/3N)^{\frac{1}{3}} \quad . \quad . \quad . \quad . \quad . \quad (4)$$

It may easily be shown that the total kinetic energy is $\frac{3}{5}NE_{max.}$. Thus, as before, we see that the kinetic energy of the electrons varies as $V^{-\frac{2}{3}}$.

Formula (3), originally due to Sommerfeld [1], is in good agreement with the breadth of the energy bands found by O'Bryan and Skinner.

This model, in which each electron is treated as moving quite freely through the lattice, is of course an approximation. Actually each electron will suffer frequent collisions with all the other elec-
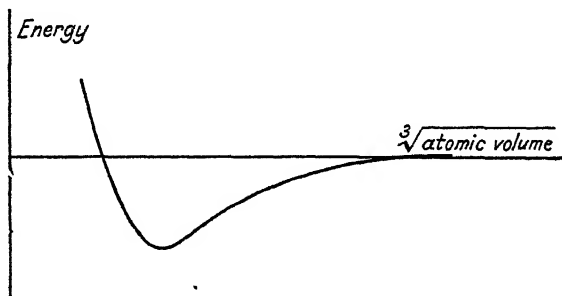
FIG. 3.—Energy of the electrons in metallic sodium as a function of the atomic volume.

trons. Detailed investigations show, however, that such collisions do not have any very large effect on the kinetic energy.

The conduction electrons, therefore, contribute two main terms to the energy of a metal; the potential energy of the electrons in the field of the ions, which, since it will be inversely proportional to the mean distance of the electrons from the ions, we may write $-AV^{-\frac{1}{3}}$; and the kinetic energy, which we may write $BV^{-\frac{2}{3}}$. The sum of these two terms, plotted against V, gives a curve with a minimum, as shown in Fig. 3. The position of this minimum

determines the actual specific volume of the metal, its depth the lattice energy and the curvature the compressibility.

Calculations carried out on these lines for the alkali metals [7, 5] have given very good agreement with experiment. Also, by calculating the change in the electrostatic potential energy when the lattice is distorted, it has been possible to estimate the *elastic constants* for single crystals of the alkali metals [9]. The results obtained are in good agreement with a recent experimental determination [10].

Similar calculations for copper or silver or gold give much too large a value of the compressibility. The reason appears to be that in these metals the ions are relatively larger than in the alkalis; consequently, before the minimum of the energy curve in Fig. 3 is reached, the ions come in contact with one another. It is known from Born's theory of polar crystals such as NaCl that the sodium ion Na^+ and the chlorine ion Cl^- repel one another strongly when they come in contact with another; the copper or silver ions may be expected to behave in a similar manner. Calculations [9] show that these repulsive forces increase very much more sharply than the Coulomb repulsion between the ions.

Metals such as copper or silver may thus be regarded as an array of hard spheres (the ions Cu^+ or Ag^+) embedded in a sea of negative charge. The negative charge, owing to its interaction with the positive ions, tries to contract, forcing the hard spheres together. This is the reason why these metals take up the cubic close-packed structure, which is one of the structures in which the greatest number of spheres can be packed into a given volume. The purely electrostatic energy, on the other hand, may be shown [11] to be lowest in the body-centred structure, which is that of the alkalis.

The electrostatic forces discussed here give energies of the order of the binding energy of metals, about 100 k. cal. per gm. atom. In order to account for the crystal structure of metals and alloys, it is necessary to estimate the very much smaller difference between the energies resulting from the various possible structures. It is often possible to do this even when no calculation of the total energy has been made. As H. Jones [12] has shown, it is possible in this way to account for the well-known Hume-Rothery [13] rule of alloy structures. This rule may be stated as follows: alloys such as Cu-Zn, Cu-Al, Cu-Sn, etc., form a series of different structures (phases) as the concentration of the second-named component is changed. If one ascribes to copper one valence electron, to zinc two, to aluminium three and to tin four, the

boundaries of the phases occur approximately at the same electron-atom ratio for the different alloy systems. For instance, the α-phase, which has the same face-centred structure as copper, becomes unstable when the ratio of electrons to atoms exceeds a value varying between 1·35 and 1·4; the β-phase (body-centred cubic) is stable in a narrow range of compositions about the electron atom ratio $\frac{3}{2}$; and the complicated γ-structure is formed when the electron-atom ratio is $\frac{21}{13}$.

Now we have seen that the electrons in a metal are moving through the lattice with various velocities up to a certain maximum; and that to each velocity corresponds a certain wave-length; the wave-lengths will have a series of values down to a certain minimum, and since, as we have seen, not more than two electrons can correspond to any state of the wave, it follows that, *the more electrons there are in any volume the smaller will be this minimum wave-length.* For the minimum wave-length we have found the formula

$$\lambda_{\text{min.}} = 2\left(\frac{\pi}{3}\right)^{\frac{1}{3}}\left(\frac{V}{N}\right)^{\frac{1}{3}} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where N is the number of electrons per volume V.

If therefore we increase the concentration of, for instance, zinc in the copper-zinc alloys, we increase the number of electrons and hence decrease the minimum wave-length. Eventually we come to a point when the fastest electrons have wave-length short enough to suffer Bragg reflection for normal incident on certain planes of the lattice. Now this is a state of affairs which the crystal will avoid by changing its structure if it can for the following reason:

The simple relation between energy and wave-length

$$E = \tfrac{1}{2}h^2/m\lambda^2$$

which may be deduced for free electrons from the two equations

$$E = \tfrac{1}{2}mv^2 \qquad \lambda = h/mv$$

is no longer true. Calculation shows that if one plots the energy E of an electron moving normal to a certain set of planes in the lattice against the reciprocal of the wave-length, one obtains a curve such as that shown in Fig. 4. λ_c is here the critical wave-length for Bragg reflection and at this wave-length there is a *discontinuity* in the energy. The width ΔE of this discontinuity may be shown to be roughly proportional to the intensity with which X-rays are reflected from the set of planes in question. It is impossible for an electron to have energies lying in the range ΔE; electrons with these energies which attempt to enter the crystal from outside would suffer total reflection. The existence of finite ranges of

energy for which total reflection from the lattice takes place is shown directly by the experiments of Davisson and Germer on the reflection of cathode rays from nickel crystals, and has been interpreted by Bethe [14] in terms of this theory. These forbidden ranges of energy are analogous to the finite breadths of reflection in Ewald's dynamical theory of X-ray reflection.
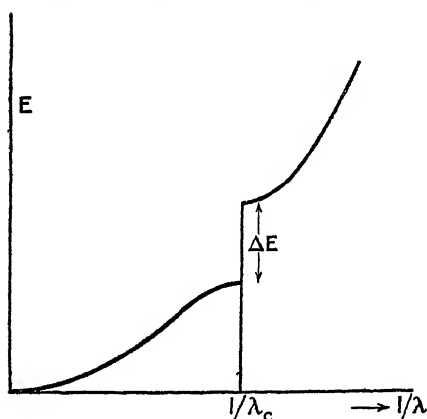
FIG. 4—Energy of the electrons in a metal as a function of the wave-length.

It follows from the results illustrated in Fig. 4 that electrons which move just not fast enough for reflection have their energies depressed, while those which have just too large a velocity have their energies raised. Thus a structure in which none of the electrons moves fast enough for reflection will have low energy and tend to be stable, while if the number of electrons is increased so that the wave-lengths of some of the electrons become less than λ_c the energy rises rapidly, and the structure tends to be unstable if another structure exists for which this is not the case.

Consider now the close-packed cubic structure, reflection takes place first from the (111) planes, which have the biggest spacing in the lattice. The corresponding wave-length is

$$\lambda_c = \frac{2}{\sqrt{3}}a$$

where a is the lattice constant. Since there are four atoms to the unit cell, this gives

$$\lambda_c = \frac{2}{\sqrt{3}}\left(\frac{4V}{N_A}\right)^{\frac{1}{3}}$$

where N_A is the number of atoms per volume V. Comparing this with (5), we see that reflection will first take place when the number of electrons is such that

$$2\left(\frac{\pi}{3}\right)^{\frac{1}{3}}\left(\frac{V}{N}\right)^{\frac{1}{3}} = \frac{2}{\sqrt{3}}\left(\frac{4V}{N_A}\right)^{\frac{1}{3}}$$

which gives

$$\frac{N}{N_A} = \frac{\pi\sqrt{3}}{4} = 1\cdot362$$

This is just about the electron-atom ratio at which the α-phase does in fact become unstable. The precise values are shown below.

In the β-phase (body-centred cubic) reflection takes place first from the (110) planes. We thus obtain

$$\lambda_c = \frac{2}{\sqrt{2}}a.$$

Since there are two atoms in the unit cell, this gives

$$\frac{N}{N_A} = \frac{\pi\sqrt{2}}{3} = 1 \cdot 480$$

We see therefore that the body-centred cubic structure does not become unstable so soon as the face-centred structure as the number of electrons is increased; and that the former phase will, in fact, become unstable at about the observed electron-atom ratio of $\frac{3}{2}$. Table I gives the observed boundaries of the phases.

TABLE I

Alloy.	Electron-atom Ratio of Maximum Solubility in α-phase.	Alloy.	Electron-atom Ratio of the β-phase boundary with Smallest Electron Concentration.
Cu–Zn	1·384	Cu–Zn	1·48
Cu–Al	1·408	Cu–Sn	1·49
Cu–Ga	1·406	Cu–Al	1·48
Cu–Si	1·420	Au–Zn	1·48
Cu–Ge	1·360	Au–Cd	1·49
Cu–Sn	1·270	Au–Al	1·370
Ag–Cd	1·425	Ag–Cd	1·50
Ag–Zn	1·378	Cu–Si	1·49
Ag–Hg	1·35	N/N_A (theoretical). .	1·480
Ag–In	1·40		
Ag–Al	1·408		
Ag–Ga	1·380		
Ag–Sn	1·366		
N/N_A (theoretical). .	1·362		

In an exactly similar way Jones has been able to account for the occurrence of the γ-structure, though here it is not the first set of planes which come into play but those for which the intensity of X-ray reflection is greatest.

The two examples which we have given will serve to show the kind of advance which the new quantum mechanics has made possible in the electron theory of metals. At present, calculations of the cohesive forces have only been made for copper, silver and the alkali metals; the theory has yet to be extended to metals like zinc or aluminium which contribute more than one electron per atom, and to the more complicated transition metals such as nickel. The theoretical discussion of the phase diagrams of alloy systems which we have given is moreover of a very qualitative

nature ; the precise energy differences between the different phases has not been calculated ; and there are many types of phase diagram which have not been treated at all. Nevertheless, it seems likely that investigations of the kind described here will in the course of time lead to an understanding of the nature of the metallic bond in the more important metals and alloys.

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PETROLOGY AND MODERN ROAD PROBLEMS

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THE very marked increase in the volume of road traffic in the last few years has focussed the attention of scientific workers on road problems, both in the matter of planning and lay-out, and in that of road construction. The road user is not usually much troubled about the foundation of the road, but he is tremendously interested in questions of road surface inasmuch as these are bound up with slipperiness, visibility at night and cost of maintenance, the frequency with which parts of a road are closed for repairs being an ever-recurring cause for complaint. Road control is slowly passing from the hands of a large number of small and poorly staffed authorities into the hands of one central authority, as shown by the taking over of the work of the Rural Councils in this matter by the County Councils in the early part of this century, and the further step forward during this year, when the Ministry of Transport is to take control of the major through routes from the County Councils. This centralisation of control will, it is to be hoped, lead to more intelligent planning of our road communications as a whole and to a more uniform and higher standard as regards the surfacing and maintenance of trunk roads.

In roadmaking, stone is used for four definite and distinct purposes, apart from the laying of the foundations, *i.e.* the bottoming of the road. These are (*a*) Stone Setts or stone block paving, (*b*) Macadam, (*c*) Chippings and (*d*) Concrete. Each usage demands that the material used shall have certain properties which fit it for that particular purpose and shall not be possessed of properties which will prove deleterious when the stone is actually in the road.

(*a*) *Setts*

These are expensive and only used where traffic is very heavy, both as to number of vehicles per day, and as to the type of vehicles.

For this reason, setts must be able to withstand high loads and heavy impacts without fracturing. At the same time they must be as non-slippery as possible and preferably of light colour so as to facilitate visibility at night. When setts are made of suitable stone they form the most durable of all known roadmaking materials, unless we except the iron roads which are as yet still in the experimental stage. The amount of maintenance on a good sett-paved road is practically nil, as evidenced by the setts at Oxford Circus, which have now been in position for twenty-five years, without renewal, and are still in good condition.

(b) *Macadam*

Macadam roads form by far the largest part of our national road system, either in the form of country roads which may be of water-bound macadam, or in the more important roads connecting towns, which are often surfaced with macadam with a binder of tar or bitumen. Macadam consists of stone of from say $1\frac{1}{2}$ to 3 inch gauge which when rolled into place on the road interlocks to form a more or less rigid substratum to the topmost surface. The stones are usually covered nowadays with tar or bitumen, which is referred to as the binder. The function of the binder is not only to make the interlocking more secure, but also to increase further the water-proofing of the surface so that rain drains from the road instead of soaking through it. In addition it reduces the amount of dust formed by attrition of the aggregate or macadam. It was, in fact, the use of tar as a dust layer that originally led to its use in our modern roads.

(c) *Chippings*

In modern practice many forms of road are surfaced with a thin carpet of chippings, usually of $\frac{3}{4}$ inch gauge or smaller. As in the case of macadam the chippings are held in place with tar or bitumen as binder. Thus chippings come directly into contact with the traffic and so must be of very high-grade material to have a long life. An inferior stone will disintegrate under heavy traffic in a matter of weeks or even days, and the expense involved is great if the work has to be done again, to say nothing of the inefficiency which results from continual interference with the flow of traffic caused by constant road repairs. For this reason, high impact values are required of stone used for chippings. It is known that as the ratio of surface area of stone to its total weight increases, the resistance to impact requires still higher values. For this reason a stone which is quite suited for use as a sett is often use-

less in the form of chippings, even in cases where the traffic is lighter.

(d) Concrete

Concrete is coming more and more into prominence as a material for road building, especially in the case of estate roads. The actual impact value of the stone is not so important here as in the three previous cases referred to, as the traffic is supported by a mixture of cement and stone, and the cement part of the concrete is not likely to be more resistant to traffic than any material which is used as the aggregate for the concrete. The factors to be considered are rather those which will be likely to affect the life of the concrete as such, and these are found to be such things as porosity of the stone, and in the case of limestones, the clay content, since both affect the behaviour of the stone under the action of weather, and in particular of frost.

Physical tests of various kinds have been devised and are widely used in assessing the value of a particular stone for the various uses outlined above. These tests are designed to measure the resistance of a stone to crushing, fracture by impact, breakdown by attrition due to traffic and weather, abrasion, and furthermore to measure the porosity of a stone and the power of binders, *i.e.* tar and bitumen, to adhere to the stone. Many of these tests have been standardised, but even so the results of such tests may be most misleading. It is difficult, moreover, to analyse the various forces at work in the disintegration of a road surface sufficiently accurately to reproduce road conditions in the laboratory. As an example of this may be quoted the persistence with which many firms marketing roadstone refer to the crushing strength of their stone, even though experience shows that this value is no true indication of the value of a stone for roadmaking, since many stones which give a relatively high crushing strength have been found in practice to be unsuitable for roadmaking. Furthermore, the crushing strength of a stone varies considerably according to the direction in which the crushing load is applied with respect to any bedding, schistosity or other structural weakness that the stone may possess.

Of the tests mentioned above, the one which measures resistance to fracturing by impact is probably the most useful in assessing the wearing properties of the stone, since under modern conditions of rubber-tyred traffic, attrition and abrasion are relatively small, and the forces at work on the setts, macadam, etc., are probably more in the nature of blows than of mere crushing stresses. We

are here, however, faced with the difficulty that the actual value of the impacts which have to be withstood are not known, nor are they easy of discovery, and impact values demanded of a material are rather in the nature of arbitrary values based on the general experience of the road engineer, than anything scientifically related to the forces which have to be withstood by the stone.

The writer became convinced many years ago that the behaviour of a stone in the road must be a reflection of its petrological character, and that if the relation of these two to each other could be worked out, then it would be possible to replace our somewhat cumbersome and arbitrary physical tests by the more readily carried out method of examining the material by hand lens and the microscope. This line of work has been pursued constantly from that time, and has proceeded on what may be termed a "finger-print" system. In collecting data, rocks were chosen whose behaviour under traffic was known as the result of their use in roads of various kinds in different parts of the country and under varying conditions of weather and volume of traffic. These rocks, both successes and failures, were examined on the road, in the hand specimen, in micro-section, and whenever possible in the quarry from which they were obtained. As a result of this collection of data, certain well-defined principles emerged, which could then be used for forecasting the behaviour of a stone whose performance was not yet known. A short review of the conclusions reached with an outline of their application will now be given.

Petrological Characters of Importance for Roadstone

Rocks may be broadly classified as regards their suitability for roadwork according as to whether they are igneous, sedimentary or metamorphic, with one or two notable exceptions the most durable being igneous rocks, and the least satisfactory being metamorphic rocks such as schist and slate. This is naturally by no means a strictly accurate division of roadstones, as there are some igneous rocks which are totally unsuitable for use in road building, while some sedimentary rocks such as limestone and quartzite can be used with considerable satisfaction to the user and the road engineer.

Igneous Rocks

It has been found that different igneous types are definitely suited for some road purposes and not suited to others, while some igneous rocks should never be employed in any circumstances. In general, coarse-grained rocks are not suitable for roadwork,

PLATE I

Fig 1 —Bonawe Quarry Argyllshire
One of the highest quality stones
for the manufacture of setts and
chippings for very heavily trafficked
roads It is a fine grained granite
(or granite porphyry) The figure
shows Kernel decomposition of
the felspar crystals (A) Fissuring
is absent Ordinary light (× 25)
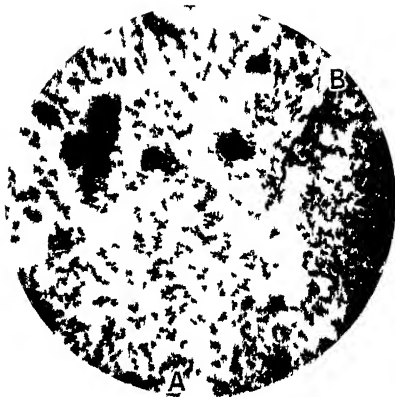
Fig 2 —Connell Dunstaffnage Argyll
shire Olivine Basalt This rock is
very decomposed the dark patches
representing altered olivine and the
shake' of calcite from A to B
bring a plane of weakness The
stone powdered rapidly when used
as chippings Ordinary light
(× 25)
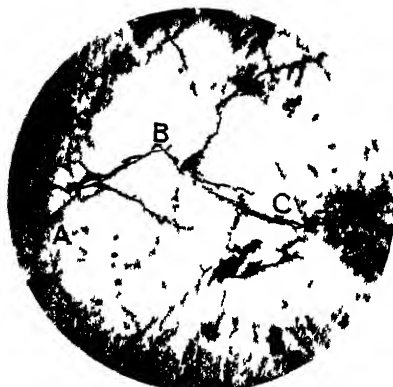
Fig 3 —Mysore, India Granite
Showing high degree of fissuring of
quartz (A B C) which tends to
render the rock brittle Ordinary
light (× 25)

Fig 4 —Groby Quarry Leicestershire
Granophyre Showing central
pheno cryst of twinned orthoclase
felspar (A B) surrounded by perfect
inter growth of clear quartz and
felspar This stone is a good
quality roadstone Crossed nicols
(× 25)

*(The above illustrations are from Road Aggregates their Uses and
Testing,' by B. H Knight)*

whether it be setts, macadam or chippings; further, the smaller the size in which the stone is to be used, the finer grained it should be. Thus while the coarser-grained granites, such as Aberdeen, Creetown and Mountsorrel, are eminently suitable for setts, they do not stand up to wear in macadam sizes as well as some of the finer-grained rocks such as the granite-porphyry of Bonawe, or the quartz-dolerite of the Whin Sill. Chippings demand a particularly high grade of material, and for this purpose such fine-grained stone as the dolerite of Penmaenmawr is suited. It should be noted that as regards wearing properties, a stone which will make good chippings will also yield a good macadam, and in general will also be suitable for sett-making, though this is not necessarily true, as there are such factors as slipperiness and expense of working that may militate against a stone which is being used satisfactorily for chippings giving equal satisfaction as a sett.

As just explained, the suitability of an igneous rock for a specific purpose is governed primarily by the coarseness of its grain size. There are, however, other factors which will modify the properties of the stone. These may be summarised as:

(a) Uniformity of grain size, and of mineral distribution (see Fig. 1).

(b) Presence of weathered and decomposed minerals (see Fig. 2).

(c) Presence of deleterious minerals.

(d) Presence of minute fissures in the minerals (see Fig. 3).

(e) Special structural features between the various mineral-grains in the rock (see Fig. 4).

(a) Uniformity of Grain Size, and Mineral Distribution

For the purpose of roadmaking, it is only the chief minerals which are of importance, the minor accessories such as apatite, zircon, rutile, magnetite, etc., being present in such small quantities as to be of no account as affecting the physical properties of the stone. As regards the bulk minerals, such as quartz, felspar, hornblende, augite and mica, however, uniformity of grain size must be taken into consideration. In general, a rock of porphyritic type, such as some of the Cornish granites where the felspars may be anything up to two or more inches long, proves to be unsatisfactory in the road, largely because the big crystals tend to be brittle and there is not a good interlocking structure between separate crystals. Further, such a rock often shows a tendency to vary considerably from one part of a quarry to another, so that the ordinary crusher run grade of material may contain a large variety of material. This is detrimental, unless all the varieties are equally good, for

just as a chain is as strong as its weakest link, so it is the poor quality material in a batch of macadam that ruins the road, no matter how good the better parts of the macadam may be. In the same way a stone which has uniformity in the distribution of its constituents is to be preferred to one in which the minerals present vary in proportions from one part of deposit to another. Two outstanding examples of uniformity are the Whin Sill, which stretches for some 80 miles across the North of England, and has substantially the same mineral composition and grain size wherever it is worked, and the granite-porphyry of Bonawe, in which measurements show that specimens taken from all parts of the quarry are as nearly identical as different samples can be.

(b) Presence of Weathered and Decomposed Minerals

Weathered and decomposed minerals in an igneous rock may or may not render it unsuitable for roadmaking, according as to the particular form the breakdown of the primary minerals has taken. In this respect, the felspars are the most important minerals, as they may be sericitised or kaolinised, as indeed they frequently are to a greater or less extent. The most suitable igneous rocks show a decomposition figure of 15 per cent. or less. Where the felspars are seen under the microscope to be cloudy in the centre, but to have clear rims, the decomposition has little effect on the wearing properties of the stone ; if, however, the decomposition is seen to be in the borders of the crystals, or worse still to be in the form of a general cloudiness and breakdown right through, then the stone is immediately suspect, and would not prove suitable for roads which are carrying any appreciable amount of traffic. It is common in the less acid rocks to find that the augite and/or hornblende has been partly changed to green chlorite. This alteration is not usually detrimental to the strength of the stone, unless it reaches a very high percentage of the rock as a whole.

(c) Presence of Deleterious Minerals

There are few minerals which in the undecomposed state are definitely dangerous, but one in particular is of importance. This is chlorophæite, a peculiar substance of glassy appearance in the hand specimen, which has the property of oxidising and expanding when it is exposed to the air. If this mineral is present it is unusually dangerous if used as aggregate in concrete, since it will soon cause a whole batch of newly laid concrete to break up. Fortunately this is not a widely distributed mineral, and the quarries in which it is to be found are now known.

There are other considerations, however, than that of a mineral being actually dangerous, as for instance minerals which render a rock unsuitable for the purpose for which it is intended to be used. In this connection, it may be noted that hornblende and augite both seem to increase the slipperiness of a stone used for sett paving, while on the other hand the presence of about 10 per cent. of mica is an advantage, as this mineral is softer than quartz and felspar with which it is associated, and its more rapid attrition tends to leave a roughened and therefore more non-skid surface as the stone is used. On the other hand a stone which is to be used for macadam has been found to be more satisfactory if mica is absent, and hornblende and/or augite are present, as these two do not cause slipperiness when the stone is used in broken pieces and covered with a binder, while these more basic rocks are tougher than those of the granitic class because of their low content of free silica.

(d) Presence of Minute Fissures in the Minerals

Many of the finer-grained granites which are fresh yet give poor service in the road. This has been found to be correlated with the fact that in nearly all granites the quartz is more or less shattered or fissured. This is readily distinguishable under the microscope, especially if the section is treated with a synthetic resin dyed with methyl violet. This resin has a high surface tension and soaks into the fissures and renders them more easily seen. Where the fissuring on this small scale is really marked, it often happens that the cracks reach from the quartz crystals into the felspars. It is thought that this fissuring of quartz is connected with the change from β-quartz to α-quartz which occurs at the inversion temperature of 575° C. during the cooling of the magma, probably intensified by earth movements which occurred during the cooling, or even subsequently to it, since the granites which show the fissured quartz are to be found in disturbed areas. The fissuring has been shown to be independent of the percentages of the bulk minerals or of their grain size.

The amount of fissuring becomes more important as the size in which the stone is to be used decreases. Thus material intended for chippings should be substantially free from fissures in quartz, while for sett-making purposes it is not so important, though a high degree of fissuring will render an otherwise suitable stone far too brittle.

(e) *Special Structural Features as between the Several Mineral Grains in the Rock*

Stones which show under the microscope good interlocking between the several crystals, form tough roadstone. One of the best materials known is an epidiorite or thermally metamorphosed diorite which occurs at Penlee in Cornwall. In this the interpenetration of the crystals at their edges to form sutured boundaries is very well marked. Rocks which show ophitic crystal-growth are for this reason likely to be tough stones suitable for good macadam or chippings, the ophitic structure being one in which a mineral such as felspar is to a greater or less extent enwrapped by augite.

SEDIMENTARY ROCKS

While the best igneous rocks are much to be preferred to any sedimentary rocks, there are certain sedimentaries which are well suited to some purposes. For instance, some very hard forms of grit have been used with success as setts in the north of England, where various bands of Millstone Grit are sufficiently resistant to wear to be worth working. There are also some quartzites which are suitable for macadam, especially on lightly trafficked roads. A good example of a well-hardened quartzite is that of Hartshill in Warwickshire. The general criterion of small and uniform grain size, and the presence of interlocking structures due either to the cementing material, or to the rock having been thermally affected so that the quartz has partly fused together, are as essential to suitable arenaceous rock for roadstone, as in the case of igneous rocks.

Limestones of various kinds have been largely used in the past for roadmaking. In the old days of light traffic and water-bound macadam there was something to recommend this practice, since most districts have some reasonably good local supply of the material, and limestone binds together well when it is watered and rolled. It is safe to say that there are no limestones younger than those of Carboniferous Age in this country which can be relied on to give good service under modern traffic. Some of the Carboniferous Limestones, however, make good macadam and chippings for country roads which are not carrying much traffic.

Perhaps the largest use of limestone in the roadstone industry is as aggregate for concrete roads. In this case there are two factors which have to be watched very carefully, or the concrete will be a failure and the road will crack and disintegrate very rapidly.

These two factors are
- (a) Porosity ;
- (b) Clay content.

Failures have occurred because the aggregate was not examined before use with the object of determining these two factors. If the stone is highly porous, as often occurs with the younger limestones, then frost very readily disintegrates the aggregate, and the concrete breaks up. Clay content is also significant, especially if the clay is of the bentonitic variety. This and similar clays have the property of increasing enormously in bulk on absorption of water. If a limestone containing these very absorbent clays is employed in concrete, pieces of the aggregate swell and burst and in their turn break up the concrete of which they form part.

Flint gravel is very largely used in the south of England both for chippings and as aggregate for concrete. In the latter capacity it has proved very satisfactory, but its use as chippings is much to be deprecated. This is for two very different reasons. First, flint is a material which is very difficult to bind with tar or bitumen, so that often when a layer of flint chips has been put on the road and sprayed with tar and lightly rolled, in a short time, may be in a matter of days, most of the chippings have been driven into the channels by the passing traffic and the rain. This is very wasteful and unsatisfactory. The other objection to flint chippings is from the point of view of the motorist, for the crushed flints have very sharp edges which penetrate and ruin tyres.

Serious objections can be raised against the use of both limestone and hard arenaceous rocks for macadam and chippings. The trouble with limestone is that this stone is inherently slippery when wet and polished. For this reason many macadam roads which use limestone develop very unsatisfactory surfaces. Further, the use of limestone as chippings is liable to form a fine limestone flour by attrition which gives a very slippery or greasy mud when the road is wet. On the other hand, limestone has an affinity for tar and bitumen which makes it an easy material to bind, and when rolled it makes a well-knitted-together surface. Limestone which is to be used for macadam or chippings should of course be a tough one which is free from much clay and as non-porous as possible, as explained above with relation to concrete.

The chief trouble with quartzites and fine grits is that they tend to be brittle, as quartz is a mineral which fractures easily. Furthermore, silica is a mineral with a low affinity for tar and bitumen and difficulty is frequently experienced in practice in finding a binder which will give good adhesion to this material.

F F

The question of an aggregate which will give good visibility for night driving is of importance. Here it is the actual wearing surface which has to be considered, and for most of our roads that means the chippings. Limestone does not give a light surface, since the suitable limestones are well consolidated and this type of limestone is not a light-coloured one. Quartzites are rarely light enough and tough enough for the purpose, so that it is to the light-coloured igneous rocks that we must look for the solution. Good porphyries and felsites have proved to be the best, as they are of light colour and sufficiently fine grained to form a tough stone which will resist impact. Such materials are, however, not abundant, nor easy to work, so that they are expensive, and this has probably militated against their wider use. Suitable materials are found in the stone from Bonawe in Argyllshire, which will withstand a great deal of traffic, some of the elvans of the south-west, such as Tresarrett, which can also be used with success in heavy traffic, and for giving very good visibility some of the Norwegian white granites, though these are only satisfactory where the traffic is rather light.

GEOGRAPHICAL DISTRIBUTION OF ROADSTONE IN GREAT BRITAIN

Some parts of Great Britain are much more favoured than others as regards the accessibility of good roadmaking material. In the south-east, where Palæozoic rocks and igneous intrusions are absent, the only local material of any significance is flint, derived directly or indirectly from the Chalk. The various gravel deposits yield a suitable material for making concrete but, as explained above, flint is not a satisfactory surfacing material, so that for high-class chippings and macadam roads the aggregate has to be imported from other parts of the country.

The south-west is more favourably situated in that the elvans of Cornwall and the epidiorite of Penlee are in the area. The Cornish granites are not very suitable for roadmaking. Bristol and South Wales are close to regions producing a good grade of Carboniferous Limestone, but are not favoured with good igneous rock, excepting in the more western areas (Porthgain).

The Midlands generally are well situated in this respect, since there are large outcrops of good-class igneous material in Leicestershire, the hard quartzite of Hartshill in Warwickshire, the dolerites of Clee Hill, and the smaller intrusive bodies in Shropshire, as at Squilver. Further to the northern part of the Midlands the Carboniferous Limestone provides roadstone in North Staffordshire, Derbyshire and Yorkshire, while North Wales has both suitable

limestone and the igneous rocks of the Penmaenmawr district, a good-class material for the purpose.

The North of England is fortunate in having large exposures of the Great Whin Sill, a quartz-dolerite which makes a very good aggregate. Suitable limestone is also available. Threlkeld on the borders of the Lake District has a body of good micro-granite which makes a high-class aggregate, while the Shap Bluestone, not far away, is a baked rhyolite of high quality, as is also the porphyry of Cockermouth.

Scotland suffers in having too much metamorphic rock, which is so schistose as to be useless for roadmaking. Many of the granite intrusions, and also the series of east-west Carboniferous dikes of Southern Scotland, are also unsuited for the purpose. There is, however, plenty of good material in Scotland, notably in Argyllshire, though the county also possesses large areas of poor quality granite, and in Aberdeenshire, where the granites are very good for sett-making, but do not make such a high-class material for macadam or chippings if the traffic is heavy. A more basic rock of high quality is quarried at Troon in Ayrshire, though, like most dike rocks, it is very dark in colour.

Enough has been said to show the importance of the application of scientific methods to the choice and use of surfacing materials, not only from the view-point of public economy, but also from that of individual safety.

THE EXCHANGE OF ENERGY BETWEEN A GAS AND A SOLID OR LIQUID SURFACE

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I. Introduction

The transfer of heat from a hot solid to a gas takes place by means of the exchange of energy between the surface molecules and the gas molecules coming into contact with them. Whenever a gas molecule strikes the hot surface, it has a definite probability of attaining temperature equilibrium with the solid and a measurement of the total heat transfer renders possible an estimate of this probability. From a theoretical point of view such measurements are important in providing a test of the theories of energy transfer during the collision of a free gas molecule with a molecule of the solid. Further, the heat transfer is largely controlled by the physical state of the solid surface so that the measurement of this quantity provides a method which can be used as an alternative to the electron diffraction methods in the examination of the structure of surfaces and of films formed thereon.

In recent years a great deal of experimental work on the interchange of energy at a solid surface has been performed and it is the object of the present paper to give a brief summary of the more important results so far obtained.

II. The Accommodation Coefficient

If a solid at temperature T_2 is immersed in a gas at temperature T_1, $(T_2 > T_1)$ there will be a conduction of heat from the solid to the gas. This energy transfer was very thoroughly investigated many years ago [1, 2, 3] and it has been shown that in general the amount of energy given up to the gas is not so great as would be expected on the assumption that each gas molecule striking the surface attains temperature equilibrium with the latter before again evaporating into the gas phase. In other words, the velocity of rebound of the gas molecules depends on their velocity of incidence as well as on the temperature of the surface on which they are incident.

If the energy exchange is complete in any particular case, the velocity distribution in the rebounding gas molecules will be that corresponding to the temperature (T_2) of the solid. If, however, the exchange is incomplete, the gas temperature (T'_2) will be lower than T_2 and the ratio

$$\lim_{T_2 \to T_1} \left(\frac{T'_2 - T_1}{T_2 - T_1} \right) = \alpha$$

has been defined [1] as the " accommodation coefficient " at the surface.

This coefficient measures the degree of perfection of the energy transfer and is the quantity in which we are particularly interested. If the gas molecules all attain temperature equilibrium with the surface, α will be equal to unity. If there is *no* energy exchange, it will be equal to zero. Hence $0 < \alpha < 1$, and it appears that the larger the value of α, the more complete is the energy transfer.

For accurate determinations of α the usual experimental arrangement is to immerse a thin heated wire in a gas at very low pressure where the mean free path of the gas molecules is long in comparison with the diameter of the wire, and to measure the heat conducted from the wire by the surrounding gas. The gas pressure is measured so that the number of molecules striking the wire can be calculated and hence the maximum theoretical heat transfer. The ratio of the actual to the theoretical heat transfer then gives the value of α.

Early work showed that α was smallest for the lighter gases H_2 and He, and that it did not depend greatly on the nature of the solid surface. This is illustrated in the following table, which shows the accommodation coefficient of hydrogen on various solids.

TABLE I.

Author.	Surface.	α
Knudsen [1]	Glass	·26
Soddy and Berry [3]	Pt	·25
Blodgett and Langmuir [4]	W	·22
Hughes and Bevan [5]	Ni	·25
Chapman and Hall [6]	Ag	·25
Rowley and Evans [7]	Fe	·31

These determinations were all made at temperatures between 300° K and 400° K.

When a gas atom strikes a solid surface it may follow any one of several courses :—

    (1) The atom may be specularly reflected from the surface as has been shown by Stern [8, 9] and others. In this case it spends a negligible time in contact with the surface and the energy exchange between them is a minimum.

(2) It may be temporarily trapped among the surface molecules and rebound from one point on the surface to another before returning finally to the gas phase. The rougher the surface of the solid the greater will be the number of collisions suffered by the atom in this way.

(3) It may condense on the solid surface, remain there for a longer or shorter time depending on the intensity of the forces of adsorption, and eventually be re-evaporated.

If the gas is initially at a temperature different from that of the solid the energy exchange between them will depend on the relative importance of the three processes described above. It will be most complete in those cases where many of the incident molecules are adsorbed on to the surface, less complete when the majority leave the surface after a small number of collisions and still less complete when a large fraction of the incident molecules are specularly reflected from the surface. The experimental measurement of α thus throws light on the nature of the interaction between the gas and the solid molecules and, as will be shown later, can be used to examine other surface phenomena.

In recent years renewed interest in the question of accommodation at a surface has been aroused for two reasons. It has been found possible to work out a theory of heat transfer by wave mechanical methods and it has been shown, primarily by Roberts [10, 11] that many of the early determinations of α were complicated by the presence of adsorbed gas films on the solid surface. In the presence of such films collisions at the surface would be mainly between free gas atoms and adsorbed gas atoms and would be influenced only indirectly by the nature of the underlying solid. It is therefore not surprising that the nature of the solid did not appear to be of great importance. When the film has been removed the influence of the solid is much more pronounced.

III. The Accommodation Coefficients of Neutral Atoms and Molecules at Solid Surfaces

The first attempt at a theory of the accommodation coefficient was made by Baule [12], who treated the collision between the gas and solid atoms by classical methods. He considered the solid atoms to be in a state of vibration and applied the laws of conservation of energy and momentum to calculate the energy change when the gas atoms strike this system of oscillators. It follows from his theory that the energy exchange is complete if the mass of the gas atom is greater than that of the atom of the solid. For the case in which the gas atom is the lighter, Baule obtains an

expression for α in terms of the masses of the particles, which indicates that α should be smaller the smaller the mass of the impinging gas atom. These deductions from Baule's theory are qualitatively supported by experiment; the quantitative agreement between the theory and experiment is however not good except in certain particular cases. Further, the theory gives no information about the temperature variation of α.

The wave mechanical treatment of the problem is not yet complete but the new method appears much more promising than the classical one. It has been developed chiefly by Zener [13, 14] and Jackson and Howarth [15]. They consider the collision of an inert gas with a solid crystal and calculate the probability that the solid atom struck by the gas atom shall have its vibration or rotation quantum state altered by the collision. Such quantum jumps would involve change of energy of the solid, and by summing over all possible changes, the total energy exchange can be determined and α deduced therefrom. Zener [14] obtains as his expression for α

$$\alpha = \pi^2 \left(\frac{m_g}{m_s}\right) \chi\left(\frac{\theta}{t}\right) \Omega\left(\frac{d}{\lambda}\right)$$

θ is the characteristic temperature of the metal; m_g and m_s are the masses of the gas and solid atoms respectively; the function $\chi\left(\frac{\theta}{t}\right)$ has values lying between zero and unity and represents the effect of quantisation of the solid. The function $\Omega\left(\frac{d}{\lambda}\right)$ also has values between zero and unity and gives the effect of a finite time of collision on the energy exchange. Both these functions can be calculated for any given experimental conditions. Zener considers only normal impacts so that his result may need correction by a constant factor of the order of unity. The variation of α with temperature predicted by the theory has the same form as that found experimentally in recent work [11].

The theory of the collision of polyatomic molecules at a solid surface is complicated by the fact that they may suffer not only changes in their average kinetic energy but also in their internal energy. For this reason the theory of such impacts has not yet advanced far.

The same complication arises in the interpretation of experimental results. Knudsen [1] in his work on the accommodation coefficient of hydrogen has assumed that α for translatory energy is equal to that for internal energy. This assumption has been criticised by Smoluchowski [2] and recently Knudsen [16] has undertaken a new set of experiments to prove this equality of $\alpha_{(\text{translation})}$ and

$\alpha_{\text{(rotation)}}$. He used hydrogen and helium as experimental gases with two platinum ribbons, one polished on both sides and the other polished on one side and blackened on the other. By comparing the heat losses from the two strips he obtains α', the accommodation coefficient for polished platinum and $(\alpha' - \alpha'')$ the difference between the coefficients for polished and blackened platinum.

Knudsen then measured the radiometer pressure $(p' - p'')$ on the two sides of the semi-blackened ribbon. This depends on the *momentum* of the impinging molecules and not on their internal energy at all. The momentum of rebound from the strip will depend on the magnitude of $\alpha_{\text{(translation)}}$ only, so that from the measurement of $(p' - p'')$ it is possible to deduce $(\alpha'_t - \alpha''_t)$ the difference between the accommodation coefficients for translation energy at polished and blackened platinum surfaces. He compares the difference $(\alpha' - \alpha'')$ between the " total " accommodation coefficients with that $(\alpha'_t - \alpha''_t)$ between the coefficients for translatory energy only and obtains the results shown below :

For hydrogen $\alpha' - \alpha'' = \cdot420$
$\alpha'_t - \alpha''_t = \cdot415$
For helium $\alpha' - \alpha'' = \cdot498$
$\alpha'_t - \alpha''_t = \cdot512$

so that for both gases $\alpha' - \alpha'' = \alpha'_t - \alpha''_t$.

Since helium exchanges only translational energy with the wall while hydrogen exchanges both translational and rotational energy, it follows that the accommodation coefficient for translational energy in hydrogen must be equal to that for rotational energy in the same gas.

On the other hand, Knudsen's conclusion is not accepted by Bonhoeffer and Rowley [17], who measured the accommodation coefficient of various mixtures of ortho- and para-hydrogen at a platinum surface. Ordinary hydrogen gave $\alpha = \cdot34$ at room temperature and had a negative temperature coefficient. When parahydrogen was introduced, α decreased with increasing concentration. Now the hydrogen exchanges both translational and rotational energy with the surface but in parahydrogen the rotational energy forms a larger part of the whole. Since the efficiency of total heat exchange decreases with increase of parahydrogen concentration, they deduce that $\alpha_{\text{rotation}} < \alpha_{\text{translation}}$ and from the measured variation of α with composition calculate that

	$\alpha_{\text{translation}}$	α_{rotation}
at 140° K.	$\cdot43$	$\cdot26$
and at 170° K.	$\cdot44$	$\cdot18$

These authors suggest that $\alpha_{rotation}$ possesses a large negative temperature coefficient which swamps the normal positive coefficient of $\alpha_{translation}$ and leaves the total α with a small negative coefficient, as found experimentally.

A similar increase in the accommodation coefficient of hydrogen with diminishing temperature was observed by Rowley and Evans [7]. They used an iron wire and found that α for hydrogen increased from 0·3 at 350° K. to about 0·6 at 120° K. Their results, too, refer to an iron wire covered with an adsorbed hydrogen film.

The value of the accommodation coefficient depends greatly on the nature and extent of the gas film adsorbed on the metal surface. The exact influence of such films cannot however be determined until α at a completely clean surface has been measured. Once this quantity has been obtained, small quantities of adsorbable gas may be introduced into the apparatus and the formation of the adsorbed layer may be followed by its influence on the accommodation coefficient, which usually increases considerably when adsorption takes place. The measurement of α at a clean metal surface is therefore of fundamental importance both in providing a test of the theory under the simplest experimental conditions and also in making possible the examination of surface films by the accommodation coefficient method.

The first measurements in which adequate steps were taken to remove adsorbed films of impurities from the solid were those of Roberts [10]. In order to avoid difficulties due to changes of internal energy, monatomic gases were used with a tungsten wire. He thoroughly outgassed his apparatus before each experiment and kept the gas (helium) pure by circulating it through charcoal and liquid air traps during the experiment. The usual hot wire method was employed and the surface was cleaned immediately before the measurement of α by raising its temperature to 2000° K. for a minute. The accommodation coefficient was measured about once a minute during a period of time commencing at the instant at which the temperature of the wire was reduced from 2000° C. to about 20° C. above that of the surrounding gas. Roberts' results indicate that for some time after reducing the temperature of the wire, α increases steadily. This increase he ascribes to the slow building up of a film of impurity on the surface. He obtains the true value of α for a perfectly clean surface by plotting his experimental determinations against the time and extrapolating the almost linear curve so obtained to the initial instant at which the heating current was cut off.

The result of this great care in working with clean gas and solid

was to show that the accommodation coefficient at a clean surface was much smaller than any previously measured. Roberts' value of α for helium on tungsten was 0·05–0·07 and for helium on nickel 0·085. He also showed that after the gas had remained in the apparatus all night without being circulated the accommodation coefficient rose to 0·19. The higher value would be that for a film-covered wire, and this result confirms the fact that the high results of earlier workers were also due to this cause.

In more recent work Roberts has shown that the accommodation coefficient of helium on tungsten decreases rapidly with temperature, being only 0·025 at –194° C. The variation of α with temperature indicates that at the absolute zero the accommodation coefficient must be very nearly zero. He has also [18] measured α for neon at a clean tungsten surface, obtaining the value $\alpha = 0·07$ at 295° K.

Experiments similar to the above were carried out by Mann [19], who used a platinum wire instead of the tungsten employed by Roberts. Mann found it necessary to keep the platinum wire at a temperature of 1000° C. for as long as 50 hours before constant values of α were obtainable. He supposes that this prolonged initial heating is necessary to drive out the gases absorbed in the wire. Once the wire was cleaned, any new surface contamination could be removed by a further heating of a few minutes' duration. If the platinum wire were heated in an atmosphere of oxygen for 20 hours, Mann found that the oxygen attacked the metal and that after this treatment the values of α were much reduced and the wire was less susceptible to contamination. With a platinum wire treated in this way he found the accommodation coefficient of helium to be between 0·030 and 0·035, values rather lower than those obtained by Roberts for tungsten.

As in Roberts' experiment, α increased with the time after the wire had been newly cleaned. On working with argon, Mann found no such time variation, α assuming its equilibrium value immediately after the wire was cooled from its cleaning temperature. The values obtained for argon ranged from 0·748 at 100° C. to 0·355 at 1000° C. The heat exchange is thus much more complete than for the lighter gases helium or neon.

In the case of hydrogen Mann showed that α increases with decreasing temperature at low temperatures (room temperature and below) and increases with increasing temperature at high temperatures (1000° C.). He ascribes this to adsorption of hydrogen at the low temperatures and penetration of the hydrogen into the metal at high temperatures. From his results between the two limits (at

about 800° C.) he deduces the value of α on clean platinum at 813° C. to be about 0·08 and suggests that the values found by Bonhoeffer and Rowley [17] are too large because they gave their platinum wire a preliminary heating of only one hour.

Very interesting measurements of the accommodation coefficient of hydrogen on tungsten have been made by Langmuir and Blodgett [4]. They find that at 1300° K. the accommodation coefficient on a clean wire is 0·54 and that the value is greatly reduced by the presence of a trace of oxygen on the surface. This effect was also noted by Mann. The great difference between the accommodation coefficient of hydrogen at a clean tungsten surface (0·54) and that found by Mann at a platinum surface (0·08) is very surprising.

From their results Blodgett and Langmuir deduce that at temperatures less than 1200° K., hydrogen is adsorbed on tungsten in two forms, both of which reduce α. An atomic hydrogen film is formed at the higher temperatures and a molecular one at low temperatures, and the one changes over into the other at an appreciable rate at about 600° K. For the tungsten wire covered with an atomic film of hydrogen (i.e. at temperatures greater than 600° K.) they find $\alpha = 0·143$. They also show that, at room temperature and below, α increases with decreasing temperature. This effect is no doubt due to an increasing concentration of molecular hydrogen on the surface which is thereby rendered more " molecularly rough " and so gives rise to a more complete energy exchange.

Roberts [18] has also extended his work to investigate films formed on tungsten. He found that the accommodation coefficient of neon increased from 0·07 to 0·6 when an adsorbed film was formed on the wire and used this sensitivity of α to the presence of a film to examine the formation of the latter. His method was to measure α for neon on clean tungsten, then introduce a little hydrogen into the neon and measure α again. He found a very rapid increase of α when the hydrogen was introduced. On introducing more hydrogen a further rise of α was produced. The process was continued until no further change in α followed further introduction of hydrogen. At this stage Roberts concludes that the wire must be completely covered with a hydrogen film. From the quantity of gas introduced he was able to show that each surface atom of tungsten is covered by a single H atom. The values of α for the saturated atomic hydrogen film were found to be 0·175 at 295° K. and 0·32 at 79° K. The film was formed almost instantaneously at hydrogen pressures as low as 10^{-4} mm., and Roberts therefore concludes that it is impossible at ordinary temperatures to have a *clean* tungsten wire in any atmosphere containing even a trace

of hydrogen. He also shows that this atomic hydrogen film is stable up to 900° K.

Similar experiments were carried out with oxygen. Small quantities of this gas were added to the neon and produced large changes in α. Again it appeared that the film formed was an atomic one, each tungsten surface atom being covered by a single oxygen atom.

The accommodation coefficient of neon was used to investigate the stability of the adsorbed film. For this purpose a saturated oxygen film was formed on the tungsten and the temperature of the latter was raised to (say) 100° C. for 1 minute. At the end of that time the temperature of the wire was reduced to its original value and the accommodation coefficient of neon was redetermined. If any appreciable evaporation of the oxygen film took place at 100° C., α after the heating would be smaller than before. This procedure was repeated, the wire being heated to a higher temperature in each successive experiment. It was found that gentle warming made no difference to the value of α, which remained unaltered until the temperature was raised to about 350° K., when it began to decrease from its original value of 0·36 to a second equilibrium value of 0·24. No further change in α occurred when the wire was heated beyond 1300° K. These results are interpreted as showing the presence of two different oxygen films, one very stable even at 1300° K. and the second easily removed at 350° K. Evidence is given to show that the first film is atomic and the second molecular.

These experiments of Roberts show how the measurement of the accommodation coefficient in a clean system can be made to yield detailed and valuable information regarding the nature of the solid surface at which the heat exchange occurs.

A novel method of measuring α is that used by Ornstein and Wyk [20]. They used two concentric glass cylinders with the gas (helium) in the annular space between them. The inner cylinder had its outer surface roughened so that all the molecules striking it would attain temperature equilibrium with it. This cylinder was cooled while the outer one was heated electrically to about 380° C. An electric discharge was passed through the helium and was viewed through a window in the outer cylinder, the direction of vision being along the radius of the cylinder. The spectral lines photographed through the window showed an unsymmetrical broadening, since the light originates partly from atoms moving towards the window with a velocity distribution corresponding to the temperature of the roughened inner cylinder and partly from atoms moving away from the window with a velocity distribution

corresponding to a temperature equal to or lower than that of the outer cylinder, the actual temperature depending on the completeness of the heat exchange at the outer wall. From the intensity distribution on the two sides of the helium line 5016 A the authors deduce the temperatures of the two streams of atoms moving to and from the hot wall, and hence, knowing the temperature of the hot wall itself, they can deduce the accommodation coefficient of helium on glass. With wall temperatures of 650° K. and 370° K. they found that the gas temperatures were 480° K. and 400° K. They consider that the difference between the temperature (370° K.) of the cold wall and that of the gas stream leaving it (400° K.) is due to the warming effect of the discharge. Their result is therefore

$$\alpha = \frac{T'_2 - T_1}{T_2 - T_1} = \frac{480 - 400}{650 - 400} = \frac{80}{250} = 0.32.$$

In spite of the large value of $(T_2 - T_1)$ the result is not very different from those found by the hot wire method for similar film-covered surfaces.

Zaicev and Spivalc [21] measured α for the inert gases at a tungsten wire and considered the variation of α with temperature and with the atomic weight of the gas atoms. Films of oxygen and CO_2 were then produced on the tungsten surface and α was remeasured. The values obtained under different conditions are shown below.

Gas.	On Clean Tungsten.	Tungsten with O_2 Film.	Tungsten with CO_2 Film.
Ne	·05	·33	·15
A	·10	·63	·24
Hg	·58	—	—

The above figures show that, on clean tungsten, α is very approximately proportional to the atomic weight of the gas. The usual very large change in α produced by the presence of a gas film is seen, and this is taken to indicate that when such a film is present collisions take place primarily between the adsorbed gas film and the free gas atoms. This point has already been mentioned.

Zaicev and Spivalc show that their results are not in agreement with Baule's classical theory of the accommodation coefficient but are in better agreement with Zener's quantum theory of the effect. In Zener's expression for α

$$\alpha = \pi^2 \left(\frac{m_g}{m_s}\right) \chi\left(\frac{\theta}{t}\right) \Omega\left(\frac{d}{\lambda}\right)$$

m occurs only in the second and last factors. If its influence on the latter factor is not large, then approximately

$$\alpha \propto m_g$$

as was found experimentally by Zaicev and Spivalc. On the other hand, for comparison with these results it may be noted that Roberts found

$$\alpha_{He} = 0 \cdot 05 - 0 \cdot 07$$
$$\alpha_{Ne} = 0 \cdot 07$$

so that in this case the accommodation coefficient does not appear to be proportional to the atomic weight of the gas.

IV. THE ACCOMMODATION COEFFICIENT OF IONS

The work described up to this point refers to the energy exchange between neutral atoms and a metal surface on which they are incident. A very interesting extension of the idea of an accommodation coefficient has been made by K. T. Compton and his collaborators [22, 23, 24, 25], who have investigated the energy exchange between gaseous ions and a metal cathode at which they are discharged. It has usually been assumed that when the ion collides with the cathode it gives up its energy to the metal and rebounds with a negligible velocity into the gas. This does not now appear to be the case. Van Voorhis and Compton [23] placed a 3 mm. molybdenum sphere in a low voltage gas discharge and accelerated positive ions towards it. A copper constantan thermocouple was embedded in the sphere and the current to it and the heating produced in it were measured. This heating was compared with that to be expected if each ion neutralised at the sphere gave up all the kinetic energy it had acquired in the attracting electric field. The accommodation coefficient is then defined as the ratio of the *measured* energy transfer to the sphere to the *calculated* transfer. In this way they obtained the following results :

Gas.				α
A	.	.	.	0·75
Ne	.	.	.	0·65
He	.	.	.	0·55 for accelerating potentials of 111–141 volts.
				0·35 for accelerating potentials of 21–51 volts.

For argon and neon, α was independent of the accelerating voltage, but for helium the value increased with the voltage—probably because the small helium ions penetrate into the target more and more as their velocity is increased.

The above experiment leads to a measurement of the total energy transfer to the sphere. A second experiment due to Compton and Lamar [24, 25] measures the accommodation coefficient for translatory energy only. In this experiment an auxiliary cathode

in the form of a light pendulum having at its lower end a plate of molybdenum was immersed in the discharge. The pressure exerted by the ions striking the front surface of this plate was measured by the deflection produced in the pendulum. From this deflection and the current collected by the pendulum, the total momentum transfer to the molybdenum plate could be calculated and hence the accommodation coefficient of helium ions at a molybdenum surface. For helium ions accelerated to the pendulum by potentials of 35 volts and 125 volts Lamar found $\alpha = 0.491$, in good agreement with the values of Compton and Van Voorhis. The two methods are thus consistent. Owing to the high speeds of the ions striking the metal surface, Compton [26] considers that Baule's classical theory should apply to his results. He therefore uses this theory and shows that his results are in very good agreement with it. He shows that the value of α for argon calculated from the theory is 0.83, while its experimental value was found to be 0.87–0.82.

The results obtained in a *neon* discharge are interpreted as being due to a slight penetration of the gas into the solid, and from the magnitude of α Compton and Lamar deduce that 64 per cent. of the neon atoms are reflected in the outermost layer of molybdenum atoms, 64 per cent. of the remainder in the second layer, and so on.

In the case of helium the penetration is so great that the authors suppose the helium atoms to be scattered at random into the metal like elastic spheres. This leads to a value $\alpha = 0.75$ as an upper limit for α. The experimental values are 0.55 and 0.35, which are about what would be expected. It must be remembered that these high values apply to high speed ions. Roberts' value of $\alpha = 0.05$ for helium gas indicates that the ordinary thermal collisions with the metal surface lead to little penetration into the metal. Compton and Lamar conclude that the classical theory of the accommodation coefficient is quite adequate to explain momentum and energy transfer by ions at a metal surface.

V.   Accommodation Coefficients of Liquids

Few measurements of the accommodation coefficient at a liquid surface have been made. The presence of vapour above the liquid surface complicates the experiment somewhat and the only determination of α for a liquid appears to be that of Alty and Mackay [27], who measured the accommodation coefficient of water vapour at a water surface at 0° C. They showed that the energy exchange between the liquid and its vapour was complete, α being equal to unity. This result would be expected if every vapour molecule striking the surface succeeded in entering the liquid. In that event

its energy would naturally be given up to the liquid. However, the incident molecules do *not* all appear to be able to penetrate the liquid surface. It has been shown [27, 28] that only about 4 per cent. of the water vapour molecules incident on the liquid surface are able to enter the liquid. The other 96 per cent. must therefore collide with the surface of the liquid and attain thermal equilibrium with it before returning to the vapour phase. A similar state of affairs seems to exist for ethyl alcohol. In the cases of carbon tetrachloride and benzene [29], however, and of mercury [30] no surface reflection of this sort takes place, all the incident molecules entering the liquid. Hence for these liquids also α must have the value unity and, in spite of the apparent difficulty experienced by the vapour molecules of some liquids in penetrating the liquid surface, there is no known exception to the rule that the accommodation coefficient of a vapour at the surface of its liquid has always the value unity.

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RESINOUS PLANT PRODUCTS

By T. HEDLEY BARRY, F.I.C.

RESINOUS products play a very important part in both the scientific and industrial activities of the present day. The introduction of synthetic resins not only resulted in the establishment of new industries but opened up an entirely new field for chemical and physical research. In his attempts to establish the constitution and chemical relationships of a substance, the chemist sought to obtain crystalline products with definite physical constants and not unnaturally regarded the production of a resinous or tarry mass as a disaster. The discovery that such products might have industrial applications therefore reversed the position, and the study of reactions with the specific intention of avoiding crystallisation and producing resins has become an important branch of chemistry. The chemical and physical knowledge gained in the study of synthetic resins has also had a beneficial effect upon the scientific study of the natural products.

Inspiration to the study of natural resins was also received from the rapid development of the chemistry of the terpenes to which it was clear that many of the characteristic ingredients of the natural resins were closely related both chemically and botanically. Recently another and more fundamental consideration has led to an intensified study of natural products and particularly those of forest origin.

For many years archæologists, botanists and geologists have insisted upon the vitally important part which forests play in maintaining that balance of forces on which the existence of man in civilised communities depends. It cannot be over-emphasised that denudation, which results from the destruction of trees and over-cropping of the soil, is irreparable—a desert cannot be rendered fertile by allowing it to lie fallow for a time. All these impulses to research on the natural resins have exercised a definite influence upon both the direction and intensity of research. From the point of view of the conservation of natural resources, the main con-

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sideration is of course the preservation of the forest whilst at the same time obtaining the maximum production of resin.

There are, however, many resins of industrial importance, the valuable characteristics of which are only developed in the long periods, often many centuries, during which they have been exposed to the atmosphere or buried in the soil, subsequent to exudation from the tree. Of these resins, the enormous copal deposits found in the Congo basin are the most important, and here the problem is to devise methods of collection, preparation, and use in industry which will enable the resin at least to maintain its position in industry.

In the East Indies the problem is of an intermediate character as both the recent and the old exudations are of value and the trees are of such slow growth and great age that the vigorous methods of exploitation, combined with replanting, which have been brought to a high state of perfection in dealing with the comparatively quick-growing pines from which resin and turpentine are obtained, cannot be adopted.

A similar respect for great age also compels caution and leisurely methods in dealing with the valuable Kauri trees of New Zealand. So far as the chemist is concerned, it is clear that as the first step in the systematic study of these substances must be the establishment of the botanical origin of the resin, active co-operation of the botanist is required. This is by no means an easy process, and even when the parentage of the resin is established, the value of the information is much limited by the fact that apparently the production of resin is only a minor activity of the tree and consequently the classification of the botanist does not help the chemist so much as he would wish in his endeavour to trace the chemical processes involved in resin production, nor assist in the co-relation of the characteristics of the resin either with those of the species which produced it or with the climatic and soil conditions under which the particular tree which produced the resin grew. It does appear, however, that there is some connection between the species of tree and the resin produced. Thus among the coniferæ the resins obtained from Pinus species all contain as characteristic ingredients, acids, closely related to abietic acid, whilst the resins from Agathis species contain an acid (agathic acid) which is structurally related to pine resin acids. Another group of coniferæ, Cupressineæ, growing in places so far apart as Australia and North Africa, yield almost identical resins (sandarac), though in this case the structure of the acids is unknown, and the composition of the exudation is very different from the pine resin. A chemical link with the pine

resins is, however, found in the presence of the characteristic pine resin acid, pimaric acid. The significance of these connections, however, cannot yet be assessed, for our knowledge of both the structure of the resins and the process of their formation is still of a very disconnected character. So far our detailed knowledge of the structure of resins is confined to the acids of the pine resins which form the basis of the important naval stores industry, and even in this case there still remain the non-acidic constituents of the structure of which we are totally ignorant. There are also many species of pine which yield comparatively small quantities of oleo-resin. This question of the connection between botanical origin and the constitution of the resins is of more than academic interest. Apart from the necessity of the chemist being able accurately to describe the source of his material, the detailed investigation of the chemical products of plant life is often equally valuable to the botanist in selecting species for cultivation with a view to economic exploitation. On the other hand, chemical differences in the constitution of plant products are sometimes found for which the classification of the botanist affords no explanation. Thus the camphor tree (*Cinnamomum camphora*) appears to be of two kinds, one producing solid camphor, and therefore of economic value, and the other producing camphor oil, which is practically valueless—a difference of much interest to the British Empire, as apparently the trees producing the solid camphor are confined to Japanese territories, though the camphor trees of New Zealand may prove worthy of economic development [30]. Equally remarkable differences were found by A. R. Penfold and his colleagues in the Eucalypts [1]. The species, *E. dives*, for example, was found to be divisible into three sub-groups differing in the piperitone content of the essential oil. As the value of the oil for the manufacture of thymol and menthol depends mainly on the piperitone content, the difference is an important one.

The difficulties of botanical classification are enormous. In the first stage, at least, classification must necessarily be based upon morphological and anatomical data obtainable from herbarium specimens, but when the question of utilising the results either in general science or in industry arises, a more comprehensive scheme including plant biology and ecology is necessary and the importance of collaboration with the chemist in the development of such a system of classification is evident. So far as the resins and essential oils are concerned, the classic researches of R. T. Baker and H. S. Smith on the pines and eucalypts of Australia is an outstanding example [2, 3], and although one is glad to see such work worthily

enshrined in splendid volumes, it is to be regretted that it is not also available in volumes more suited to the chemist's laboratory bench.

As a result of these researches, Baker and Smith came to the conclusion that, so far as the Australian trees were concerned, the older classification of the botanist could be so modified by taking into account the chemical characteristics of the essential oils and resins with due regard to the conditions under which the tree grew, that it was possible to arrange a classification in which chemical and botanical features were presented in a coherent and logical sequence. One of their most interesting and useful observations, for example, was that the system of venation of the leaves of eucalypts was a reliable index to the composition and amount of essential oil obtained from them.

The presidential address by Hill already referred to and that by Professor Barger [4], in the previous year, are also very encouraging and inspiring to the chemist who realises that the importance of the field and forest to humanity is even greater than that of the factory.

During recent years much work has been carried out on the turpentine-yielding pines of America, in order to determine the effect of tapping upon the number and distribution of resin ducts. The more directly practical problem of the relation between the condition of the tree and the yield of oleo-resin has been closely studied in France, the United States and India.

An extremely important part of the work in America is that based upon the microscopic study of the effect of tapping upon the distribution and number of resin cells [5]. The resin cells are located in the sap used, and are of two kinds, horizontal cells which remain active for some years (until the sap wood begins to change to heart wood) and tracheids in the outermost layers which early lose their protoplasm and act as resin ducts. The immediate effect of tapping is to empty the cells of resin, but it also stimulates the formation of resin cells, the effect being noticeable as much as 8 or 9 feet above the wound. In the horizontal direction, however, the effect does not extend more than a few inches. About 90 per cent. of the resin is exuded in the first three days, and after the sixth the flow is negligible so that the wound or blaze has to be freshened up and extended vertically. In this respect, the pine tree differs from the "Douglas Fir," which is often suggested as a source of resin and turpentine on the evidence of remarkable yields from single trees [6]. In these trees, however, the resin accumulates in internal fissures (wind shakes), but though the

amount may in some cases be large, normally tapping operations fail to produce any quantity of resin, though if the accumulated resin is drawn off and the tap hole plugged, a further small supply can be obtained.

In the East Indies and Malaya, the problem of tapping both copal and damar trees has been studied from the strictly practical point of view of forest preservation, but little or no work has been done upon the structure and distribution of the resin-producing cells. This is to be regretted, as experience has shown that whereas the copal-producing conifers are best tapped by means of a blaze in much the same way as the pines are tapped for resin, the damar-producing Dipterocarps are best tapped by tap holes about 2 inches square distributed all over the trunk and even over the larger branches [7]. This of course may be connected with the proportion of liquid terpenes to resins produced by the tree and consequently the fluidity of the oleo-resin. It is of interest to note that apparently the composition of the exudation from the copal trees of this area may vary widely. The tree *Agathis alba* normally produces the resin known as Manila copal, but under certain conditions a liquid exudation is obtained, "Damar Minyak," which contains about 13 per cent. of terpenes mainly of high boiling point, so that the exudation never hardens [8]. Normally, the exudation contains only 3 or 4 per cent. of volatile terpenes and is sufficiently hard to be collected after a few days.

Such abnormalities are of much interest as they indicate that the constitution as well as the quality of the exudation is determined, to some extent at least, by the conditions of the tree. It would seem not impossible therefore that, given an accurate knowledge of the chemical and biochemical processes involved in resin production, the nature and quality of the exudation might be altered, within certain limits at will. In the case of annual crops, this of course is quite a common achievement, but the difficulties involved in applying similar scientific methods to trees with a life cycle of several generations are obviously very great.

Variations of this extent, however, are not common, as regards the resins of any one particular species. If the resins are to be regarded as a more or less final stage in the chemical history of the terpenes produced by the tree, this is perhaps to be expected, but at the same time the very limited nature of our chemical knowledge must be remembered, for this seeming uniformity may be due to lack of subtility in our methods of examination.

This survey of the botanical aspects of the subject as they appear to the chemist may appear somewhat incoherent and dis-

couraging to those accustomed to the ordered sequence of chemical synthesis in the laboratory. It is, however, necessary that the nature and magnitude of the problems be realised and especially the difficulties with which the botanist is faced in his efforts to reduce the innumerable forms of vegetable life to some sort of orderly and scientific sequence. So far as the particular group of substances under consideration is concerned, the difficulties are the greater because it is by no means clear what status the terpenes and resins occupy in the internal economy of the plant.

The general opinion seems to be that they are mainly waste products and, apart from forming a protective layer over wounds in the bark, have no particular value. The essential oils seem to be of a similar character, though in some cases, more especially leaf oils, they may attract or repel the attentions of insects.

This view, however, is not to be taken as final. Baker and Smith point out that the oleo-resin is produced in the tree, and as it only exudes when the bark is wounded, presumably the tree must find some use for it under normal conditions. On the other hand, the fact that the tree can respond to tapping by developing more resin seems to indicate that beyond a certain point at least the production of resin is a pathological condition.

As to the balance between the resinous exudation and other plant products, it appears from the few cases examined that increase in the production of resins is accompanied by a reduction in the formation of starch and tannins [9]. This is a matter of some interest in view of the occurrence of " gum resins " in which both carbohydrates and terpenic compounds are present; for example, the exudation from *Boswellia serrata* [10], an Indian tree of the order Burseraceæ, has the composition :

Moisture	.	.	.	.	.	.	.	10–11 per cent.
Essential oil	.	.	.	.	.	.	.	8–9 ,,
Resin .	.	.	.	.	.	.	.	55–57 ,,
Gum .	.	.	.	.	.	.	.	20–23 ,.
Insol. .	.	.	.	.	.	.	.	4–5 ,,

The resin, however, is very different from that obtained from the Coniferæ. On the other hand, the " damar Hitam " which is obtained from a Burseraceæ species grown in Malaya in no way resembles the exudation from *Boswellia serrata* and is not associated with a gum. It does not appear therefore that trees of closely related botanical origin necessarily yield exudations of the same type, though in many cases this is so.

Turning to the more purely chemical aspects of natural resins, the most interesting development of recent years has been the

way in which so many diverse substances closely associated with biological functions have been found to be structurally related.

The basis of our present conceptions was laid by Wallach [31], Ipalieff [32] and Euler [33], whose work in the 'nineties suggested the connection between the terpenes and isoprene. In the hands of Ruzicka this idea has proved one of the most fruitful in modern chemistry. From the study of the terpenes Ruzicka and his colleagues proceeded to that of some of the more easily obtainable and well-defined resin acids and from these to the polyterpenes and sterols. Recently the study of the chemical structure of gland extracts, vitamins, sex hormones, and carcinogenic bodies has brought an astonishing array of substances intimately connected with the control of vital processes within the scope of what is termed the isoprene rule, which was first suggested by O. Wallach, who as a result of his researches on terpenes found that they could be represented structurally as built up of isoprene units joined by their unlike ends.

In addition to this series of substances, there are many open chained compounds such as squalene [11] which are found in the liver oils of elasmobranch fishes, phytol, an alcohol related to chlorophyl, and various colouring matters such as crocine, which may also be considered to be built up of isoprene units, bixin and the carotenes [12]. These researches are of great interest not only for the light they throw upon the structure of resinous plant products, but still more for the impetus which they give to study of the processes by which they are formed in the plant. In this respect, the study of abietic acid upon which organic chemists have spent so much labour, though of great value, is apt to be misleading unless it is realised that the acid does not occur in the original oleo resin but is produced during the distillation of the oleo-resin on an industrial scale for the production of resin and turpentine. As a result of numerous researches, the formula of abietic acid seems to have been firmly established as (I) [13]. In the oleo-resin itself are found two acids rather unfortunately named d-pimaric (II) and l-pimaric acid (III), on account of their respective optical rotations, though in fact they are structurally very different. d-Pimaric acid is stable to heat and structurally different from abietic acid. The other acid, l-pimaric, is unstable, and though its structure has not yet been definitely established, it is related to that of abietic acid. Various acids generally designated " sapinic acids " have been isolated from the oleo-resins of other pines, and there is much to be said for the suggestion that l-pimaric acid should be renamed l-sapietic acid [15]. A number of these acids have

been examined particularly by G. Dupont and his colleagues of L'Institut Coloniale de Marseilles [16].
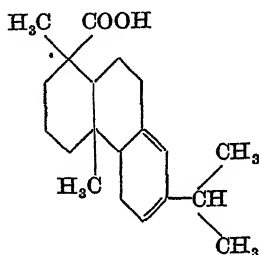
(I) Abietic Acid
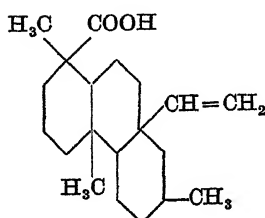
(II) d-pimaric Acid
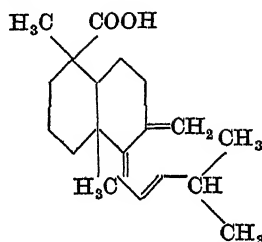
(III) l-pimaric Acid (suggested by Kraft)

The sapic acid which Vocke [14] crystallised from American oleo-resin had the same molecular formula ($C_{20}H_{30}O_2$) as abietic acid and was apparently identical with the corresponding acid obtained from European Pine Resin. An interesting property of this acid was that on irradiation it was changed from a crystalline to a non-crystalline substance of much lower optical rotation. As to the formation of these acids, Dupont [17] elaborated an interesting theory. Starting with the aldehydic substance isolated by Kohler [18] from the Red Pine to which the formula $C_9H_{15}CHO$ was given, it would be easy to account for the formation of both resin acids and terpenes by diastatic action and oxidation. The composition of the oleo-resin from the Maritime Pine was found to agree closely as regards the ratio of acids to terpenes with the formula calculated from the chemical equations. Further, if it were assumed that the parent substances of the two terpenes present in the essential oil were also the parent substances of the acids in the non-volatile portion, the calculated composition agreed very closely with that obtained in practice.

Turning to the more purely chemical theories of resin formation, the most interesting scheme is that devised by Read [19], in which geraniol is taken as the starting-point. The constitution of this

terpene is remarkable in that the molecule may be considered as consisting of two portions, each of which is highly reactive. By a series of oxidations, hydrations and dehydrations together with the wandering of a hydrogen atom in ways which are known to be quite within the bounds of possibility, a series of formulæ can be devised which will account for the formation of a large number of the terpenes found in association with resin acids.

A synthetic scheme advanced by Stewart [20] is of interest as it included the sugar apiose and thus brings the carbohydrate and terpenes into the same scheme. The relations noted in the production of tannins, starches and terpenes are therefore of much interest [27, 28]. It is possible that all these schemes and probably many others are represented in nature, for it is clear that the production of any particular compound or series of compounds depends upon a very delicate balance of forces.

The ability of a reaction to proceed along numerous lines is one of the predisposing conditions to resin formation.

On the other hand, the formula devised by Grün [22] and by Virtanen [21], based mainly upon botanical and biochemical considerations, is not to be ignored. For in the living organisms we are dealing with a dynamic rather than a static condition. It would seem that Nature's methods are not so much a matter of " opening a door with a key " in contrast to the drastic methods of the laboratory, but rather that Nature never allows the door to be locked. In the last edition of his book, Tschirch [23] reaffirms his belief that the seat of activity in the plant is an intra-cellular substance or membrane and here operate the forces such as surface tension, electro-osmosis, and so forth, which effect and control the chemical changes in the plant. " The problem of resin formation is therefore botanical, biological and phyto-chemical, and cannot be solved by either purely botanical or purely chemical means." This may, at first sight, seem to savour of the old theory of vitalism, but at the same time it does state the position with at least a very high degree of completeness. The chemist is becoming more and more inclined to admit that in the living organism the atoms are bound together with far less rigidity than he is accustomed to assume in dealing with the products obtained in the laboratory in the course of a scheme of synthesis.

It seems clear that so far as the acid constituents of the oleoresin are concerned, the condition is one of what may be described as dynamic equilibrium, a large number of more or less stable groupings being present, which on removal from the tree and subjected to chemical, or even merely physical, action, resolve them-

selves into isomeric forms of definite structure. Abietic acid, the product of very drastic heat treatment, may be regarded as a particularly well-defined stage of stability and is probably a mixture of isomers. As to how the plant manufactures these resin acids, it must be admitted that a good deal of speculation enters into the various theories propounded. This must necessarily be the case until the other constituents of the oleo resins are more fully understood.

The wide ramifications and persistence of the "isoprene rule" in biological synthesis must not be allowed to become an obsession. Apart from substances of a terpene character, phenolic substance and oxygenated bodies are frequently, if not invariably, associated with natural resins. In some cases, as for example the Xanthorrhea resins (accroides), they form a large proportion of the resin. R. D. Haworth [13], whose brilliant syntheses of phenanthrene derivatives have done much to place the constitution of the pine resin acids on a firm foundation, has drawn attention to the biogentic significance of a series of lactone compounds of the type IV [24], which are not amenable to the isoprene rule. Compounds of this type have been isolated from various species of Coniferæ, though not from Pinus species.

(IV)
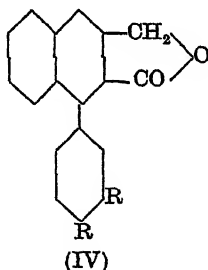
At the present time, the neutral resinous substances which occur in all natural resins, varying from 5 to 10 per cent. in Pinus oleoresin, to 80 per cent. in the damar resins, are simply labelled resenes and our almost complete ignorance of the structure admitted. Apparently they are not all of one kind as it is possible to separate at least two different kinds from many resins, the difference being in solubility and melting-point. In the case of Kauri, J. R. Hosking [25] found that the resenes did not appear to be formed from the acids, as the total resene content remained practically constant at about 10 per cent. in resins of widely different ages. This figure seems to be a fairly general one in the case of the resins from Coniferæ. Tschirch has suggested that the resenes may be oxygenated polyterpenes. Recently, Hosking [26] and his colleagues have

isolated from the resins from Dacrydium species three oxygenated diterpene derivatives which are of interest as showing that this type of compound does exist in nature (V). The formulæ for these is also of great interest as they are similar to that of sclareol [29]. There are also similarities with agathic acid (VI) and Vitamin A (VII).
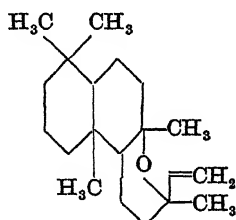
(V) Manoyl Oxide
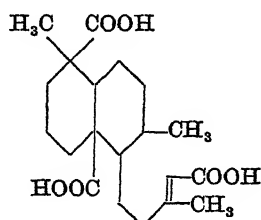
(VI) Agathic Acid
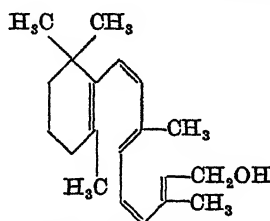
(VII) Vitamin A

Apart from any effect which the knowledge of the structure of these substances might have upon our theories of resin formation, they form a very serious obstacle to the study of the more reactive ingredients. Their presence even in small quantities greatly affects the reactivity and physical properties of the acidic ingredients and the colloidal character of the resins as a whole, and their sensitiveness to even the mildest form of treatment renders the isolation of the unchanged chemical entities of the resin a difficult if not impossible task. It is easy to cast aspersions on the chemical individuality of the " acids " which Tschirch so laboriously isolated from a vast array of natural resins, but it is a more profitable attitude to realise the limitations of purely chemical methods and study more carefully the nature of the complexities introduced by what may be termed colloidal phenomena. For example, Tschirch's basic method of attack consisted in dissolving the resin as completely as possible in an organic solvent and then extracting the solution with dilute alkaline aqueous solutions. In this way, a series of acidic substances was extracted which could sometimes be further separated by the difference in solubility of their lead salts and similar standard methods.

The process was incredibly laborious, hundreds of extractions sometimes being necessary. More recent work has shown that much greater amounts of substance can be extracted much more rapidly by simply interposing washings with water.

Evidently separation in this case is not the simple straightforward chemical process as it is in the case of separating fatty acids from their glycerides. Partition coefficients, interfacial tension and hydrogen-ion concentration, to mention only the more obvious factors, are evidently involved. These of course play their part even in the simplest chemical reaction, but their effect is small and often negligible. In dealing with such substances as resins in which the colloidal properties predominate, they become all important. This is not to disparage the value of purely chemical work. It has been wisely said that the more a colloidal substance is studied the less colloidal it becomes, but the effects of colloidal forces upon chemical reaction must be understood and due allowance made for them before the chemical results can be rightly interpreted. Even in the case of a synthetic resin produced from simple and well-defined chemical bodies such as phenol, phthalic acid, glycerine and formaldehyde, the process of resinification soon reaches a stage at which the separation of a definite chemical entity becomes impossible except for traces of the original components that may have escaped combination. Indeed, on the whole, the chemist has probably got more information out of the natural resins than he would ever have done out of the synthetic resins had he been confronted with them without any knowledge as to how they had been produced.

Possibly some of the compounds isolated from natural resins must be regarded in much the same way as the small amounts of pure chemical substances which can be isolated from the synthetic resins—traces of original substances or the products of incomplete or secondary reaction—valuable indications of the nature and origin of the resin but not necessarily representative of the structure as a whole.

With an increasing knowledge of colloidal phenomena, and a more elastic conception of valency in organic compounds, the development of a coherent theory of resin formation which will be useful, both from the scientific and economic point of view, is quite within the bounds of possibility, and enough has been said to show that the subject abounds in interesting problems worthy of the attention of the chemist, no matter in what branch of chemistry he may have specialised.

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THE PHYSICAL PROPERTIES OF DISPERSE SYSTEMS

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SINCE quite bulky books have been written under a title similar to that which heads this article, and since there is almost no limit to the systems which can be classed under the denomination of " colloid "—was it not the late Professor Ostwald who, asked to define a colloid, pointed to a bottle of freshly distilled water and said, " That, so far as I know, is not a colloid " ?—it may seem rather presumptuous of the author to attempt a short article on the subject. He intends, however, to restrict the scope of the article rather closely, and deal with recent work on those systems in which discrete lumps of material are dispersed in a fluid of continuous structure, either gaseous (aerosols) or liquid (hydrosols). The subject is topical in as much as two conferences have recently been held in this country which dealt with some important aspects of these two types, *i.e.* the International Congress on Soil Science (Oxford, August, 1935) and the Faraday Society's Conference on Disperse Systems in Gases (Leeds, April, 1936). Nevertheless, it is not proposed to limit this article to matters discussed at these conferences. Much of the research work done on disperse systems in recent years represents an effective co-operation between the physicist and the technological expert, and we shall have to deal with branches of applied physics as diverse as agriculture, river engineering, meteorology, ceramics and the manufacture of pickles !

The information which is sought from the physicist is mainly of two types. One set concerns the degree of dispersion,—how the matter in suspension is graded for size, and what factors are likely to change the size frequency—and the other concerns the interaction of the continuous and discrete phases, and it will be convenient to group our subject-matter under these heads.

1. MECHANICAL ANALYSIS

One of the most important processes to which the physicist submits a suspension is that by which he determines the relative numbers of particles falling within certain limits. This, for instance, is a dominating factor in determining that elusive quality of the soil which the farmer calls tilth ; it tells the mining engineer whether or no a specimen of coal dust is likely to explode ; in the ceramic industry, it serves to differentiate those clays which are suitable for making such diverse products as face powder and paper ; an analysis of this type applied to wind-borne dust and river-borne silt in cities may enable the origins of possible pollution to be traced, while to the geologist it forms the foundation of the science of pedology, *i.e.* the classification of soil types. A rather uncommon application of the process was recently made to the soil at the earthwork fort made by the Romans on the Wall at Birdoswald in Cumberland. For a short distance the fort was accompanied by a diversion of the north bank of the ditch, the line of which was intercepted by foundations of the fort. It was a question whether the ditch and fort were contemporaneous or whether part of the ditch within the diversion had been filled in when the fort was subsequently constructed. In the former case the north bank within the diversion would be undisturbed soil ; in the latter it would have presumably the same composition as that of some other certainly artificial works in the interior of the fort. This latter

FIG. 1.—Mechanical analysis by sedimentation.

hypothesis was, in fact, found to fit the case, and the deduction could be confirmed by archæological evidence, although negative evidence—dissimilarity between the suspect soil and the man-handled soil—would not, of course, have been conclusive for the final line of the ditch wall being the original one.

Now to describe the method by which a mechanical analysis of a hydrosol is made. The particles are allowed to settle under gravity in a tall cylinder and it is assumed that they do so under Stokes' law, *i.e.* that they shortly attain a terminal velocity proportional to the square of their diameters, d. Into the validity of

this assumption we cannot enter here. If now a " sample " is taken at a depth h after a time t from the commencement of sedimentation (Fig. 1), the " weight "—using the word in no literal sense—of the sample will be proportional to the number of particles, n, in the suspension having velocities $<h/t$ *i.e.* to $\Sigma n d^2$, where the summation extends from particles so small as to remain permanently in suspension up to those having the given terminal velocity. If the sampling is continuous the curve so obtained is a summation curve (Fig. 2—continuous line), whose slope plotted against $1/d^2$ (Fig. 2—dotted line) gives the required frequency : size curve. The two curves are related in the same way as the summation and
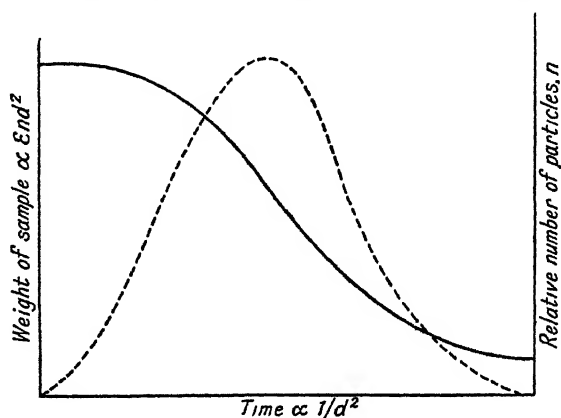
FIG. 2.—Size-frequency analysis of a disperse system. Typical summation (continuous line) and distribution (dotted line) curves.

distribution curves of statistics, and in simple types, the second curve bears a close resemblance to the normal error curve of Gauss.

Many methods have been devised to do the necessary sampling. Pipette or hydrometer insertions at intervals give the curve step by step. In Oden's [1] method the falling particles are caught on a balance pan in the sedimentation tank, while in the method recently devised by the writer [2], a beam of light falling on a photo-electric cell does the sampling, the " weight " of the sample being obtained from the light absorbed in the tank. These two methods are apparently the only ones which delineate the summation curves continuously and automatically. Whether the sedimentation takes place in air or in water, convection currents must be prevented.

2. Coagulation

As the majority of the particles in the systems which we are considering will be too large to be affected by Brownian motion,

the collisions which may produce cohesion between neighbours must be looked for in the relative motion under gravity of particles of various sizes through the continuous medium.   When the relative density of the two phases is large, as in most dust clouds, the suspension can only be maintained by convection currents or turbulent draughts.   Although such conditions will induce frequent collisions, it is not likely that coagulation of the solid phase will ensue, unless the particles are insulated oppositely charged bodies.   Some particles are " dipoles " and such will form chain aggregates.   When the disperse phase is liquid, as in fogs and emulsions, it is generally assumed that collisions will result in coalescence.   There is, further, the well-known tendency, even under static conditions, for a small drop to distil into a larger one, which will eventually equalise the sizes of drops in such systems.   Although a fog commences by the condensation of water vapour on hygroscopic particles of ultra-microscopic size (mostly sulphur dioxide nuclei), these soon grow to microscopic proportions forming eventually a system having a peaked size-frequency curve with a maximum in the neighbour-hood of 10μ.   During growth the system will show selective absorp-tion—hence the familiar coloured sun seen through a mist—but as an adult fog it will absorb all wave-lengths equally and, inci-dentally, monochromatic vehicle lights will penetrate it no more effectively than white ones.

The stabilisation of emulsions is a matter of some importance in the preparation of a number of foods and medicines.   Analogous conditions are found in paint and colour manufacture, *viz.* a disperse phase, which, solid or liquid, though having a small density relative to the oil in which it is dispersed, and although continuing suspension is favoured by the relatively high viscosity of the oil, eventually settles out and coalesces.   To avoid the injunction " shake the bottle," and perhaps also to satisfy the customer's æsthetic sense, purveyors of commercial emulsions must endeavour to incorporate a stabilising agent in their products.   The precise mechanism by which the emulsifying agent operates still seems uncertain, but evidently belongs to the domain of electro-chemistry.   In synthetic emulsions the phases are separated by agitation (mechanical or thermal) or by an " atomiser " in which the dispersion takes place by the injection of a jet of one liquid which breaks up into drops in the other liquid, in the same fashion that water from a tap breaks into drops in the air.   The fact that the disruptive action of super-sonic vibrations of large amplitude is capable of producing an emulsion at the surface of separation between oil and water was one of the most striking observations made by Wood and Loomis

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[3]. If a supersonic quartz generator is operated with a power of some hundreds of watts beneath the surface of oil, on the top of which a layer of water has been placed, rapid dispersion takes place having its origin at the interface. It should be pointed out, however, that there is no reason to suppose that the effect is limited to the supersonic frequency region or that a sonic generator of sufficient power would not have the same effect, except that the former agency may " cavitate " the liquids and so hasten dispersion. To accelerate the natural rate of coagulation of disperse systems, a number of alternative processes may be employed. Since the particles are generally electrically charged, they can be driven together by an electrical field either direct or alternating, a process first developed industrially by Lodge [4] for precipitating dust and smoke. By a somewhat similar process the dispersed globules in an emulsion may be " salted out " by adding an electrolyte producing ions of the opposite sign to that of the prevailing charge on the drops. This upsets the stability of the emulsion, causing coalescence of the droplets on collision. Particles may be herded together, ultimately to produce aggregates, by the action of light or X-rays (photophoresis), although this effect is small on the larger particles.

The fact that large particles are drawn together in the nodes of stationary sound waves has been known since Kundt devised his famous " tube," but recently a number of papers have been devoted to ad hoc studies of the coagulation of fog, dust and smoke by sound and supersonics. Since the latter under suitable circumstances induce dispersion, it is obvious that two opposing effects are at work here. Söllner and Bondy [5] have in fact found a critical intensity in the irradiation of two contingent liquids by supersonics, at which the rate of emulsification balances the re-aggregation of the oil drops. The work of Brandt and Freund [6] and of Gorbatschew and Severny [7] on aerosols indicates that for a given concentration there is a certain frequency at which the rate of coagulation is a maximum. The last-named authors and also Andrade [8] have advanced theories of the phenomenon based on the action between particles in a sound wave (although the former appear to be in error in supposing that at such high frequencies the particles can follow the alternating motion of the vibration, and attract each other in accordance with the so-called Bjerknes effect). It appears to the writer that beside a most favourable frequency, there must be a most favourable amplitude for coagulation, as too great an amplitude will encourage greater dispersion. No doubt this point will be cleared up in further experiments.

One feels that, in spite of much work in the laboratory, there is at present little expectation of being able to precipitate or remove fog and dust once they have been produced or liberated in the atmosphere. Occasional attempts to clear a local region are reported, e g. recently sufficient space was cleared of fog for the safe landing of an aeroplane in the U.S.A. by the burning of two 50-gallon barrels of petrol on the landing ground, but at the present time we must pin our faith on the excellent work of the Atmospheric Pollution Committee in encouraging the abatement of smoke and dust at or near the source.

3. RELATIONS BETWEEN THE DISPERSOID AND THE CONTINUOUS MEDIUM

A. *Static Relations*

The tenacity with which the particles in suspension cling to the fluid permeating their interspace may be estimated by measuring the quantity of the latter which can be squeezed out from a " saturated " suspension when a pressure gradient is applied to it. The ultimate limit to such removal appears to be the Langmuir layer of fluid particles which surround a solid immersed in a fluid, but in the aerosols there is no difficulty in producing what amounts to a vacuum between the solid particles. In the hydrosols, however, capillary attraction resists the removal of the liquid medium and in a tightly packed arrangement of particles such as the soil may raise the liquid against gravity to a considerable height above the water table formed over an impermeable subsoil. In the hydrophilic sols there is still a not inconsiderable residue of liquid adsorbed by the solid phase even when sufficient suction has been applied to overcome normal surface tension. Such adsorption seems to be chemical in nature. This is shown when the continuous medium is electrically charged. So it requires less suction to remove the final traces of alcohol from a suspension of soil particles in alcohol than of water from a suspension of soil in water.

Schofield [9] has shown an interesting relation between the moisture content of the soil and the hydrostatic suction against which it is maintained—expressed as the common logarithm of the height of the water column producing the suction. This latter quantity—by analogy with the well-known symbol for hydrogen-ion concentration—he labels pF. The pF curve for the removal of moisture lies above that for re-wetting, so that the two processes form a hysteresis cycle (Fig. 3). The lower curve here is obtained from experiments in which water in a porous pot is earthed in the

dry soil, which is allowed to take up moisture against the pressure of a mercury column connected between the pot and the atmosphere, while the upper curve may be obtained from experiments in which a suction pump is connected to the specimen or from measurements of vapour pressure and freezing-point determinations. The steepness of the curves at the top indicates the tenacity with which the soil clings to small traces of moisture, which cannot be used by plants. As atmospheric pressure represents a column of water 1000 cm. high, it corresponds to a pF of 3, and at this point the moisture content in equilibrium with the soil is 10 to 25 per cent., depending on whether it is damping or drying out. Oven dryness corresponds to a pF of 7—a water column higher than Mount Everest !—while the maximum saturation corresponding to a complete filling of the interspace is equivalent to 45 per cent. of the total volume and occurs at $pF = 0$. Barkas [10] has demonstrated—by means of vapour pressure curves—that a similar hysteresis exists in the cell-water relationships of wood or coal. The effect here is more complicated in that the cell wall may be saturated but the cell cavities empty, but in the main the behaviour of wood follows in this respect that of the soil which we have outlined above.

FIG. 3.—Height of Water Column (plotted logarithmically as pF) in equilibrium with soil at various moisture contents (after Schofield).

A gradient of pF through such a disperse system will then constitute a potential gradient tending to drive the continuous medium from a place where it is in equilibrium with the pressure to one where it is unsaturated. This occurs on a large scale in the ground when heavy rain follows drought. The actual movement is of course also governed by the prevailing gravitation potential, but the movement of water through an experimental plot of land can be studied as a problem in diffusion, as Childs [11] has recently shown. Other investigators have endeavoured to predict the water-holding capacity of a disperse system—idealised as a pile of closely packed spheres—in terms of the surface tensions over films of fluid occupying the interspaces.

The other factor controlling the rate of run-off of the continuous from the discrete substance is of course the hydrodynamic resistance. Here again, although much work has been done by the drainage engineers, assisted by empirical theories, little fundamental experimentation can be noted. Green and Ampt [12] some years back measured the rate of percolation of air and water through soil specimens and also through agglomerates of tiny glass spheres, the dispersoid in the latter instance being in fact the " glistening dew " with which the Christmas card manufacturer simulates snow. As an assumption of Poiseuille's law produced a viscosity coefficient— or permeability, as they preferred to call it—which was constant, it may be presumed that the system may be regarded in these experiments as a solid with capillary tubes bored through it. The exact relation between the particle size and the diameter of these imaginary capillaries is less certain.

Another interesting example of a disperse system determined by the static equilibrium between two liquid systems is that of the coarse emulsion, since removal or disturbance of the continuous medium may result in the coalescence of the disperse globules and the end of the emulsion, as such. The conservative force in an emulsion is surface tension which tends to keep the dispersoid in spherical detachments ; the disruptive forces are gravity and viscous shearing. These forces fill the same rôles in the breaking up of a jet of water into spray. G. I. Taylor [13] has recently investigated the relative effects of surface tension and viscosity in determining the deformation of spherelets of oil in syrup into lenticules, under the action of suitable shearing forces, and the subsequent disruption and dispersion into smaller masses of oil. This question of the stability of emulsions touches a number of present-day manufactures very closely. The preparation of such diverse substances as ice-cream, margarine and mayonnaise involves the successful stabilisation of the emulsions involved, usually assisted by chemical agencies.

B. *Dynamic Relations*

From the physical point of view, the most striking differences between the behaviour of homogeneous and disperse systems occur when they are subjected to forces which tend to move the system bodily. The shearing forces between the two phases which then occur give rise to " anomalous flow," of which the main characteristic is a viscosity, as defined by the usual Newtonian formula, which decreases with increasing rate of shear instead of being constant for all rates of shear as in a homogeneous fluid. One result of this is that, whereas at sufficiently low velocities the distribution of

velocity across a circular tube in which a homogeneous fluid moves is parabolic (Poiseuille), that of a disperse system approaches more closely to plug-flow (Bingham), in which the velocity is more or less uniform over a large part of the section, particularly when the concentration of the continuous phase is reduced (Fig. 4). It is a moot point whether the term " viscosity " should still be used for such systems—some writers prefer " fluidity " or " plasticity " —at any rate it is hopeless to calculate a " coefficient of viscosity " for a disperse system placed in the usual types of viscometer employed for homogeneous systems since the rate of shear varies across the fluid contained in such instruments. By using an instrument in which the velocity gradient can be measured from point to point across the direction of flow, such as a hot-wire anemometer, it is possible to correlate this gradient with the rate of shear at
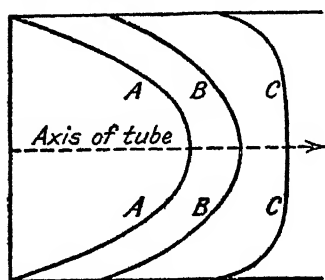
FIG. 4.—Flow of disperse system in tube. A, B, C represent stages of increasing concentration of dispersoid.

each point and so calculate the coefficient of viscosity of a suspension such as clay in water as a function of the rate of shear [14]. As, however, up to the present the industrial chemist and physicist have continued to use the older type of apparatus, such results have interest only for the pure physicist. Indeed, it is difficult to see how a knowledge of this variable viscosity is to be applied in industrial processes where such variations cannot be allowed for. The position is analogous to that in industrial acoustics, where a knowledge of the undoubted variation of sound absorption in materials with pitch is useless to the architect who has to build a concert hall to suit the whole gamut of musical frequencies.

. Similar variations may be noted in systems containing two liquids as the phases, viz. emulsions, or liquid and gas, viz. froths. The latter form the subject of an interesting set of experiments by Sibree [15]. The viscosity of aerosols has been little studied as it is difficult to get such systems to flow bodily, without the dispersed particles coalescing or impeding each other's motion when solid friction intervenes. Einstein [16] has treated the viscosity of a system consisting of solid spheres suspended in a liquid and deduced theoretically that the viscosity grows in linear fashion with the concentration of the spheres by volume, and his formula has been modified by G. I. Taylor [17] to suit an emulsion, in which shearing forces between the phases produce internal motion in the

spherical droplets. The experiments of Bond [18], however, on the rate of fall of single drops in another liquid show that the drops behave as rigid spheres unless the square of their radius exceeds a critical value, determined by the quotient of the surface tension by the difference of specific gravity. As most emulsions require a small difference of density for stability in these systems, the disperse phase behaves like a rigid solid and the observed viscosity falls under the Einstein formula. Owing, however, to the anomalous variation of viscosity with velocity gradient mentioned above, an experimental test of the Einstein formula or any other based on classical hydrodynamics, which takes no account of this variation, is difficult. The writer [19] has proposed an alternative formula to explain the variation of mean viscosity with concentration, for which he has endeavoured to give a theoretical justification, and which covers the discrepancies observed when the viscosities of concentrated emulsions are compared with Einstein's formula. The author's formula indicates an exponential variation of viscosity with the volume of the disperse phase (thus falling into line with the expressions previously put forward for the variation of viscosity of homogeneous liquids with temperature), and passing over into the Einstein formula when the concentration of the disperse phase is low.

Some interesting and special effects are observed when a suspension sufficiently concentrated to exhibit " stiffness " and " stickiness " is sheared. Such systems often exhibit a quality called by the colloid chemists " thixotropy," which covers a number of phenomena closely related to fatigue in metals. There is both a time lag in the taking up of the shear by more distant parts of the substance and a behaviour characteristic of its previous history. Thus when clay is " puddled " in water to form a 50–50 suspension in a concentric cylinder viscometer and the outer cylinder rotated slowly with increasing speed, the inner cylinder first twists considerably against the torsion control and then suddenly relaxes to a smaller twist before steadily increasing again (Fig. 5—continuous line). As the speed is reduced the twist is gradually and continuously released (broken line), forming a hysteresis loop [14]. The relaxation of such systems is often encouraged by agitation either at low frequencies or by supersonic means. Baker's dough in the hands of Schofield, Scott-Blair and Halton [20] has proved an admirable medium for the exhibition of relaxation. In such a material the relaxation is best defined by the Maxwellian conception of a product of relaxation-time and rigidity-modulus which product has the same dimensions as viscosity. They measured the rigidity of a roll of

dough in a plastometer, in which one end of the roll was twisted
while the movement of the other end was noted both in extent
and time.   The viscosity was measured by the stretch of a roll
under elongating stress, the stress being removed after five minutes
and the strain not recorded until relaxation was finished—this took
several minutes.   The ratio of viscosity to rigidity is the most
important criterion of the quality of the flour for breadmaking, as
a high value means that the dough has a good " spring," the quality
which the baker tests less accurately by pulling a piece between
his fingers.   The same workers have been able to reduce the " short-
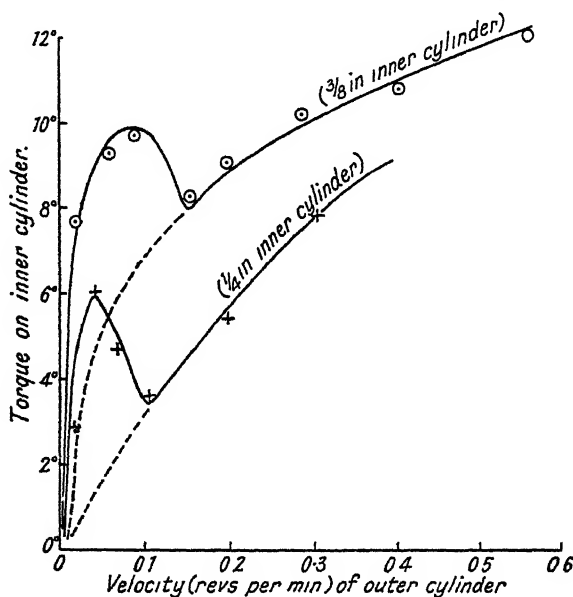
Fig. 5.—Thixotropic behaviour of clay-water suspension (from *J. Agric. Science*,
23, 182, 1933).

ness " of bread to physical dimensions.   This susceptibility to
rupture under stress is mainly dependent on the rate at which the
viscosity falls with increasing stress, and an instrument which can
measure this is found to be much more consistent in its estimate of
" shortness " than the baker's efforts to tell it by feel.

Thixotropy of the coarse suspensions is often due to admixture
of colloidal particles.   Such a system will exhibit a low viscosity,
viz. small resistance to shear if continuously agitated, whereas if
kept undisturbed it will set firm.   According to Freundlich and
Juliusberger [21], it is the admixture of a small percentage—2 per
cent. suffices—of clay which is characteristic of quicksands, which

are markedly thixotropic. Removal of the clay consolidates the sand and makes it able to resist the effects of agitation.

In technology, the study of the flow of coarse suspensions of solids in water is of extreme importance. Problems involving the deposition of silt, and its converse—erosion—have occupied the minds of harbour and river engineers for many years, and although we are not cursed in this country with such incorrigible rivers as those of the European and American continents, the silt problem is now coming to the fore in connection with the new hydro-electric schemes in Scotland. Other aspects of this problem concern the farmer, and sometimes it is the wind and not water which causes the erosion. It was reported in 1935 that a severe gale removed vast quantities of the loose and valuable topsoil of the Lincolnshire market gardens in a single day. The problem—of both wind and water erosion—is only now being put on a fundamental basis, although a great deal of empirical and disconnected information is available. It is known, for instance, that an uninterrupted run-off on a steep gradient favours erosion, a slow stream and much obstruction favours silt deposition. The method adopted by the U.S. Soil Conservation Service to prevent erosion, where excessive cropping or firing of the vegetation on a hill slope has accelerated the run-off of water, is to terrace the hillside with excavations parallel to the contour lines. Although engineers are fond of describing the silt-carrying capacity of a stream in terms of the mean velocity and the cross-section (which bear little relation *per se* to the quantity carried in suspension), it is obvious that what really determines the pick-up from the bed is the vertical gradient of velocity near the bed, and one needs to correlate the velocity distribution with the silt distribution over a vertical section of the stream, carrying spherical grains of uniform size. The only experiments in which this correlation has been attempted are those of the writer, who used a small open water channel, 150 cm. long, 5 cm. wide, having a polished wooden floor and glass sides. The bed of silt (sifted sand or fuller's earth) was laid in a shallow trough near the mid-point of the length of the channel. At a section downstream of this the vertical silt-distribution was measured by a beam of light and photo-electric cell; and the velocity-gradient by means of a calibrated hot-wire anemometer. Fig. 6 shows a typical set of records (silt-density : continuous lines, velocity : broken lines). The mean density of silting is roughly proportional to the velocity-gradient at the bed, although when the flow becomes completely turbulent (top lines) the distribution of silt with height becomes markedly exponential. As to a theory of silt transport, it seems

best to apply the diffusion equation to this problem, at any rate when the motion is turbulent, as Schmidt [23] first suggested. We should then write $\dfrac{\partial s}{\partial t} = \dfrac{\varepsilon}{\rho}\dfrac{\partial^2 s}{\partial h^2}$, where s is the property transported—in this case, silt-density—ε is the *austausch* or pick-up coefficient, ρ the density of the fluid. If ε can be considered a constant, independent of velocity-gradient—an assumption which is doubtfully justified by the experiments—and v is the velocity of free-fall of
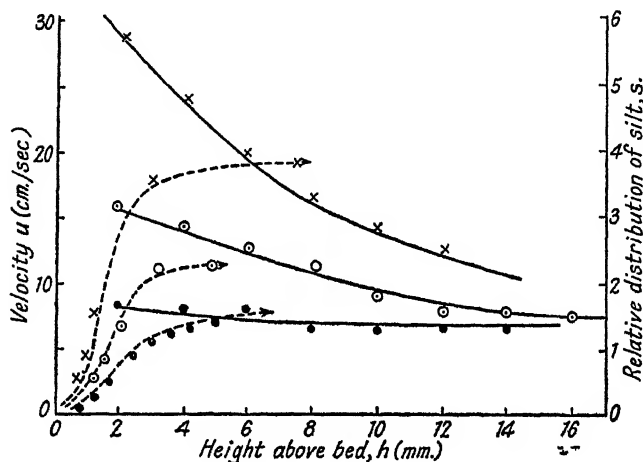
Fig. 6.—Silt-distribution and velocity-gradients in water channels (from *Phil. Mag.*, **17**, 776, 1934).

the silt in the fluid (by Stokes' law), we can write for the supply and withdrawal of silt from any section

$$vs + \varepsilon\ \partial s/\partial h = 0$$

of which the solution corresponds to the exponential distribution of s with h, noticed above.

Further experiments are required to establish the validity of Schmidt's or any alternative theory, but it may be said in conclusion that quite a number of natural phenomena involve transport of some quantity by turbulent mixing. Other cases are (1) the diffusion of a dye or soluble salt from the bed of a stream, (2) of smoke from a chimney into the atmosphere, (3) of sand into the air during a sandstorm, (4) of heat from an aeroplane radiator, (5) of seeds from deciduous plants, (6) of plankton in a turbulent ocean—provided that the effects of light and temperature are duly accounted for in the last instance.

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RECENT ADVANCES IN SCIENCE

MATHEMATICS. By J. H. C. Whitehead, M.A., Balliol College, Oxford.

Combinatorial Analysis Situs, II.—The first of these two articles contained a brief description of combinatorial complexes. For many purposes it is convenient to think of the cells in a complex as simplexes, and in this article we shall be concerned with what are called *simplicial complexes*.

By an *n-dimensional simplex*, or *n-simplex*, is meant a region in n-dimensional Euclidean space which is given by

$$0 \leq x_i \leq 1, \ 0 \leq x_1 + \ldots + x_n \leq 1$$

in a suitable cartesian, not necessarily rectangular, co-ordinate system. The virtue of a simplex lies chiefly in the fact that it is completely determined by its vertices. An n-simplex has $n + 1$ vertices, say $a_0, a_1, \ldots, a_n$, and may be written $a_0 a_1 \ldots a_n$. The boundary of $a_0 a_1 \ldots a_n$, which we denote by $\beta(a_0 a_1 \ldots a_n)$, is the set of simplexes $a_1 \ldots a_n$, $a_0 a_2 \ldots a_n$, $\ldots$, obtained by omitting the letters a_λ, one at a time. Thus the boundary of the 1-simplex ab consists of the pair of 0-simplexes a and b (it is conventional to describe a single point, or vertex, as a 0-simplex), the boundaries of the two- and three-dimensional simplexes abc and $abcd$ consist of the three 1-simplexes bc, ca, ab and of the four 2-simplexes bcd, acd, abd, abc respectively.

If $a_{i_0}, a_{i_1}, \ldots, a_{i_n}$ are the vertices $a_0, a_1, \ldots, a_n$ in any order we say that $a_{i_0} a_{i_1} \ldots a_{i_n}$ is the same *oriented* simplex as $a_0 a_1 \ldots a_n$, provided

$$\begin{pmatrix} 0, & 1, & \ldots, & n \\ i_0, & i_1, & \ldots, & i_n \end{pmatrix}$$

is an even permutation, and the same simplex with the opposite orientation if it is an odd permutation. If A is an oriented simplex we denote the same simplex with the opposite orientation by $-A$. If we think of $a_0 a_1 \ldots a_n$ as an algebraic product in the symbols $a_0, a_1, \ldots, a_n$, this follows from the rule

$$a_i a_j = - a_j a_i$$

for interchanging the order of multiplication. The boundary of an oriented n-simplex $a_0a_1 \ldots a_n$ consists of the oriented $(n-1)$-simplexes

$$(-1)^i a_0 \ldots a_{i-1}a_{i+1} \ldots a_n \quad (i = 0, 1, \ldots n),$$

and the machinery described in the first article can now be applied to the chains and cycles associated with any complex of which the cells are simplexes.

The systematic use of simplexes is particularly convenient in the theory of combinatorial equivalence. In discussing the latter we shall abandon the concept of orientation and a simplex will be a product $a_0a_1 \ldots a_n$ with

$$a_i a_i = a_j a_i.$$

A (simplicial) complex is a collection of simplexes which we write as a sum

$$K = \Sigma a_{i_0}a_{i_1} \ldots a_{i_n},$$

in which the vertices (*i.e.* the symbols a_0, a_1, ..., a_n, ... appearing in K) can be manipulated according to the following rules:

1. $K + K' = K' + K$
2. $xK + yK = (x + y)K$
3. $x + x = 0$
4. $xx = 0$,

where K and K' stand for any complexes, x and y for any vertices, and 0 is a symbol with the properties $K + 0 = 0 + K = K$, $0.K = K.0 = 0$. A simplex in K which is not on the boundary of any other simplex in K is called a *principal* simplex of K and it is to be assumed that no principal simplex occurs more than once in the symbolic expression for K.

If x is any vertex in K we can write

$$K = xL + M,$$

where the complex M does not contain x. We may suppose that no term in L contains x. For if

$$L = xP + Q$$

we should have

$$xL = xxP + Q$$
$$= xQ$$

since $xx = 0$, $0.P = 0$ and $0 + Q = Q$. Thus L may be written symbolically as $\partial K/\partial x$ and is called the *complement* of x in K. The boundary βK of K is the sum of the boundaries of all the principal simplexes in K. If $\beta K = 0$ we describe K as *unbounded*, and as *bounded* if $\beta K \neq 0$. If K is finite, that is to say contains

but a finite number of simplexes, and unbounded, it is often described as a *closed* complex. In the remainder of this article it is to be assumed that we are dealing with finite complexes. The boundary of any simplex $a_0 a_1 \ldots a_n$ is given by

$$\left(\frac{\partial}{\partial a_0} + \ldots + \frac{\partial}{\partial a_n} \right) a_0 a_1 \ldots a_n$$

and it follows that

$$K = \left(\sum\nolimits' \frac{\partial}{\partial a_i} \right) K,$$

where the summation extends over all the vertices which occur in K. Therefore we have the rule

$$\beta(K.K') = K.(\beta K') + (\beta K).K'$$

for finding the boundary of any complex which is expressed as the product of two complexes. Our formalism demands that we introduce a symbol 1, the conventional boundary of a single vertex, such that $1.K = K.1 = K$, which we accordingly do.

If the vertex x occurs in βK it is called a boundary vertex and otherwise it is called an internal vertex. It is an internal vertex if, and only if, $L = \partial K / \partial x$ is closed. For

$$\beta K = \beta(xL + M)$$
$$= x.\beta L + L + \beta M.$$

Thus x is absent from βK if, and only if, $\beta L = 0$.

The theory of combinatorial equivalence is based upon the notion of sub-division. Consider, for example, a complex K, in which all the principal simplexes are three-dimensional. We can represent K by a model consisting of solid tetrahedra, each vertex of which is marked with a letter. The model is based on the principle of a jig-saw puzzle, two vertices with the same letter representing the same vertex in K. Thus two tetrahedra $abcx$ and $abcy$ are to be fitted together so as to have a common face abc. However, we need not actually fit them together, as observed in the first article, since the scheme of marking the vertices gives us all the information we need about K. Now suppose that someone pieces these tetrahedra together in groups to form solid blocks and cuts the latter up into tetrahedra, possibly in quite a different way, forming a new simplicial complex K'. Under these conditions the complexes K and K' are said to be *combinatorially equivalent,* and one of the central problems in analysis situs is to determine whether or no two given complexes are combinatorially equivalent.

In our symbolism the equivalence problem can be stated in terms of certain operations called *simple transformations.* If A is

any k-simplex in a given n-dimensional complex K, we can write the latter in the form

$$K = AP + Q,$$

where Q does not contain A (If $A = x_0 \ldots x_k$, $P = k + 1/\partial x_0 \ldots \partial x$ and is called the complement of A). The operation which replaces K by the complex

$$K' = a'(\beta A)\, P + Q,$$

where the vertex a' is not contained in K, is called a *simple subdivision of order* k. A simple sub-division of order k or its inverse is called a simple transformation of order k (a simple transformation of order zero merely changes the name of a vertex), and two complexes K^1 and K^r are said to be equivalent if they are connected by a chain of complexes K^1, K^2, ..., K^r, where K^{i+1} is obtained from K^i ($i = 1, \ldots, r - 1$) by a simple transformation. There is a theorem which states that either of two equivalent complexes can be transformed into the other by a sequence of simple transformations of orders 0 and 1.

The simplest type of bounded n-dimensional complex is the n-element, an n-element being any complex which is equivalent to an n-simplex. A complex which is equivalent to the boundary of an $(n + 1)$-simplex is called an n-sphere. One can prove that $\beta(\beta K) = 0$ (cf. the first article) and that any complex which is equivalent to a closed complex is itself closed. Thus an n-sphere is closed and an n-element bounded.

It is also a theorem that the boundary of any $(n + 1)$-element is an n-sphere.

A complex M is called an n-dimensional manifold if the complement of every vertex is an $(n - 1)$-sphere or an $(n - 1)$-element, according to whether the vertex is internal to M or on βM (in case M is bounded).

If P is either an $(n - 1)$-element or an $(n - 1)$-sphere it is easy to prove that xP is an n-element, where x is any vertex which is not contained in P. Thus the *neighbourhood* of any vertex in an n-dimensional manifold M, namely the complex $x(\partial M/\partial x)$, is an n-element.

The further developments of this theory are mainly of a technical nature and it would be difficult to describe them without giving detailed arguments. It may be more interesting to indicate some of the problems which remain unsolved. Perhaps the most urgent of these is the characterisation of the n-sphere. If $n > 2$ there is, at present, no known method for finding out whether a given n-dimensional complex is an n-sphere. Of course one can do so

in many special cases. For example, the boundary of an $(n + 1)$-simplex is immediately recognisable as an n-sphere, and a complex which contains non-bounding cycles is certainly not an n-sphere. But after studying a particular 3-dimensional manifold for ten years one might still be unable to tell whether or no it was a 3-sphere. From this it will be seen how far we are from solving the general equivalence problem for n-dimensional complexes ! In fact, since we cannot be certain of recognising an $(n - 1)$-dimensional sphere or element unless $n \leq 3$, we cannot always determine whether or no a given n-dimensional complex is a manifold. However, we can recognise a 2-sphere or 2-element and hence a 3-dimensional manifold. Moreover, the 2-dimensional manifolds are completely classified and one can, in a finite number of steps, determine whether or no two given closed 2-dimensional manifolds are equivalent.

The increase of difficulty in the passage from two to three dimensions is one of the most remarkable features of topology. It seems to be mainly due to the presence of knotted circuits in a 3-sphere. A polygonal circuit without double points in 3-dimensional Euclidean space E_3 (if an ideal point at infinity is added to E_3 it is topologically equivalent to a 3-sphere) is said to be unknotted if it is the boundary of a 2-element. Otherwise it is said to be knotted. One can prove that this definition is equivalent to a description of a knot based on ordinary physical experience. Namely, a loop of string is unknotted if it can be laid out on a flat surface in such a way that it does not cross itself, and otherwise it is knotted. If one tries to characterise the 3-sphere by methods similar to those which are successfully applied to the 2-sphere one is almost invariably brought to a standstill by this one obstacle, the possibility of a knot.

Similarly, an $(n - 2)$-sphere in an n-sphere is said to be knotted if it does not bound an $(n - 1)$-element, and one can, by an inductive process, construct knotted $(n - 2)$-spheres in an n-sphere for any value of $n \geq 3$. If $n \geq 4$, for example, let S_{n-3} be any knotted $(n - 3)$-sphere in the hyper-plane given by $x_n = 0$ in the space E_n, of n real co-ordinates $(x_1, \ldots, x_n)$. Let a be the point whose co-ordinates are $(0, \ldots, 0, 1)$, let b be the point whose co-ordinates are $(0, \ldots, 0, -1)$, and let p be a variable point on S_{n-3}. As p varies over S_{n-3} the linear segments ap and bp sweep out a knotted $(n - 2)$-sphere S_{n-2}. The following is an outline of the proof that S_{n-2} is knotted. If S_{n-2} were unknotted it would bound an $(n - 1)$-element F_{n-1}. The complement of a in F_{n-1}, $\partial F_{n-1}/\partial a$, would be an $(n - 2)$-element bounded by $\partial S_{n-2}/\partial a$ and contained in S_{n-1} $= \partial E_n/\partial a$. But one can prove that $\partial S_{n-2}/\partial a$ is a knotted $(n - 3)$-

sphere in the $(n - 1)$-sphere S_{n-1}, and so does not bound an $(n - 2)$-element in S_{n-1}. Therefore S_{n-2} is knotted. The details necessary to complete this argument depend upon certain established theorems concerning the relation between n-dimensional recti-linear geometry and the symbolic theory outlined in this article.

Another unsolved problem which seems to be very difficult has to do with the separation of a combinatorial n-sphere S_n, by an $(n - 1)$-sphere S_{n-1}, the latter being a sub-complex of S_n. This much is known :

1. S_{n-1} separates S_n into two regions R_1 and R_2, each of which has the same homology characters as an n-element.
2. If R_1 is an n-element, so is R_2.
3. If $n \leq 3$, R_1 and R_2 are n-elements.

The unanswered question is : " are R_1 and R_2 n-elements if $n > 3$? "

The symbolic theory outlined here was developed in a series of articles by M. H. A. Newman and by J. W. Alexander. (See, among others, Newman, *Proc. Akad. Amsterdam*, **29** (1926), 611–26 and 627–41 ; Alexander, *Annals of Math.*, **31** (1930), 292–320.) There is now a substantial theory of knots (*i.e.* knotted circuits in 3-dimensional Euclidean space) an account of which is to be found in a book by K. Reidemeister (*Knotentheorie*, Berlin, 1932). The theory of knots and linkages is closely bound up with the theory of 3-dimensional manifolds. For Alexander (*Bull. American Math. Soc.*, **26** (1920), 370–2) proved that any orientable 3-dimensional manifold can be regarded as a Riemann 3-spread. That is to say, it covers a 3-sphere in a transformation whose only singularities lie on certain branch lines, along which two sheets coincide. Further, the covering can be so modified, if necessary, that the branch lines consist of a set of non-singular and non-intersecting circuits, which may, however, be knotted and interlinking. In general, any orientable n-dimensional manifold can be mapped on an n-sphere in such a way that the only singularities occur on an $(n - 2)$-dimensional " branch complex ". A brief discussion of knotted $(n - 2)$-spheres in an n-sphere is to be found in a paper by E. Artin (*Abhand. Math. Seminar Ham.*, **4** (1926), 174–7). Of the theorems on separation enumerated in the last paragraph the first is a special case of Alexander's duality theorem (Alexander, *Trans. American Math. Soc.*, **23** (1922), 333–49). A very polished account of this theorem is to be found in a recent book by P. Alexandroff and H. Hopf (*Topologie*, Berlin, 1935). The second theorem, that both of R_1 and R_2 are elements if one is, is proved in the first paper by Alexander to which we have referred. If $n = 2$ the theorem that R_1 and R_2 are elements is the famous Jordan theorem. A proof of Jordan's

theorem, in combinatorial analysis situs, is to be found in a note by Newman (*Proc. Akad. Amsterdam*, **29** (1926), 1–5). The corresponding theorem for $n = 3$ was first proved by Alexander (*Proc. Nat. Acad. of Sciences*, **10** (1924), 6–8).

ASTRONOMY. By R. W. WRIGLEY, M.A., F.R.S.E., Royal Observatory, Edinburgh.

THE EXTRA-GALACTIC NEBULÆ.—Study of the distribution in space of the extra-galactic nebulæ was inaugurated in 1911 by A. R. Hinks (*Monthly Notices R.A.S.*, **71**, 588), who pointed out their asymmetry with respect to the positions of the galactic poles. The investigation has since been carried on chiefly at the Harvard, Lick, and Mount Wilson Observatories. A comprehensive survey covering the whole sky and including all nebulæ brighter than the thirteenth magnitude, to the number of 1,249, was undertaken at Harvard and published in 1932 as *Annals* **88**, Part II. This catalogue of the brightest of the external systems contains the integrated magnitude, the diameter, and the type of each object, and shows that the spirals outnumber the elliptical and irregular nebulæ in the proportion of three to one, there being on the average one system in a cube of space with sides 400,000 parsecs in length. The distribution was, however, found to be far from uniform, there being twice as many systems in the northern galactic hemisphere as in the southern, while vacant regions were conspicuous in both. The emptiness found in low galactic latitudes can be readily ascribed to the presence of obscuring material in and around the plane of our own system, but the other gaps which undoubtedly exist are not so easily explained.

A similar but more restricted investigation has been completed by J. H. Reynolds (*Monthly Notices R.A.S.*, **94**, 196), consideration being restricted to objects within a radius of 4,000 kiloparsecs. The criterion was actually the angular diameter, the lower limit of 5′ corresponding, in Reynolds' opinion, to the above maximum distance. Tables are given showing the probable distances, the angular diameters, and the approximate diameters in parsecs of 27 nebulæ, and figures show the galactic distribution of 61 spirals of angular diameters ranging from 5′ to 16′. The interesting fact comes to light that, while there is a great preponderance of the systems in the south galactic hemisphere up to a distance of 700 kiloparsecs, beyond this range and up to the considered limit of 4,000 kiloparsecs the preponderance passes very definitely to the northern hemisphere.

A further very important contribution is due to E. Hubble

(*Mount Wilson Contribution*, 485). His material has been drawn from photographs of small selected areas distributed over the region of the sky north of declination — 30°, and consists of counts of some 44,000 nebulæ on 1283 plates taken with the 60-inch and 100-inch reflectors. About three-quarters of the whole sky may thus be regarded as coming within the survey. The object of the investigation was to determine the distribution of the nebulæ down to a faint uniform limiting photographic magnitude, the threshold of identification being about 20, this limit being obtainable with an exposure of one hour at the zenith under first-class observing conditions with the 100-inch reflector. As expected, no nebulæ were found along the heart of the Milky Way, the zone of avoidance being irregular and unsymmetrical, varying in width from 10° to 40°, and following in general pattern the distribution of known obscuring clouds. This zone is bordered by one of partial obscuration which gradually fades away into regions of normal distribution. From the number of nebulæ counted it is possible to obtain further information regarding this obscuring material, which appears to be present in isolated clouds rather than in a uniform layer. It consists of a narrow belt from 10° to 20° wide, centred approximately on the galactic plane, with several great protuberances extending into higher latitudes, of which the most extensive, situated in Ophiuchus and Taurus, coincide with the tips of the inclined belt of helium stars and diffuse nebulosity. Another protuberance in Cepheus, between longitudes 60° and 100°, and extending to latitude + 20°, includes the celestial north pole and accounts for the scarcity of nebulæ in this region.

There is a definite variation in the number of nebulæ with galactic latitude, and from the poles to latitude 15° the frequency is represented satisfactorily by the cosecant formula

$$\text{Log N} = 2 \cdot 115 - 0 \cdot 15 \text{ Cosec } \beta$$

where N is the number of nebulæ per square degree and β is the galactic latitude. This indicates a total obscuration of 0·5 magnitude from pole to pole, with no appreciable difference between the two hemispheres. The red shift observed in the spectral lines of the very distant systems has the effect of making them photographically fainter, and, when due allowance has been made for this, the rate of increase of log N with the exposure time suggests a uniform distribution of these distant nebulæ in space. The number of nebulæ per square degree down to magnitude *m* is given by the formula

$$\log \text{N}_m = 0 \cdot 6 \ m - 9 \cdot 12_{\text{s}}$$

Similar results have been reached by N. U. Mayall from counts of 15,000 nebulæ on 489 plates taken with the Crossley reflector (*Lick Observatory Bulletin*, 458). The apparent limiting photographic magnitude was approximately 19, which, corrected for the red-shift and with the assumption of − 13·8 as the mean absolute magnitude of the nebulæ, would imply that the counts extended to a distance of 100,000,000 light years. The hypothesis of a uniform spatial distribution was found to be consistent with the counts.

There are thus conflicting views as to whether or not the galaxies are distributed at random over the sky. In *Harvard College Observatory Bulletin*, 895, Bart J. Bok analyses the results of the Harvard and the Mount Wilson surveys. Of the former, both the Shapley-Ames catalogue of objects brighter than magnitude 13 and the counts of samples down to magnitude 18 (*Bulletin* 890) show a definite tendency towards clustering. As regards the Mount Wilson survey, although Hubble considers that, after excluding a few well-known clusters, "uniform distribution appears to be a general characteristic," Bok finds that, after a correction for the dispersion in limiting magnitude has been applied, the observed frequency curve differs considerably from that computed for random distribution. He therefore agrees with Shapley and Lundmark, among others, that there is a widespread lack of uniformity in the grouping of the external galaxies, and that the irregularities are an important characteristic of the Universe, being possibly a demonstration of evolutionary tendencies in the metagalactic system.

The apparent radial velocities of a further hundred extragalactic nebulæ are given in an important paper by M. L. Humason (*Mount Wilson Contribution*, 531), supplementing a former list of 46 velocities published in 1931 (*M. W. Contrib.*, 426). For the new determinations, a table is given comprising the position of each nebula, its apparent photographic magnitude, its type, its spectral type, and information as to whether it is an isolated object or belongs to a group or cluster. Of the 100 objects, 56 are isolated and the others are divided between 6 clusters and 5 groups. Excepting 6, all the velocities are recessional, and range from + 50 km./sec. for a member of the Virgo Cluster to one of + 42,000 km./sec. for a member of the Ursa Major Cluster No. 2. For velocities less than 2,000 km./sec. the values are given to the nearest 50 km./sec., while larger ones are given to the nearest 100 km./sec. Humason considers the uncertainty in the estimated velocities to be of the order of 50 km./sec., though for the largest it may reach 200 km./sec. A comparison of the Mount Wilson values with five velocities observed by Slipher at Flagstaff and one obtained at Lick shows

satisfactory agreement. The velocity-distance relation for clusters is demonstrated by plotting the logarithms of the velocities against the apparent photographic magnitudes of the fifth brightest member of each cluster. For ten clusters, ranging in distance from that in Virgo to those in Bootes and Ursa Major No. 2, whose brightest members are only of magnitude 18, the points agree reasonably closely with the straight line log V = 0·2 m + 1·025, and it therefore seems that the relationship between velocity and distance remains sensibly linear up to a distance of at least 70,000,000 parsecs.

Humason also summarises the available information regarding six clusters and four groups of nebulæ, giving the estimated number of individual objects in each, together with the range in their velocities and brightness. A more detailed study of the Virgo Cluster is given by Sinclair Smith in *Mount Wilson Contribution* 532, who for the first time gives sufficient data to determine some of the physical characteristics of a cluster of nebulæ. In this case the velocities of the different members of the cluster show an average range of 500 km./sec. around a mean value of + 1,200 km./sec., while the range in apparent photographic magnitude is from 10 to 15. Rather surprisingly, there seems to be no connection between the variation in velocity and that in brightness. Further, the absence of any central concentration of those bodies with high velocities implies that the cluster as a whole is not in rotation. Smith deduces that the cluster is neither condensing nor breaking up, but forms a stable system, more or less held together by its gravitational field. If this be assumed, an estimate of the mass of the cluster can be derived from the measured radial velocities of its members. Velocities up to 1,500 km./sec. appear to be fairly common, and Smith assumes this to be the velocity of an outer body describing a circular orbit round the whole cluster. From this he finds the mass of the cluster to be 10^{14} times the sun, and, on the basis of 500 nebulæ in the cluster and no internebular material, the mean mass of a single nebula to be 2×10^{11} times the sun. This value is about 200 times Hubble's estimate of the mass of an average nebula, the difference being possibly due to the presence of a quantity of internebular material within the cluster. The mean peculiar velocity of the cluster nebulæ is about four times that found by Hubble for isolated nebulæ, so it seems that, owing to the strong gravitational field of a cluster, nebulæ leaving it must lose energy, and vice versa.

THE VELOCITY OF LIGHT.—All the above deductions regarding the velocities and distances of the nebulæ are, of course, entirely

dependent on the constancy of energy of the light quanta or photons in their long journey of millions of years through space. In this connection, special interest is attached to the latest determination of the velocity of light by Michelson, Pease, and Pearson, whose results are summarised in the *Annual Report of the Director of the Mount Wilson Observatory*, 1935. Four series of measures, each including several hundred observations, have resulted in a velocity of 299,774 km./sec., with an uncertainty of 1 or 2 km./sec. This velocity is 22 km./sec. lower than the value obtained in 1926, and no reason can yet be given for the disagreement, though the observing conditions were quite different in the two cases. On the former occasion the light followed an air path of 45 miles which twice crossed a wide deep canyon, while in the later investigation it had a relatively short path of 8 or 10 miles, obtained by multiple reflections within a pipe line in which the air pressure was only a few millimetres of mercury. In spite of every care and precaution, it was nevertheless found that abnormal values persisted at times for days or even weeks, indicating the existence of disturbing forces of unknown origin, though the influence of these deviations on the adopted mean velocity was relatively small.

In the view of M. E. J. Gheury de Bray the observational evidence indicates that the velocity of light has definite fluctuations. In *Astronomische Nachrichten*, No. 6,038, he gives the following list of separate determinations :

1874·8 Cornu	299,990 ± 200,	1902·4 Perrotin	299,901 ± 84.
1879·5 Michelson	299,910 ± 50,	1924·6 Michelson	299,802 ± 30.
1882·7 Newcomb	299,860 ± 30,	1926·0 Michelson	299,796 ± 4.
1882·8 Michelson	299,853 ± 60,	1928·5 Karolus	299,778 ± 20.

1932 Michelson, Pease and Pearson 299,774 ± 11.

These determinations are obviously of unequal reliability, those of 1874 and 1902 having a large relative probable error, but a general tendency to decrease during the sixty years covered by the observations seems clearly marked. De Bray suggests the existence of a regular sinusoidal variation of the velocity, and considers that the available observations are satisfied by the equation

$$V = 299,885 + 115 \, \text{Sin} \, [2\pi/40 \, . \, (T - 1901)]$$

The reality of this harmonic variation, with a period of only 40 years, cannot of course be accepted without considerably fuller evidence, for it depends as yet entirely upon the observations of Perrotin in 1902, for which the probable error is large. The situation is, however, unsatisfactory from the point of view which regards

the velocity of light in vacuo as a "Constant of Nature," and a further series of experiments is urgently needed to clear up the mystery.

PHYSICS. By W. N. BOND, M.A., D.Sc., F.Inst.P., The University, Reading.

THE ABSOLUTE MEASUREMENT OF X-RAY AND γ-RAY ENERGY.— In measuring a quantity of X-rays or γ-rays, we are confronted with problems somewhat similar to those of photometry. The beam of rays may be nearly homogeneous, consisting of radiation of one wave-length, λ, and being analogous to monochromatic light. But, in general, the beam will comprise radiations of a whole range of wave-lengths. To describe the nature of such a complex beam fully, it would be necessary to specify the rate of arrival of energy per unit area of the transverse section of the beam, for each of a series of small ranges of wave-length, λ_1 to λ_2, λ_2 to λ_3 et cetera. A more crude specification might be made, by stating the total rate of arrival of energy and the approximate mean wave-length or "hardness" of the rays.

The ideal method of measuring the energy conveyed by the beam would be to absorb the whole of the beam in a suitable calorimeter. Various difficulties arise in carrying out such an experiment. In the first place, the rate of arrival of energy is usually rather small. A typical heterogeneous beam of X-rays might supply as much as 1 calorie per hour, but a monochromatic beam would probably provide much less. Nevertheless, the heat produced when X-rays were completely absorbed by a metal was measured by Dorn, as long ago as 1897. It was later shown that the heat produced was independent of the particular metal used as absorber (Augerer, 1907; Bumstead, 1908).

It is necessary to remember that a beam of X-rays may fluctuate in intensity and position. It is therefore often advisable to let the whole of the beam pass through the measuring apparatus (before reaching whatever other apparatus is in use) so as to measure a definite fraction of the total energy conveyed by the beam. In the case of γ-rays the penetrating power is very great, and it would be difficult to design a calorimeter that would absorb the whole of the energy. Hence it is again necessary for the measuring apparatus to absorb only a fraction of the total energy.

When the beam passes through the measuring apparatus and thence to any other apparatus that is being used, a further difficulty arises. The fraction of the total energy that is being absorbed and measured will depend, in general, on the wave-length of the

rays. If the beam is homogeneous, the fraction that is being absorbed can be measured by comparing the intensity of the beam as it enters and leaves the measuring apparatus (or it may be computed from the known coefficient of absorption of the rays). But if the beam be heterogeneous, the fraction absorbed can be directly determined only by absorbing and measuring all of the energy that emerges from the original measuring apparatus.

If a nearly parallel homogeneous beam (of wave-length λ), conveying I_0 ergs per square centimetre per second, is incident normally on a uniform slab of material of thickness d, the intensity of the emergent beam is

$$I = I_0\, \varepsilon^{-\mu d}$$

where μ is the "linear total absorption coefficient" of the material for rays of wave-length λ. Near the first face of the slab, energy will be absorbed (or scattered) by the material at a rate of μI_0 ergs per cubic centimetre per second. Thus, if (μI_0) is measured, and if μ is either known previously or found by determining the ratio I/I_0, the required intensity I_0 can be deduced in ergs per square centimetre per second.

It is not usual to measure the rate of heat production per unit volume of the "slab," but to measure the rate of production of pairs of ions per unit volume in some gas (which for standard purposes may be dry air at normal temperature and pressure). In fact, the Second International Congress of Radiology (1928) defined the standard dose or quantity of X-rays, the röntgen, as : " that quantity of X radiation which, when the secondary electrons are fully utilised and the wall effect of the chamber is avoided, produces in 1 cm.³ of atmospheric air at 0° C. and 76 cm. mercury pressure, such a degree of conductivity that 1 e.s.u. of charge is measured at saturation current." Various standard ionisation chambers suitable for measuring X-ray doses in röntgens have been made and compared, and found to agree with one another to within 0·5 per cent. (Kaye and Binks, *Brit. J. Radiol.*, **2,** 553, 1929; **6,** 195, 1933; **6,** 530, 1933. Taylor, *Bur. Stand. J. Res.*, **2,** 771, 1929; **8,** 9 and 325, 1932).

The dose expressed in röntgens is proportional to the ionisation produced *per unit volume* (corresponding to an energy absorption of $\mu\, I_0$ ergs per cm.³ per second). The *total* ionisation would correspond to an energy absorption of I_0 ergs per cm.² per second. A beam of large intensity, I_0, might be of so short a wave-length that it was little absorbed (μ small). In that case it would take a long time for the beam to deliver a dose of 1 röntgen.

Standard air ionisation chambers, such as I have referred to,

enable the total charge produced, rate of production of charge, and rate of production of ions to be measured per unit volume of the air. From the rate of production of ions the rate of absorption of energy may be deduced. As the result of various types of experiment by some twenty sets of experimenters it is believed that the average energy required to produce a pair of ions in air is equal to the energy acquired by an electron in falling through a potential difference of about 32·5 volts, that is to say $5·2 \times 10^{-11}$ ergs. (For references see L. H. Gray, *Proc. Roy. Soc.*, A, **156,** 592, Sept. 1936.) There is evidence that the work done in producing a pair of ions in air is independent of the wave-length of the X-rays ; and it appears likely that the work done per ion-pair is the same for γ-rays as for X-rays.

Multiplying the number of ion-pairs produced in air per c.c. per second by the energy required to produce an ion-pair ($5·2 \times 10^{-11}$ ergs) and dividing by the linear absorption coefficient, we obtain the intensity of the original beam in ergs per cm.² per second. In this calculation, the " true " and not the " total " absorption coefficient should be used, because part of the X-ray energy is used in producing scattered X-rays, which do not contribute to the measured ionisation.

The standard ionisation chambers, mentioned above, are of the " free-air " type ; the beam of X-rays passes between two plane electrodes (one fitted with a guard ring) situated so far from the beam that the electrons ejected from the air by the X-rays produce their full ionising effect. The electrodes are usually of aluminium covered with graphite, in order to prevent the scattered radiation causing much secondary emission from the electrodes. In the case of γ-rays, the electrons ejected by the rays may traverse considerable distances in the air before ceasing to produce ionisation. Nevertheless, at the National Physical Laboratory a " free-air " ionisation chamber has been constructed that is suitable for measuring the ionisation due to γ-rays ; the electrodes in this chamber are a number of feet apart.

Of recent years another type of ionisation chamber, the " air-wall " chamber, has been coming into vogue. A potential difference is maintained between a central electrode and the walls of the chamber, and the beam of X-rays passes right through the whole system. The walls and electrode are made of materials having atomic number not much different from the effective atomic number for air (graphited ash-free paper being suitable). In this type of chamber the central electrode can be quite near to the walls. Consequently a large electrostatic field is easily attained,

and the ionisation current nearly reaches its saturation value even for intense beams of X-rays. References to work on chambers of this type were given by I. Backhurst (*Reports on Progress in Physics*, Physical Soc., 1934, p. 237). Two other papers on the subject may be mentioned : J. A. Crowther (*Brit. Journ. Radiol.*, **2**, 175, 1929) and L. R. G. Treloar (*Brit. Journ. Radiol.*, **2**, 188, 1929).

Dr. L. H. Gray, in a recent paper (*Proc. Roy. Soc.*, A, **156**, 578–96, Sept. 1936), describes the absolute measurement of γ-ray energy by ionisation chambers of a type somewhat like the " air-walled." He shows that the γ-ray energy absorbed per unit volume of a solid medium is almost exactly equal to the product of (1) the ionisation per unit volume in a small gas-filled cavity in the solid, (2) the average energy lost by the secondary electrons per pair of ions formed in the gas, and (3) the ratio of the stopping power of the medium and the gas for the secondary electrons. Dr. Gray also suggests the application of the same method to the absolute determination of the energy of a stream of neutrons.

Walled chambers were used as long ago as 1908 (Laby and Kaye, *Phil. Mag.*, Dec. 1908), and were discussed very ably by Sir William Bragg (*Studies in Radioactivity*, p. 94 *et seq.*, 1912). Before leaving this subject, mention may be made of two recent accounts of experiments on the Absorption of X-rays (T. R. Cuykendall, *Phys. Rev.*, **50**, 105–109, July 1936 ; M. T. Jones, *Phys. Rev.*, **50**, 110–14, July 1936).

THE PRODUCTION OF POLARISED X-RAYS.—The production of polarised X-rays was originally demonstrated by Prof. Barkla in 1904 (*Phil. Trans.*, A, p. 467, 1905 ; *Proc. Roy. Soc.*, A, **77**, 247, 1906). He found that if a beam of X-rays were allowed to fall on a material of low atomic weight, such as carbon or wax, fewer X-rays were scattered sideways in a direction parallel to the original beam of cathode rays than in a direction at right angles to this. Thus there was evidence that the original beam of X-rays was partly polarised. The polarisation is small, however, and has never been observed to exceed 10 or 20 per cent. The subsequent scattering of the beam by the carbon or wax produces further polarisation ; but it has not been found possible to produce an intense fully polarised beam by these means.

It would increase the usefulness of X-rays for research work if means could be found of producing a polarised beam of reasonably large intensity. This problem has been investigated with considerable success by Dr. W. H. George (*Nature*, **136**, 180, 1935 ; *Proc. Roy. Soc.*, A, **156**, 96–107, Aug. 1936). There was little pros-

pect of increasing the polarisation of the original beam. It was necessary, therefore, to use as intense a source of rays as possible; to place the apparatus and the slits that limit the beam in the most suitable relative position; to use a sensitive method of detecting the polarised beam; and, especially, to improve the arrangement for scattering the beam. Dr. George decided to substitute for the lump of carbon or wax a single metal crystal, cut in such a way and used at such an angle as to be appropriate for homogeneous X-rays of some particular wave-length. He used the K radiation of copper, scattered from a single crystal of copper ground parallel to the (311) planes, using a scattering angle of 45°. The copper crystal was grown by Bridgman's slow cooling method, was cut, examined on an X-ray spectrometer, and finally ground and polished very carefully on a specially designed geometrical slide. A spectrometer table was adapted for adjusting and holding the crystal in correct relation to the beam of X-rays; and a tube of the Shearer type was designed for producing the rays.

Photographs were taken using the crystal as the scatterer, and also using carbon and wax scatterers. Measurements of the photographs by means of a Moll microphotometer showed that the copper crystal scatterer produced a polarised beam that was about 1000 times as intense as that yielded by the carbon or wax scatterers. In other words, the method devised by Dr. George would enable an experiment with polarised X-rays, which would have taken 40 days by the earlier methods, to be performed in an hour.

THE VALUE OF THE ELECTRONIC CHARGE.—For many years the most reliable direct estimate of the electronic charge was $e = (4 \cdot 774 \pm 0 \cdot 005) \times 10^{-10}$ e.s.u., deduced by Millikan from his experiments on the movement of single charged drops of liquid (*Phil. Mag.*, **34**, 1, 1917). A slight change was made in the estimate by Professor R. T. Birge (*Rev. Mod. Phys.*, **1**, 1, 1929), who used more recent values of the electrical units and the velocity of light. The amended value, $4 \cdot 770 \pm 0 \cdot 005$, was upheld by Professor Millikan (*Phys. Rev.*, **35**, 1231, 1930). This estimate is confirmed by that of J. Mattauch (*Zeits. f. Physik*, **32** (6 and 7), 439–72, 1925), who experimented on charged drops of vacuum oil in nitrogen and carbon dioxide at various pressures, obtaining the value $4 \cdot 758 \times 10^{-10}$ e.s.u.

Subsequently it was found possible to estimate the value of e by another method. The spacing of atoms in a crystal was compared with the known spacing of a ruled grating, by means of X-ray spectra. Knowledge of the density of the crystal enabled the value of Avogadro's constant to be deduced; and, hence, using

Faraday's electrolytic constant, the value of e could be found. Several estimates were made by this method, most of which indicated that e was about $4\cdot80_5 \times 10^{-10}$ e.s.u. In particular Bearden obtained the value $(4\cdot8036 \pm 0\cdot0005) \times 10^{-10}$ e.s.u. (*Phys. Rev.*, **48**, 385, 1935).

It was suggested by Shiba (*Sci. Pap. Inst. phys. chem. Res.*, Tokyo, **19**, 97, 1932, and **21**, 128, 1933) that the disagreement between the results of the oil-drop and X-ray methods might be due to Millikan having assumed too small a value for the viscosity of air. Recently, G. Kellström (*Nature*, **136**, 682, 1935) gave a preliminary account of viscosity measurements made by a rotating cylinder method, which gave the value $\eta_{23} = (1834\cdot8 \pm 3\cdot0) \times 10^{-7}$ c.g.s., which is appreciably higher than the value assumed by Millikan ($1822\cdot7 \pm 0\cdot9$). Kellström's value of the viscosity, together with Millikan's oil-drop data, lead to the value $e = (4\cdot816 \pm 0\cdot013) \times 10^{-10}$ e.s.u., in reasonable agreement with the X-ray estimates (R. T. Birge, *Phys. Rev.*, **48**, 918, 1935).

E. Bäcklin and H. Flemberg (*Nature*, **137**, 656, 1936) have given a brief account of a fresh determination of e by the oil-drop method. Using Millikan's value for the viscosity, their experiments gave $e = 4\cdot752$; and with Kellström's value for the viscosity the result became $e = 4\cdot800 \times 10^{-10}$ e.s.u. These results are in good agreement with those deduced from Millikan's oil-drop experiments, though slightly lower in value.

The higher value of the viscosity of air has been confirmed by measurements by a capillary tube method (W. N. Bond, *Nature*, **137**, 1031, 1936) which gave $\eta_{23} = (1834\cdot7 \pm 0\cdot8) \times 10^{-7}$. Further confirmation is given by Kellström, who has repeated his rotating cylinder experiments with certain improvements in the apparatus (*Phys. Rev.*, **50**, 190, 1936), obtaining $\eta_{23} = (1834\cdot9 \pm 2\cdot7) \times 10^{-7}$, where the $\pm 2\cdot7$ indicates the *limit* of error, and not the probable error.

Combining the oil-drop measurements of Millikan and of Bäcklin and Flemberg with the recent viscosity data, we may estimate the value of the electronic charge as

$$e = (4\cdot808 \pm 0\cdot008) \times 10^{-10} \text{ e.s.u.}$$

There is no longer any disagreement between the results of the oil-drop and X-ray methods.

THE ANGULAR MOMENTUM OF CIRCULARLY POLARISED LIGHT.— A " suggestion as to the angular momentum in a beam of circularly polarised light " was made by J. H. Poynting (*Proc. Roy. Soc.*, A, **82**, 560, 1909), and he also suggested possible means of detecting the effect.

According to the electromagnetic theory, it was to be expected that a beam of polarised light would exert a torque on a doubly refracting plate which alters its state of polarisation (A. Sadowsky, *Acta et Commentationes Imp. Universitatis Jurievensis*, **7**, Nos. 1–3, 1899 ; **8**, Nos. 1–2, 1900. P. S. Epstein, *Ann. d. Physik*, **44**, 593, 1914). The same effect would be predicted by the quantum theory, if it were assumed that each quantum of circularly polarised light has an angular momentum of amount $h/2\pi$ (A. E. Ruark and H. C. Urey, *Proc. Nat. Acad. Sci.*, **13**, 763, 1927).

The predicted couple is very small, but it was detected by R. A. Beth (*Phys. Rev.*, **48**, 471, 1935). Later in the same year A. H. S. Holbourn reported the result of measurements that he had made, according to which each quantum of the light was found to have an angular momentum of $(1 \cdot 05 \pm 0 \cdot 15) \, h/2\pi$ (*Nature*, **137**, 31, 1936). A detailed account has now appeared of an extensive investigation and measurement of the effect by Beth (*Phys. Rev.*, **50**, 115–25, July 15, 1936). The result of observations made independently by two observers showed the magnitude and sign of the effect to be correct, and showed that it varied as predicted when each of three experimental variables was changed.

GENERAL AND ORGANIC CHEMISTRY. By O. L. BRADY, D.Sc., F.I.C., University College, London.

FIXATION OF DOUBLE BONDS IN KEKULÉ'S BENZENE FORMULA.— Mills and Nixon (*J. Chem. Soc.*, 1930, 2510) first pointed out that, if the tetrahedral theory of the distribution of carbon valencies be
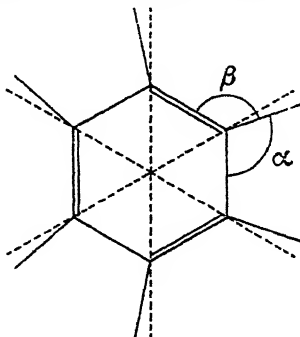
FIG. 1.

applied to the Kekulé formula for benzene, the external valencies are not directed as from the centre of the hexagon as shown by the dotted lines in Fig. 1, but lie in directions inclined alternately to either side as shown by the full lines.

This follows from the probable arrangement of the valencies of

the doubly bound carbon atom. In such an atom if the angle α between the two single bonds is the normal tetrahedral angle of 109·5°, the angle β which the single bonds make with the plane of the double bonds would be $\frac{1}{2}(360 - 109 \cdot 5)$ or 125·25°.
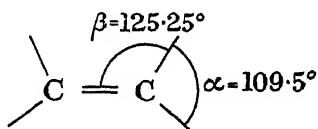
Though it is probable that the presence of the double bond will effect the angle α, it seems likely that some difference between α and β will remain. In the case of the Kekulé formula for benzene a further modification of the internal angles is necessitated since six $=C<$ residues are therein united with the consequent reduction of the internal angle to that of a hexagon, namely 120°. If this reduction involves correlated increases in the external angles and if these are distributed equally the difference between α and β will be further increased.

If this reasoning be correct, when the benzene ring is fused with a five carbon atom ring the most stable form will be when the linking common to the two rings consists of a single bond, because the angle of a regular pentagon (108°) approximates more nearly to the angle α than to β. On the other hand, if a six carbon atom ring is fused to the benzene ring, then the most stable form will be when the linking common to the two rings is a double one, the angle of the hexagon (120°) approximating more nearly to the angle β. Mills and Nixon tested this theory by substitution experiments on 5-hydroxyhydrindene which might be represented as either (I) or (II). The action of bromine and diazonium salts on
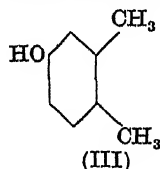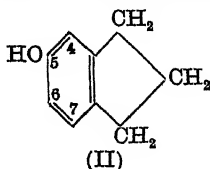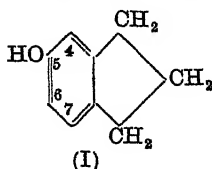
phenols is analogous to that of these reagents on aliphatic enols; reaction occurs at the carbon atom which is linked to that bearing the hydroxyl group by a double bond, for example ethyl aceto-acetate :—

$$CH_3.C(OH)=CH.COOEt \xrightarrow{Br.} CH_3.C(OH)=CBr.COOEt.$$
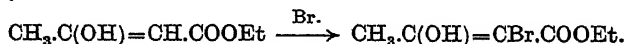
If, therefore, 5-hydroxyhydrindene has the structure (I) it should substitute mainly in position 6, and if it had structure (II) mainly in position 4. If the double bond changed places readily as de-

manded by the Kekulé dynamic formula one might expect substitution in both positions in approximately equal amounts. Actually the compound brominated and coupled with toluenediazonium salts to the extent about 90 per cent. in the 6 position. The evidence is not perfectly conclusive, however, because, as Mills and Nixon mention, 4-hydroxy-1 : 2-dimethylbenzene (III) also substitutes mainly in the 5 position, and in this case there seems no reason why there should not be free movement of the double bonds. The alkyl groups evidently have an orienting effect and it might be held that the alicyclic ring might have a similar influence sufficient to cause the preferential substitution observed. If (IV) represents a more stable form of 3-hydroxytetralin than (V), then reaction should take place mainly at position 4, and this is known to be the case (Schroeter, *Annalen*, 1922, **426**, 83).

(IV) (V)
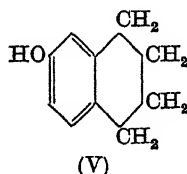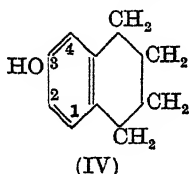
The question of the degree of strain in the reduced ring in the case of the tetralin is complicated by the possibility of the ring becoming multiplanar, but Mills and Nixon on geometrical considerations concluded that (IV) would be the less strained ring. If the preferential substitution at 6 in the hydroxyhydrindene was due to mere orienting effect of the reduced ring, one would expect a similar effect in the hydroxy-tetralin.

Recently two groups of investigators have supplied important addition evidence on these points. Fieser and Lothrop (*J. Amer. Chem. Soc.*, 1936, **58**, 2050) point out that if Mills and Nixon's views are correct 5-hydroxy-6-methylhydrindene (VI), where the

(VI) (VII) (VIII)
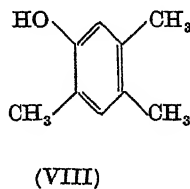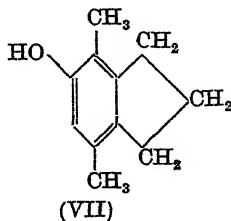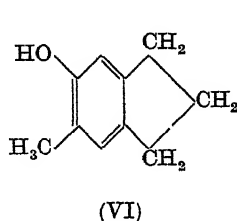
reactive position ortho to the hydroxyl is blocked, should couple with diazonium salts less readily than 5-hydroxy-4 : 7-dimethylhydrindene (VII), where it is free. Actually they found that (VII) coupled readily whereas (VI) does not couple at all. Pseudo-

cumenol (VIII) couples readily, indicating that the lack of reactivity of (VI) is not due to a simple orienting effect of the second ring. This confirms the conclusions of Mills and Nixon as regards the hydrindene compounds. Where the second ring is a six membered one (IX) and (X) both compounds couple with diazonium salts so
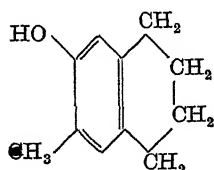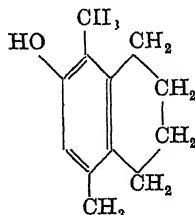
(IX) (X)
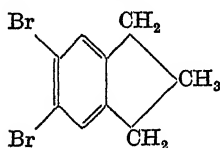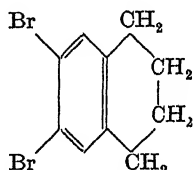
(XI) (XII)

that the double bond is comparatively free to move and the Mills-Nixon effect if operative causes but slight restriction.

Exactly similar conclusions are reached by Sidgwick and Springall (*J. Chem. Soc.*, 1936, 1532) on entirely different grounds, namely from the determination of the dipole moments of 5 : 6-dibromo-hydrindene (XI) and 2 : 3-dibromotetralin (XII) and comparing them with the dipole moments of the unsubstituted hydrocarbons, the monobromo-compounds and with o-xylene, 4-bromo- and 4 : 5-dibromo-o-xylene. They found that the moments of the Br—C—C—Br system in dibromotetralin and dibromo-o-xylene are the same and equal to that found for o-dibromobenzene, but that in the dibromohydrindene the moment is 0·34 D lower than the mean value of 2·12 D found in the above compounds and is only 0·06 D lower than that calculated on the Mills-Nixon hypothesis. From a consideration of various possibilities the authors conclude that the result cannot be due to causes other than an increase in the valency angles of the Br—C=C—Br system as suggested by Mills and Nixon.

The present writer can supply from his own experience another example of apparent restriction of movement of the double bonds in a benzene ring fused to a five membered ring; 4-methyl-7-amino-1 : 2 : 3-benztriazole (XIII) does not undergo ring closure like o-diamines on treatment with nitrous acid :—

$$CH_3N \xrightarrow{HNO_3} CH_3N \longrightarrow$$

(XIII)
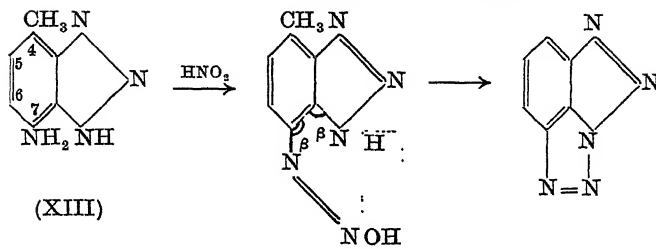
The second five membered ring will not form owing to the strain involved in reducing the angles (β) from about 125° to the angle of 108° for the pentagon.

ORGANIC COMPOUNDS CONTAINING DEUTERIUM.—The organic chemistry of deuterium is presenting more problems than were anticipated by those who entered this field so enthusiastically. Perhaps the most notable stumbling-block in the way of a rapid extension of the subject has been the unexpected ease of interchange between hydrogen and deuterium in many compounds ; this has necessitated the use of reagents free from hydrogen and the consequent exclusion of water even for crystallisation. The history of hexadeuterobenzene C_6D_6 illustrates the difficulties. The various methods employed for preparing this compound were indicated in a recent article (SCIENCE PROGRESS, 1936, 100) and some of the physical properties then assigned to it were given. Ingold, Raisin and Wilson (*J. Chem. Soc.*, 1936, 915) have now given more details of their preparation of hexadeuterobenzene by the action of successive quantities of deuterium sulphate D_2SO_4 in deuterium oxide on benzene, all operations being carried out in a vacuum. In this way they obtained a hexadeuterobenzene containing not more than 1 per cent. of pentadeuterobenzene CD_5H. They conclude that the hexadeuterobenzene of Klit and Langseth (*Z. physikal. Chem.*, 1936, **176**, 65) contained 11 per cent., of Bowman, Benedict and Taylor (*J. Amer. Chem. Soc.*, 1935, **57**, 960) about 14 per cent., and of Erlenmeyer and Lobeck (*Helv. Chim. Acta*, 1935, **18**, 1464 ; 1936, **19**, 336) about 30 per cent. of pentadeuterobenzene. After allowing for the residual hydrogen in their sample, Ingold, Raisin and Wilson give the following physical properties for hexadeuterobenzene as compared with benzene :

	C_6D_6	C_6H_6
b.p.	79·288	80·112
$d_{25°}^{25°}$	0·9456	0·8760
$n_D^{22°}$	1·49779	1·49982

The analysis of deutero-organic compounds is not easy. The percentage of carbon gives an approximate idea of the deuterium content, for example C_8H_{12} contains 88·9 per cent. C whilst C_8D_{12} contains 80 per cent. C, but for accurate work special methods are necessary for what is becoming known as isotopic analysis. Erlenmeyer and Gärtner (*Helv. Chim. Acta*, 1936, **19**, 129) describe a method in which the substance is burnt with cupric oxide and the water collected in a tube cooled in solid carbon dioxide mixed with chloroform and carbon tetrachloride. This water is then mixed with a known amount of standard water, distilled and the density of the distillate, after removal of dissolved gases, determined. Ingold, Raisin and Wilson (*loc. cit.*) analysed their deuterobenzene by mixing 0·1 gm. of their product weighed on a micro-balance with 9 c.c. of accurately weighed purified benzene. The whole of the mixture was burnt over hot copper oxide in a stream of nitrogen, the water collected at $-78°$, distilled and its density determined to an accuracy of 1 in 10^6 which should give an accuracy of 0·1 per cent. on the whole determination. Corrections for the deuterium content of ordinary water and of ordinary benzene (shown to be the same as in the standard water) must be applied.

As was to be expected, attempts are being made to resolve compounds into optical isomerides where the asymmetry of the molecule is due to replacement of hydrogen by deuterium. There seems some doubt whether success in this direction is possible, whether, geometrically, the replacement of hydrogen by deuterium can introduce asymmetry into a molecule. Further, the expression of Boys (*Proc. Roy. Soc.*, A, 1934, **144**, 675) for calculating the specific rotatory power of a simple asymmetric organic compound, C*abcd*, contains in the denominator a series of factors $(a-b)$-$(a-c)(a-d)(b-c)(b-d)(c-d)$ expressing the differences of the effective radii of the groups attached to the carbon atom. There seems reason to suppose that the radius of the deuterium atom is the same or very near that of the hydrogen atom, in which case one of the factors would become zero and no rotation would be observable. Bülmann, Jensen and Knuth (*Ber.*, 1936, **69**, 1031) claimed to have obtained 2-deuterocamphane with a small positive rotation by treating *l*-bornylchloride with magnesium and deuterium oxide. By treating *l*-bornylchloride with magnesium and water they obtained a mixture of camphane, *l*-camphene and *d*-dihydrocamphene which after separation gave a camphane which was inactive. Later Bülmann, Jensen and Bak (*Ber.*, 1936, **69**, 1947) found that in the latter case the camphane isolated always showed a slight positive rotation and that identical results were

obtained with water and deuterium oxide. Erlenmeyer and Gärtner (*Helv. Chim. Acta*, 1936, **19**, 145) reduced cinnamic acid with deuterium iodide and red phosphorus in deuterium oxide at 150°. After dissolving in aqueous alkali and precipitating with hydrochloric acid twice they obtained an acid which on isotopic analysis gave $C_6H_{2\cdot18}D_{2\cdot82}C_2H_{1\cdot62}D_{2\cdot38}COOH$. Besides the addition of deuterium at the double bond some hydrogen had been replaced by deuterium both in the side chain and in the nucleus. The amount replaced in the benzene ring was determined by oxidation of the deuterated cinnamic acid to the benzoic acid and isotopic analysis of this compound. Attempts to resolve this compound were unsuccessful.

Clemo and McQuillen (*Jour. Chem. Soc.*, 1936, 808), on the other hand, claim to have shown that α-pentadeuterophenylbenzylamine

$$C_6D_5.CH.C_6H_5$$
$$|$$
$$NH_2$$
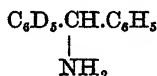
is resolvable into optical isomerides.

PHYSICAL CHEMISTRY. By H. W. MELVILLE, D.Sc., Ph.D., Colloid Science Laboratory, Cambridge.

THE HOMOGENEOUS REACTIONS OF HEAVY HYDROGEN.—In a previous article in this series (SCIENCE PROGRESS, No. 121, p. 103, 1936) an outline was given of the usefulness of isotopes in elucidating certain problems in chemistry. The applications in this field have been necessarily restricted as the technique for conducting such experiments is only now being developed and the illustrations were chosen merely to indicate the potentialities of the method. With deuterium, on the other hand, the interest aroused since its discovery has led to the accumulation of a considerable literature dealing with exchange reactions. Besides yielding information which would otherwise have been unobtainable, the results have shed much new light on a number of old problems. One of the greatest interest is the mechanism whereby deuterium exchanges with hydrogen in hydrides, a matter which is intimately bound up with the mechanism of the hydrogenation and dehydrogenation of molecules. Again, the study of the relative rates of reactions of hydrogen and of deuterium has brought to light the importance of zero point energy in contributing to the energy of activation of reactions. In this review, then, some account will be given of the progress made in the directions indicated above, confining the survey exclusively to gaseous reactions.

Homogeneous Exchange Reactions.—As these are of the simplest type and perhaps the best understood, we shall consider them first of all. The least complicated reaction is that in which a deuterium atom exchanges with a hydrogen atom, thus : $D + HX \rightarrow DX + H$. The interest lies in determining how much energy of activation is required and how this quantity varies among the hydrides of a number of elements. In practice this amounts to measuring the bimolecular reaction velocity constant, and the most difficult part is the production of a known concentration of deuterium atoms. Deuterium atoms may be produced in three ways, (*a*) by passing an electrical discharge through D_2 molecules, (*b*) by thermal dissociation of D_2, and (*c*) by photochemical dissociation of D_2 with mercury atoms, excited by the mercury line at 2537 Å. In the first method the atoms are led from the discharge tube into the hydride and the resulting mixture, after reaction, freed from hydride and deuteride and analysed for deuterium content. The concentration of atoms may be determined by the Wrede slit gauge (P. Harteck, *Z. Phys. Chem.*, **139**, A, 98, 1928), or by measuring the rate of conversion of para to ortho hydrogen. The disadvantage of the method is that the determination of the atomic concentration is not very accurate, and there is a further complication in that it is difficult to find whether the reaction is really proceeding in the gas phase or whether it takes place by a deuterium atom striking the hydride molecule adsorbed on the reaction vessel wall. It has, however, the important merit that compared with the other two methods the concentration of atomic D is high and the exchange therefore occurs rapidly. The thermal method would appear to the most accurate, for the equilibrium constant for the dissociation of deuterium is known accurately and, hence, the atomic concentration is calculable under any conditions. But in order that the method be applicable the extent of the thermal dissociation of the hydride must be negligible. Otherwise exchange will occur by the reaction $D_2 \rightarrow 2D$, $HX \rightarrow H + X$, $D + X \rightarrow DX$. This process thus restricts the technique to exceptionally stable molecules, such as H_2O, CH_4, NH_3. Moreover, the temperature range is limited to fairly high values—700° C. and upwards—which in turn introduces a certain amount of complication in that heterogeneous reactions often intrude and so upset the accuracy of the measurements of the homogeneous part of the reaction. The third method has some marked advantages since it may be operated over a wide temperature range, namely, from 0° C. to temperatures at which thermal exchange becomes appreciable. Here, too, there are unfortunately some disadvantages. The most serious is that of determining the

stationary concentration of atoms. One method consists in measuring the number of quanta exciting the mercury atoms per second, I, and calculating the value of [D] from the relationship $[D] = (2I/k[X])^{\frac{1}{2}}$, where k is the velocity constant of the reaction $D + D + X$, [X] being the total gas pressure. k is known from separate experiments. Conditions, of course, must be chosen such that all D atoms combine in the gas phase. Alternatively the hydride may be mixed with ortho deuterium and the rate of the photosensitised conversion used to compute the stationary atom concentration. This only applies provided the energy of activation of the reaction $D + D_2$ (ortho) $\rightarrow D_2$ (para) $+ D$ is smaller than that of $D + HX$, in which case the latter reaction occurs more easily and the deuterium atoms are replaced by hydrogen. There is one further complication. It may happen that the hydride itself is dissociated on colliding with excited Hg atoms. Without going into details the contribution made by this factor to the total exchange can be allowed for, if the rate of deactivation Hg atoms by the hydride is not too great compared with that of deuterium. Emphasis must finally be laid on a useful feature of the sensitised technique. The sequence of the reactions is: (a) $D + HX \rightarrow DX + H$, (b) $H + D_2 \rightarrow HD + D$, (c) $D + D + X \rightarrow D_2 + X$. If the probability of (a) occurring is greater than that of (c) it is evident that one deuterium atom will effect exchange among a number of hydride molecules. In other words, the quantum yield of the reaction, i.e. the number of DX molecules produced per quantum absorbed, will exceed unity. This is uniquely characteristic of this type of exchange reaction and in cases of doubt settles unambiguously the question of the mechanism.

From the foregoing, then, it is apparent that the three methods are really complementary, each providing some information which the other cannot yield. The results are collected together in Table I.

In so far as the molecules H_2O, NH_3, CH_4 are concerned, the different methods give results in very good agreement. It will be noticed that the $D + H_2$ reaction requires the smallest energy of activation. Furthermore, the steric factors for these reactions are all of the order 0·1, so that the deuterium atom must approach the hydride molecule within a fairly small specified solid angle before exchange will take place. Besides the reactions of the hydrides there are the analogous reaction of H atoms with deuterides. These in general proceed more slowly, requiring a greater energy of activation. The comparison of the types will be discussed below.

Next we pass to another mechanism whereby exchange may occur.

TABLE I.

Reaction.	Energy of Activation (kg. cal.).	Method.	Reference.
D + H$_2$	6	Thermal	Farkas and Farkas, *Proc. Roy. Soc.*, A, 152, 124 (1935).
D + H$_2$O	7	Electrical	Geib and Steacie, *J. phys. Chem.*, B, 29, 215 (1935).
	7	Photo	Melville and Farkas, *Proc. Roy. Soc.*, in the press.
D + NH$_3$	11	Electrical	Geib and Steacie, *loc. cit.*
		Thermal	Farkas, *J.C.S.*, 26, (1936).
		Photo	Melville and Farkas, *Proc. Roy. Soc.*, in the press.
D + PH$_3$	14	Photo	Melville and Bolland, in the press.
D + C$_2$H$_2$	5	Electrical	Geib and Steacie, *loc. cit.*

Mention has already been made of the fact that, should the hydride be simultaneously decomposed either thermally or by excited Hg atoms, there is the possibility that the deuterium atom and the fragment of the hydride may recombine to yield the deuteride. This in turn is bound up with another question in photochemistry. Hydrides, such as ammonia and phosphine, have predissociation absorption band spectra, which means that these molecules are primarily dissociated on absorbing a quantum of radiation. The quantum yield of the reaction should be unity, whereas in fact it is respectively 0·25 and 0·5. To account for these low values, it must be supposed that some back reaction comes into play whereby NH$_3$ and PH$_3$ are regenerated. Since molecular hydrogen is not an inhibitor of the photodecomposition, it would seem that the primary reactions are : $NH_3 \xrightarrow{hv} NH_2 + H$ and $PH_3 \xrightarrow{hv} PH_2 + H$. One way of showing that the presumption is correct consists in artificially increasing the H atom concentration and observing whether the decomposition is retarded. This can be done by decomposing the NH$_3$ or PH$_3$ with the light from a zinc spark at 2100 Å and generating the additional atomic hydrogen by means of excited mercury atoms. The expected retardation is well marked. But the demonstration can be carried a whole stage further by using deuterium instead of hydrogen when exchange occurs owing to the back reaction (Taylor and Jungers, *J. Chemical Physics*, 2, 452, 1934), and can in fact be made the basis for measuring the quantum yield of such a reaction by a method completely independent of the determination of the number of quanta absorbed (Melville and Bolland, in the press). The experiments must of course be made at temperatures not exceeding 60° C., when the atomic exchange reaction may be neglected.

Briefly, the method adopted is the following: The hydride—NH_3 or PH_3, is mixed with D_2. On illumination the H atom reacts with the D_2 molecule $- H + D_2 = HD + D$, and therefore the rate of the back reaction is simply measured by the rate of the exchange reaction. Simultaneously the rate of decomposition is also measured. It may then be shown that

$$\frac{\text{Decomposition}}{\text{Exchange}} = 3\sqrt[4]{2}\,\frac{\gamma}{1-\gamma},$$

where γ is the quantum yield. A single measurement of the ratio of decomposition to exchange thus provides the data for the calculation of γ. For example, with PH_3 $\gamma = 0.50$ by direct measurement and 0.52 by the exchange technique. The method may likewise be applied to mercury sensitised reactions.

The Relative Rates of Reaction of Hydrogen and Deuterium.—There are four factors which may cause the velocity of deuterium reactions to be different from those of hydrogen. They may be conveniently classified as follows:—

(a) Collision frequency difference, the factor being the ratio of the reduced masses of the colliding systems.

(b) Difference in collision radii.

(c) Difference in energy of activation due to zero point energy.

(d) Quantum mechanical leakage.

The first factor is easily allowed for, if the nature of the reaction is known exactly. The importance of the second factor is rather more difficult to assess. From measurements which have been made of the viscosity and thermal conductivity of H_2 and D_2 (Maas and Van Cleave, *Can. J. Res.*, **12**, 57; 372 (1935)) it is certain that these molecules have the same collision diameters within 1 per cent. Similarly there is good reason to believe that the collision diameters of hydrides and deuterides will have identical values. Such measurements, however, only refer to the transfer of relatively small amounts of energy—$< kT$—from one molecule to another. In practice, that is, in activating collisions, the energy transfers are much larger, of the order 5 kg. cal. upwards, *i.e.* 10 times kT, if T = 500° K. In such cases, then, the radii may not be identical. So far measurements have only been made of the transfer of energy from excited mercury atoms to hydrides and deuterides. Two types of reaction may take place (Evans, *J. Chem. Physics*, **2**, 486, 1934). The atom may be deactivated to the normal state, in which case 112 kg. cal. are transferred, or it may be deactivated to a metastable state lying only 5.0 kg. cal. below the excited state. In both transitions there are appreciable differences in the quenching radii—σ. H_2,

D_2, PH_3 and PD_3 all quench to the ground state. In each pair the deuterium compound is the more efficient; for example,

$$\sigma^2_{Hg-H_2} = 6\cdot01, \ \sigma^2_{Hg-D_2} = 8\cdot5 \times 10^{-16} \text{ cm.}^2,$$

and for the light and heavy phosphines the respective values are $26\cdot2$ and $29\cdot5 \times 10^{-16}$ cm.^{2}. On the other hand, water and ammonia quench mostly to the metastable level, the hydride being the more efficient quencher, as is shown by these figures:

$$\sigma^2_{HgH_2O} = 1\cdot00, \ \sigma_{Hg-D_2O} = 0.46, \ \sigma_{HgNH_3} = 2\cdot94, \ \sigma_{Hg-ND_3} = 1\cdot09.$$

The probable explanation in the first case is that transfer is most likely when the energy available is exactly taken up by the quenching molecule. Thus the energy of dissociation of D_2 is 104 compared with 102 kg. cal. for H_2, the mercury supplying 112 kg. cal. Qualitatively then, D_2 should be a more efficient quencher than H_2. In a similar way approximately 1 kg. cal. more energy is required to remove a D than an H atom from phosphine, and again therefore the deuterophosphine should be more efficient as is found in practice. The same principle applies to quenching to the metastable state. In this case, however, the energy available is taken up by the vibrational degrees of freedom of the molecules, and hence the quenching radius is dependent upon how close a vibrational level lies to $5\cdot0$ kg. cal., the energy available from the Hg atom. For example, the nearest level for NH_3 is at $4\cdot6$ kg. cal., and for ND_3 at $6\cdot6$ kg. cal. NH_3 is therefore more efficient.

Zero Point Energy Effect.—In activating collisions between molecules the requisite energy may be derived from energy of translation, vibration and rotation. The first factor can easily be allowed for, and since at temperatures above about 300° K. the rotational states of any molecule are fully excited, as is shown by the equality of specific heats, *e.g.* of H_2 and D_2, the second factor is the only one in which marked differences between hydrides and deuterides are to be expected. The vibrational energy E_ν of a polyatomic molecule due to a particular frequency is given by a general expression of the type $E_\nu = (n + \frac{1}{2}) \, h\nu$, assuming for simplicity that the vibration is harmonic. Since E_ν can only change by integral multiples of $h\nu$, it is evident that the smallest energy residing in this vibration is $\frac{1}{2}h\nu$. This is termed the zero point energy, for it is the energy the molecule would retain as the absolute zero is approached. ν, however, is proportional to $(f/\mu)^{\frac{1}{2}}$, where f is the force constant of the vibration under consideration and μ the reduced mass of the system. f is unchanged when deuterium is substituted for hydrogen, while μ can be calculated if the masses are known. For H_2 and D_2, $\mu_H/\mu_D = \frac{1}{2}$. The zero point energy of deuterium

compounds will thus be at most $\sqrt{2}$ times smaller than those of hydrogen. The important point is that because of the large magnitude of the vibrational quanta, the vibrations are not fully excited at ordinary temperatures, and therefore the difference in vibrational energy content of isotopic molecules at the same temperature is in general considerable. A numerical example will make the matter clear. The zero point energy of the H_2 molecule is 6·2 kg. cal., and the energy of the first vibrational quantum 11·8 kg. cal. The probability of an H_2 molecule possessing only one quantum of vibrational energy is therefore $e^{-11800/RT}$. If $T = 600°$ K., this probability is 1/20,000. At normal reaction temperatures the vibrational energy of hydrides and deuterides is mainly zero point energy. Next the question arises as how this will affect the energy of activation of any given reaction, for example, that of Br atom with H_2 and with D_2 respectively. Will the energy of activation for Br + D_2 be the same as that for Br + H_2, or will it differ by the difference in zero point energy between H_2 and D_2? The problem is to determine the difference in energies of the systems in the transition state. It has been partially solved experimentally and a number of attempts have been made at a theoretical solution, for there are difficulties in both methods of approach. For the sake of illustration, we may again consider the formation of hydrogen bromide. The respective energies of activation for Br + H_2 and Br + D_2 are 17·7 and 19·8 kg. cal. The experimental error judged from the consistency of the results is $\pm$ 0·5 kg. cal., and hence, though it may be said that the energy of activation for the deuterium reaction is greater, the accuracy of the measurements is not sufficient to determine the difference between the energies in the activated state. For the present purpose the following procedure is adopted. The relative rates of the hydrogen and deuterium reactions can be measured with an accuracy of a few per cent. at any one temperature. It is then assumed that, after correction for a collision factor, the difference in rates is wholly due to a difference in energy of activation—a natural assumption on the ordinary collision theory of activation. Therefore

$$R_H/R_D = (\mu_D/\mu_D)^{\frac{1}{2}}e^{\Delta E/RT},$$

where ΔE is the difference in energies of activation and R_H/R_D is the ratio of the rates. The results of all the relevant experiments are shown in Table II. The first important general point to observe is that the difference in zero point energy in the activated state is not negligible, and secondly that, by employing this method of calculation, the difference is never negative. The latter observation

TABLE II.

Reaction.	E for Hydrogen Reaction (kg cal).	ΔE from R_H/R_D (kg cal.).	Z P E. diff. Initial State (kg cal.).	Z P E. diff. transition State
Cl + H₂ [1] . . . Cl + D₂	*ca.* 6	1·40	1·78	0·38
Br + H₂ [2] . . . Br + D₂	17·2	1·44	1·78	0·38
H + H₂ [3] . . . H + D₂	5·5	1·05	1·78	0·73
D + H₂ [3] . . . D + D₂	4·85	1·16	1·78	0·62
H + N₂O [4] . . . D + N₂O	13·8	0·00	0·00	0·00
D + NH₃ [5] . . . H + ND₃	10·8	1·23	5·49	4·26
D + PH₃ [6] . . . H + PD₃	14·4	0·60	3·05	2·45
Na + HCl [7] . . . Na + DCl	6·1	0·30	1·2	0·8
HI + HI [8] . . . DI + DI	42·5	0·58	1·90	1·30
H₂ + I₂ [9] . . . D₂ + I₂	40·0	1·24	1·78	0·56
H₂ + C₂H₄ [10] . . D₂ + C₂H₄	43·1	1·49	1·78	0·29
H + H + H [11] . . D + D + D	0	0	0	7
H₂ + H₂ + NO [12] . D₂ + D₂ + NO	45	0·0	3·56	3·56
O + PH₃ [13] . . O + PD₃	0·6	0·0	3·05	3·05

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demonstrates conclusively that the difference in the zero point energy in the initial state is more than adequate to provide for the difference in energies of activation observed. This is just what would be expected qualitatively, for the transition complex differs

from a normal molecule in that one vibrational degree of freedom is missing, the reaction occurring in that degree of freedom. It is here that an advance in the theory is necessary in order that an *a priori* calculation of the energy of the transition complex may be made. If the calculations could be made sufficiently precise they would then show whether the simple collision theory is valid for computing the difference in zero point energies of the transition complex.

One or two striking features of Table II may be pointed out. In the $Cl + H_2$ and $Br + H_2$ reactions, the difference in energy of activation is slightly less than that between H_2 and D_2. In the $H + H_2$ and $H + D_2$ reactions, the same behaviour will be noticed. Replacing the H_2 molecule by N_2O, however, makes the energies of activation of $H + N_2O$ and of $D + N_2O$ identical. With more complex molecules, such as NH_3 and PH_3, the difference in activation energies for the exchange reactions of the isotopic molecules remains small, though the zero point energy difference in the initial state is now considerable. The other reactions fall into the general scheme. With ethylene it is noteworthy that the difference in energies of activation approach 1·8 kg. cal., probably a general rule when complicated molecules are hydrogenated by H_2 and by D_2. Such a reaction differs from all others in that only one molecule emerges from the activating collision.

Ternary collisions such as $H + H + H \rightarrow H_2 + H$ and $D + D + D \rightarrow D_2 + D$, requiring no energy of activation, proceed at relative rates wholly explained by the operation of the collision frequency factor. Even the ternary reactions $H_2 + H_2 + NO$ and $D_2 + D_2 + NO$, though requiring 40 kg. cal. of activation, go at identical speeds apart from the difference due to the greater speed of the H_2 molecules. Likewise the oxidation of PH_3 and of PD_3 by O atoms have identical energies of activation.

Finally, brief mention may be made of the so-called tunnel or leakage effect. Classical theory prohibits reaction if the energy available for activation does not exceed the critical amount necessary, *i.e.* the energy of activation. Quantum theory, however, permits reaction if this condition is not fulfilled. Under such circumstances the rate of reaction is an exponential function of the mass of the colliding particles; and furthermore, the energy of activation is quite markedly dependent on temperature. This means that the relative rates of hydrogen and deuterium reactions may far exceed 1·4 : 1, and indeed exceed that to be expected if there were no difference in zero point energy in the activated state (cf. Bawn and Ogden, *Trans. Faraday Soc.*, **30,** 432, 1934). Up

to the present no evidence whatsoever has been brought forward to support the hypothesis, and therefore it may be concluded that such a mechanism does not play any important part in determining the rate of the majority of chemical reactions in the gas phase.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., D.Sc., The University, Glasgow.

METAMORPHISM AND METAMORPHIC ROCKS.—The paper by D. T. Griggs on " Deformation of Rocks under High Confining Pressures. I. Experiments at Room Temperature " (*Journ. Geol.*, XLIV, 1936, 541–77) gives the first results of a programme of experimental research on the physical processes underlying rock deformation. The range of pressure investigation has been extended to four times the amount of pressure used in the classic experiments of F. D. Adams. The strength of rocks under high confining pressure was investigated with unexpected results. Contrary to former belief, the experiments show that when a rock enters the region of plastic flow it will not be indefinitely deformed, but will rupture if the deformation is carried far enough. The application of these and other results to geology is not immediate, but must await the evaluation of other factors such as time and temperature.

Experimental results obtained by P. W. Bridgman on shearing phenomena at high pressures in their possible application to geology have proved to be both varied and unexpected (*Journ. Geol.*, XLIV, 1936, 653–69). Bridgman states that " any (geological) speculation which hinges . . . on whether a substance hardens or softens under prolonged plastic flow, or whether rupture in shear becomes more or less frequent at high pressures, must for the present be held in abeyance."

H. W. Fairbairn discusses the mechanics of rock foliation with the aid of Sander's " petrofabrics " method (*Journ. Geol.*, XLIII, 1935, 591–608). He finds that secondary foliated surfaces may have different origins, in some cases being " plaiting " surfaces, in others " shear " surfaces. Fairbairn supports his views with X-ray data derived from examination of a Thuringian slate.

Field work on the Shabu sediments in north-western Ontario have furnished the same author with data on elongation in deformed rocks (*Journ. Geol.*, XLIV, 1936, 670–89). Two directions of elongation were found, one shown by folding and the other by parallelism of linear elements to the fold axes, which were normal to each other and unequally developed. Sander's hypothesis of a shear couple normal to the axial lines of the folds, combined with intergranular rotational movements, seems best to explain the dual elongation.

J. de Lapparent, in his paper " Rythmes du métamorphisme dans les Highlands " (*Bull. Soc. Geol. France* (5), V, 1935, 281–317), has written a valuable account of his impressions concerning metamorphic conditions in the Scottish Highlands gained during the recent excursion of the International Pre-Cambrian Association. It is not possible here to deal with the points he raises, but he believes that confusion is caused by placing a " biotite zone " between the chlorite and garnet zones of metamorphism. Biotite can occur equally well in both these zones, and the development of garnet always precedes that of biotite.

In his valuable investigation " The Alteration of Coal Seams by the Intrusion of . . . Igneous Dykes in the Northumberland and Durham Coalfield," C. E. Marshall (*Trans. Inst. Min. Eng.*, XCI, Pt. 3, 1936, 235–60) shows that the change in the coal is not gradual but exceedingly abrupt, and is usually complete within ten feet of the contact with the igneous rock. This natural coking is quite unlike the changes produced in coal during its regional elevation from bituminous to anthracitic rank. It is therefore concluded that such changes of rank have not been produced by a sudden or considerable rise in temperature.

Thermal metamorphism of sediments at the contact of the Ben Bullen plutonic complex of New South Wales has given rise to cordierite-hornfels and to magnesian and non-magnesian marbles. In her study of these rocks Miss G. A. Joplin (*Geol. Mag.*, LXXII, 1935, 385–400) shows that interesting dolomite coronas about brucite, and dactylitic intergrowths between calcite and dolomite, have been produced in the marbles.

Describing " The Endogenous Contact-Zone of the Magnesian Limestone at Ben Bullen, N.S.W.," Miss Joplin (*Journ. and Proc. Roy. Soc. N.S.W.*, LXIX, 1936, 138–58) shows that contaminated rocks have been formed at the junction of quartz-mica-diorite with limestone of variable magnesian content. All the contaminated rocks may be explained by a gradual increase in the amounts of MgO and CaO added to the magma through the assimilation first of limestone, and then by the activity of solutions derived both from the limestone and the magma.

Pursuing her studies of contact metamorphism in New South Wales, Miss Joplin describes the metamorphic types produced from the Lambian Series (Upper Devonian), consisting of arenaceous, argillaceous and areno-calcareous sediments, with intercalated acid and intermediate lavas, at their contact with the Hartley-Bathurst batholith (*Proc. Linn. Soc. N.S.W.*, LX, 1935, 16–50). Sixteen primary hornfelses are described and related to Goldschmidt's

classification. The Hartley aureole is an example of a medium-grade " wet " type of metamorphism.

T. Watanabe has described an interesting brucite-marble or predazzite from the Nantei Mine, Suian, Tyôsen (Korea) (*Journ. Fac. Sci. Hokkaido Imper. Univ.*, Ser. IV, III, No. 1, 1935, 49–59).

J. H. Taylor describes " A Contact Metamorphic Zone from the Little Belt Mountains, Montana " (*Amer. Min.*, **20**, 1935, 120–8), in which a gabbro intrusion has caused the formation of pyroxene-prehnite-calcite rocks at the immediate contact, and a zone of light silicates in which vesuvianite and garnet are the most common minerals, farther out. The endomorphic alteration of the gabbro has resulted in the substitution of orthoclase for labradorite.

" The Contact Metamorphism of the Onawa Pluton, Piscataquis, Co. Maine," is described by S. S. Philbrick (*Amer. Journ. Sci.*, XXXI, 1936, 1–40). The intruded rock is slate which has been metamorphosed with the production of a triple-zoned contact aureole composed of andalusite-schist, hornfels and injection-hornfels, the last-named zone occurring at the immediate contact.

Discussing the progressive kinetic metamorphism of the Missi Series (Pre-Cambrian conglomerates and greywackes) at Flinflon, Manitoba, J. W. Ambrose (*Amer. Journ. Sci.*, XXXII, 1936, 257–86) comes to the conclusion that the heat necessary for metamorphism was developed mechanically by and during the intimate internal shearing that the rocks have suffered.

The regional and contact metamorphism of quartzites, greywackes, slates and argillites of Ordovician age is described by W. N. Benson and J. A. Bartrum in a paper entitled : " The Geology of the Region about Preservation and Chalky Inlets, South-west Fiordland, N.Z. Part III, Petrology " (*Trans. Roy. Soc. N.Z.*, **65,** 1935, 108–52). A long series of types ranging from phyllites to paragneisses and amphibolites, and from hornfels to sillimanite-schist, has been produced by dynamothermal metamorphism and by the contact metamorphism induced by the intrusion of large plutonic masses.

Further studies of progressive regional and dynamothermal metamorphism in New Zealand are contained in the following papers : J. A. Bartrum, " Metamorphic Rocks and Albite-rich Igneous Rocks from Jurassic Conglomerates at Kawhia " (*Trans. Roy. Soc. N.Z.*, **65**, 1935, 95–107) ; F. J. Turner, " Metamorphism in the Te Anau Series in the Region N.W. of Lake Wakatipu " (*ibid.*, 329–49) ; C. O. Hutton and F. J. Turner, " Metamorphic Zones in North-west Otago " (*ibid.*, 405–6).

H. von Eckermann's detailed memoir on " The Loos-Hamra

Region " (*Geol. For. Forh.*, *Stockholm*, **58**, 1936, 129–343) deals with the geology and petrology of a region in the Swedish Pre-Cambrian. It contains a great amount of valuable material on the petrology of metamorphic rocks which it is impossible to summarise here. The memoir is enriched by the publication of 114 new chemical analyses, of which the majority are of metamorphic types. A long review of this work by C. E. Tilley is published in the *Geol. Mag.* (LXXIII, 1936, 379–82).

The paper by N. H. Magnusson on " The Evolution of the Lower Archæan Rocks in Central Sweden and their Iron, Manganese and Sulphide Ores " (*Quart. Journ. Geol. Soc.*, XCII, 1936, 332–59) is a valuable summary for English readers of recent work on the Archæan of Central Sweden. The metamorphism of these rocks is described, and the origin of the iron and manganese ores of the Leptite formation is fully discussed.

The peculiar banded gneisses which intervene between the massive diorite of Ornö (Sweden) and the leptite country-rock have been interpreted as primary gneisses of igneous origin, and also as merely a part of the Leptite formation. E. Wenk (*Bull. Geol. Inst. Upsala*, XXVI, 1936, 53–89), on the basis of investigation by Sander's petrofabrics method, now interprets them as secondary tectonic phenomena due to a simultaneous process of deformation and crystallisation.

K. H. Scheumann has published a valuable paper, " Über eine Gruppe bisher wenig beachteter Orthogneise des Granulitgebirge und deren Einschlichtung " (*Min. u. Petr. Mitt.*, **47**, 1936, 403–69), describing in minute detail the effects of intense mechanical movement on coarse gneiss. The red gneiss of the Saxon Granulitgebirge represents relics of biotite-gneiss (Erzgebirge variety) which have been left on the main horizon of movement. The marginal granulite zone is shown to have been derived from these rocks. Scheumann's paper on " Die Rotgneise der Glimmerschieferdecke des Sächsischen Granulitgebirges " (*Ber. Math.-Phys. Kl. Sachs. Akad. Wiss., Leipzig*, LXXXVII, 1935, 251–86) is a wider treatment of the same group of rocks.

A. Fiedler discusses the origin of certain amphibolites in his paper entitled : " Über Verflössungserscheinungen von Amphibolit mit diatektischen Lösungen im östlichen Erzgebirge " (*Min. u. Petr. Mitt.*, **47**, 1936, 470–516). The initial materials were probably basic lavas. Associated eclogites are regarded as an early phase of the metamorphic readjustment. The amphibolites are due to intimate interpenetration of the rocks by pegmatitic solutions.

In his memoir " Die Regelung von Quarz und Glimmer in den

Gesteinen der Finnisch-Lapplandischen Granulitformation " (*Bull. Comm. Géol. Finlande*, No. 113, 1936, 110 pp.), T. G. Sahama (Sahlstein) has applied the Sander petrofabrics method with great success to the elucidation of the structural relations of the Leptite or Granulite formation of Finnish Lappland. His results are too extensive and complicated to be indicated in this place.

T. G. Sahama discusses the " Petrographie der Eklogiteinschlüsse in den Gneisen des südwestlichen Liverpool-Landes in Ost-Grönland " (*Medd. om Grønland*, 95, Nr. 5, 1935, 43 pp.) with the aid of three new analyses. The eclogites occur as blocks and lenses in a garnet-pyroxene-gneiss, and represent basic intrusions into the gneisses. Powerful internal movement and metamorphism at high temperature have converted these basic rocks into eclogites.

H. G. Backlund has published a suggestive paper, " Zur genetischen Deutung der Eklogite " (*Geol. Rundsch.*, XXVII, 1936, 47–61). He thinks that eclogites are derived from basalts under conditions of increasing kinetic metamorphism in geosynclinals, with the aid of the " migmatite front " that advances at the outset of the revolution phase. The series of changes runs : basalt—uralite-porphyrite — greenstone — amphibolite — garnet-amphibolite — true (*i.e.* pyroxene) eclogite.

J. Parkinson's " Notes on the Petrology of the Gadabursi Country, British Somaliland " (*Geol. Mag.*, 73, 1936, 365–78), deal with a series of metamorphosed sediments, now mica-schists and paragneisses, associated with a group of porphyritic lavas ranging in composition from rhyolite to dacite ; and a complex group of coarse gabbroid epidiorites, amphibolites and hornblende-schists, of probable Upper Archæan ages.

The major part of C. S. Pichamuthu's paper on " The Iron Formations and Associated Rocks of the Eastern Bababudans, Kadur District, Mysore " (*Half-yearly Journ. Mysore Univ.*, VIII, No. 1, 1935, 1–48), is devoted to the study of a peculiar banded ferruginous quartzite consisting of fine layers of silica and iron oxides, along with the remarkable blue soda-amphibole bababudanite, of which a description and new analysis is given by Pichamuthu in another paper (*Geol. Mag.*, LXXIII, 1936, 39–45). The current theory of the origin of the banded iron formations is that they are metamorphosed derivatives of basic igneous rocks. Pichamuthu shows that this view is untenable, and that these rocks exhibit sedimentary characters, the iron oxides having been deposited as chemical precipitates.

C. E. Wegmann's important memoir, " Zur Deutung der Migmatite " (*Geol. Rundsch.*, 26, 1935, 305–50), is a comprehensive study

of the occurrence, movements, tectonic activity and origin of migmatite, the granitised material which often intervenes between a granite intrusion and its country-rock. Migmatitisation of a region causes the formation of a dome structure by mass expansion. The dome may be diversified by secondary ridges due to differential expansion of the more highly granitised portions of the country-rock.

The last memoir by the late J. J. Sederholm, " On Migmatites and Associated Pre-Cambrian Rocks of South-western Finland, Part III, The Åland Islands " (*Bull. Comm. Géol. Finlande,* Nr. 107, 1934, 68 pp.), has been prepared for publication by E. Mikkola, his collaborator in the field work. The Åland Islands are composed of the oldest supracrustal rocks (Svionian) and the associated gneissic granite, Bothnian schists, Post-Bothnian granites and still later granites (of the Third Group), with small outcrops of certain post-Archæan rock groups. The migmatites which form the main subject of the memoir are due to the interpenetration of post-Bothnian granites into the earlier gneissic granites, the Svionian schists, and the Bothnian schists respectively. The memoir is full of detail concerning metamorphic petrology which is illustrated by a large number of new chemical analyses.

Describing the " Migmatites of the Sawatch Range, Colorado," J. T. Stark (*Journ. Geol.,* XLIII, 1935, 1–26) states that the crystalline core of this range is composed of coarse schist and gneiss largely of sedimentary origin, into which two granitic batholiths of Pre-Cambrian ages are intruded. Wide zones of migmatite occur between the plutonic and the gneissic rocks. The regional granitisation is due to gaseous or liquid emanations from the invading batholiths.

The " Crystalline Complex of Liverpool Land " (East Greenland) is characterised by E. H. Kranck (*Medd. om Grønland,* 95, Nr. 7, 1935, 122 pp.) as an area of migmatitised gneisses wherein all formations of supracrustal origin have been thoroughly injected with granitic rocks. This basement has been penetrated by several presumably Caledonian granitoid masses. The petrography of both metamorphic and igneous rocks has been studied in detail. The author makes an interesting comparison and correlation between Liverpool Land and Sutherland, expressing the opinion that the basal gneisses of Liverpool Land probably belong to the same series as the Moine Gneiss of Scotland.

In his study " Zur Kenntnis der Injektionsmetamorphose im westlichen Norwegen," N. H. Kolderup (*Bergens Mus. Aarbok,* 1935, Naturvid. Raekke, Nr. 3, 39 pp.) deals with injection metamorphism in green schists, in argillaceous rock types, and in

carbonate rocks respectively, and compares western Norway in this respect with the southern margin of the Gotthard massif. An interesting point is that staurolite, kyanite and chloritoid may have been formed by injection metamorphism of argillaceous rocks.

The " Metasomatism associated with the Greenstone-hornfelses of Kenidjack and Botallack, Cornwall " is now regarded by C. E. Tilley (*Min. Mag.*, XXIV, 1935, 181–202) as due to hot solutions emanating from a granitic source, which have passed through the greenstones removing lime and producing anthophyllite and related types of hornfels. A different hypothesis of origin based on intensive weathering of the greenstones followed by shearing was entertained in an earlier paper (see SCIENCE PROGRESS, Oct. 1931, 211).

The " sanidinites " of the Lake of Laach district in the Eifel are due to the contact-metasomatism of crystalline schists. According to G. Kalb (*Min. u. Petr. Mitt.*, **47**, 1936, 185–220) there are two sharply contrasted types, ægirine-augite-sanidinite, due to the action of pneumatolytic injections from a phonolitic magma, and biotite-sanidinite, arising from similar action by a trachytic magma.

The same conclusions have been arrived at and reinforced by the study of inclusions in the leucite-phonolite tuff of the Kappiger Ley (*ibid.*, **48**, 1936, 1–26).

" Metamorphic Differentiation at Contacts between Serpentinite and Siliceous Country Rocks " has been investigated by A. H. Phillips and H. H. Hess (*Amer. Min.*, **21**, 1936, 333–62). The strong chemical contrast between the adjacent rocks, and the tendency for the contact to be traversed by abundant hydrothermal solutions, lead to the formation of a characteristic zonal arrangement of minerals, mainly talc, chlorite and biotite, around the ultrabasic igneous rock parallel to its contacts. The conditions affecting the transfer of material across the contact are worked out in detail in this paper.

F. J. Turner has petrographically described the nephrites, serpentines, talc-rocks and related greenstones which are used by the Maoris for tools, weapons and ornaments, and has endeavoured to trace the sources of these rocks (*Trans. Roy. Soc. N.Z.*, **65**, 1935, 187–210).

BOTANY. By PROFESSOR E. J. SALISBURY, D.Sc., F.R.S., University College, London.

AN interesting addition to our detailed knowledge of the life history of wild species is furnished by K. Linkola in *Acta Forestalia Fennica*, **42**, Helsinki, 1935. This is an account of the duration of the

juvenile state in various meadow species of Finland. This vegetative period may be very prolonged and, as a result of his observations, the author estimates that the minimum period prior to flowering is from two to three years for *Prunella vulgaris* ; for *Potentilla erecta*, four years ; for *Ranunculus auricomus, Chrysanthemum leucanthemum*, and *Polygonum viviparum*, five years ; for *Ranunculus acer*, six years ; and no less than eight years for *Trollius europœus, Alchemilla pastoralis* and *Geum rivale*. When, however, these same species are grown from seed in garden beds in the absence of competition the adult condition was attained in a year or less, except in the case of *Trollius europœus*, which flowered in two years. The prolonged immaturity in the meadow vegetation was accompanied by a slow rate of growth and particularly by a poorly developed root system, an effect of competition to which the reviewer has drawn attention (*Jour. Ecol.*, 1929), and which unpublished work by Varma indicates as due in part at least to soluble substances produced by the live or dead roots of the competing species. Linkola points out that a rapid attainment of the adult condition is shown by these same species when growing under natural conditions in the absence of competition. The numbers of individuals of the various age classes observed showed a rapid diminution in the first season, amounting in *R. auricomus* and *R. acer* to about 80 per cent. For *Potentilla erecta* the diminution during the first year was not so marked, about 50 per cent. only, but all the species showed rapid decrease in the first year, followed by a steady diminution in the later juvenile stages. Seeing that, as the present writer has shown, the juvenile stages are the most significant in relation to mortality, both as regards its magnitude and character, the retardation in natural communities may have important effects in relation to survival and selection.

The question as to why so few plants other than members of the Leguminosæ bear root nodules is an interesting one and is obviously correlated with the similar problem as to the reason for the freedom from nodules of some members of the Leguminosæ. O. N. and E. K. Allen (*Soil Science*, **42**, 87, 1936) have examined a number of members of the tribe Caesalpinioideæ and report the absence of nodules from three species of *Saraca*, two of *Schotia, Tamarindus indica*, three species of *Bauhinia*, nine species of *Cassia*, four species of *Caesalpinia*, and from *Peltophorum inerme, Mezoneurum kauaiense*, and *Delonix regia*. The number of individuals examined in each species ranged from three to several hundreds, so that it would seem certain that the absence of nodules in this tribe is widespread and frequent, though the fact that some members of the family

have been reported by some investigators to be devoid of nodules and by others to possess them (*e.g. Mimosa pudica*) indicates the necessity of caution in generalising on this point. The suggestion made by Nobbe that the physical character of the root hairs may inhibit nodule formation is rendered plausible by the work of Thornton, which, however, suggests that physiological as well as physical characteristics may be involved.

The seasonal changes in moisture content of certain loamy soils of Palestine are described by F. Menchikovsky in relation to the vegetation which they bear (*Soil Science*, **42**, 167, 1936). The average annual rainfall of the region is about 500 mm. Most of this falls in the period from November to May. In the first 25 cm. the water-content ranges from a minimum of 9·5 per cent. to a maximum in the rainy season of 25 per cent. In the dry season the low moisture value is maintained for several months and rises abruptly with the onset of the rainy period. Bulbous plants occupy this stratum and are active only during the humid period. Between 50 cm. and 75 cm. depth the water content is never high, but exhibits little fluctuation, being about 15·7 per cent. during the dry and 17 per cent. during the rainy seasons. From 75 to 100 cm. depth the moisture is also fairly constant at from 16·3 per cent. to 19·1 per cent. The Perennial flora, which begins its development in the wet season, persists through the dry, and the constituent species have roots occupying depths from 50 to 100 cm. Cereal crop plants on these soils have an ash content which for wheat and barley is nearly double that of these species in Europe and America, whilst the ash content of maize is more than three times that in the U.S.A.

An interesting record from the point of view of geographical distribution is the occurrence of the Oceanic species *Bartsia viscosa* in Norfolk near Hunstanton, recorded by Mr. Tutin (*Jour. Bot.*, LXXIV, 297). This adds one more to the Oceanic species which occur in the extreme east of Norfolk, and emphasises the relatively abrupt contrast between the eastern and western parts of the county.

A volume of essays in Geo-Botany dedicated to Professor Setchell contains a number of interesting contributions. Amongst these Professor Clements deals with the desert climax which is regarded as characterised by the dominance of *Larrea tridentata* and, though having a wide range, is held to be climax only in western Arizona, south-eastern California and Mexico, with a climate characterised by less than 5 inches of rainfall. The present desert area has become extended by various factors other than

climatic ones. Relic grasses occur within the desert domain which bear witness to former grassland communities. The desert endemics of to-day comprise 89 species of annuals, of which the most largely represented genera are *Phacelia cryptanthe, Gilia euphorbia, Langloisia* and *Mentzilia*. Endemic perennials number 78, of which *Astragalus* is the most important genus, whilst of the 69 endemic shrubs the genera *Opuntia, Dalea,* and *Salvia* are the best represented.

Professor Fritsch contributes an account of the rôle of the terrestrial alga in nature which is a valuable summary of existing knowledge on this subject. Three communities, characterised by increasing desiccation, are recognised, *viz.* the subterranean, the surface community, and the aerial community. The subterranean community, in which *Chlorococcum humicolum* and Hormidium spp. are constants, include some algæ which appear to be exclusive, such as *Gongrosira terricola* and Xanthophyceæ. But Fritsch regards the subterranean alga flora as in large part, if not entirely, composed of surface algæ washed down by rain. This may apply even to those species which have so far only been obtained from beneath the surface, since the composition of the surface community has not yet been intensively studied.

E. M. Merrill in the same volume emphasises the relationships between the polynesian flora and that of Malaya, while Scottsburg treats of the Antarctic plants in Polynesia. Professor Rubel contributes an English account of his classification of the plant communities of the world.

An account of twelve species of the Polish flora which do not reproduce sexually but by bulbils is furnished by Z. Radwauska-Kuleszyna in *Acta Soc. Bot. Poloniæ,* XI. The author suggests probable causes of the absence of seed production. In *Ficaria ranunculoides* the pollen and ovules are formed normally, but pollination is stated to be very rare. The pollen of *Dentaria bulbifera* exhibits very poor germination and the pollen tubes tend to degenerate, whilst in *Lilium bulbiferum,* although the pollen grains germinate well, the pollen tubes are very short relative to the length of style which they have to traverse to effect fertilisation. In *Allium carinatum* and also in *Allium schrodoprasum* the pollen does not germinate at all, whilst in *Allium vineale* and *A. oleraceum* the pollen nuclei apparently degenerate. The geographical distribution of these species and of *Poa alpina, P. bulbosa, Polygonum viviparum* and *Saxifraga cernua* in Poland is also dealt with.

ENTOMOLOGY. By H. F. BARNES, M.A., Ph.D., Rothamstcd Experimental Station, Harpenden.

GENERAL ENTOMOLOGY.—It is not generally realised to what extent various governments attempt to prevent the entry into their respective countries of injurious insects and plant pests. An example of this legislative control of insect pests is *The Destructive Insect and Pest Act and Regulations thereunder* (Acts, Orders and Regulations No. 8 (3rd revised edition), Canada, Dept. Agric., 1936, 36 pp.), issued recently by the Destructive Insect and Pest Act Advisory Board of Canada.

In an article on insect parasitism and biological control C. P. Clausen (*Ann. Ent. Soc., Amer.*, **29**, 1936, 201–23) deals with various phases of insect parasitism bearing on the methods of insect control. Among these can be mentioned the relation between chemical and biological control, as well as the effect of cultural methods upon the parasite population. Novel maps are given of the international movements of various predatory and parasitic insects which have been used in biological control. An instance of attempted biological control in progress at the moment is the introduction of a parasite of the cacao thrips from the Gold Coast to Trinidad. In this case the parasite, *Dasyscapus parvipennis*, a Eulophid wasp, was shipped between August and December 1935. They had been found in 1931 on the Gold Coast where apparently it was controlling the cacao thrips. According to a progress report by A. M. Adamson (*Trop. Agric.*, **13**, 1936, 62–3), 26,000 parasites have already been liberated in Trinidad. Another attempt to use the introduction of parasites to exert a biological control has been started recently in Fiji. Here three fruit flies belonging to the family Trypetidæ are known to occur. The biology of the most important one has been studied by H. W. Simmonds (*Dept. Agric., Fiji*, Bull. No. 19, 1936, 18 pp.). A parasite (*Tetrastichus giffardianus*) was imported from Hawaii in order to help the two local parasites. In six months 18,000 have been reared and strong colonies released in Vitilevu. This parasite had been introduced successfully into Hawaii by Fullaway in 1917. Already the parasite has been recovered in such numbers as to suggest that it may be a great success.

A Pacific Entomological Survey was organised by the Hawaiian Sugar Planters' Association and the Bernice P. Bishop Museum for a period of five years ending in 1932. So far nine publications have appeared. They deal with Marquesan and Society insects, the Tipulidæ and Elateridæ of Oceania and also freshwater fishes and lizards. The last contribution to be issued is by A. M. Adamson

(*Bernice P. Bishop Museum*, Bull. No. 139, 1936, 73 pp., with 8 plates) and deals with the environment of the Marquesas Islands.

The study of migrant insects is receiving more and more attention both from amateur and professional entomologists. The Immigration Committee of the South-Eastern Union of Scientific Societies is the nucleus of a large band of observers. In order to make a sound scientific study of this phenomenon of migration more observers are required. A popular account of the scheme has been written by Kathleen J. Grant (*Biology*, **2**, 1936, 214–19).

The influence of moonlight on the activity of nocturnal insects has been studied by C. B. Williams (*Phil. Trans. Roy. Soc.*, B, **226**, 1936, 357–89), using the light trap that was mentioned in the last Recent Advances (SCIENCE PROGRESS, No. 121, 1936, 125–6). The investigation was started in order to test the general belief that insect night activity is reduced at full moon. After analysing the data, in particular that of the Noctuidæ, it is considered that the lunar effect on the captures of the light trap has been definitely demonstrated. There is distinct evidence that it differs in the various groups apart from any difference in their time of flight. The investigator is of the opinion that it is probably a physiological effect on the activity of the insects and not merely due to reduction in the efficiency of the light trap when the moon is shining.

A new type of respiratory interrelation between an insect parasite and its host has been described by W. H. Thorpe (*Parasitology*, **28**, 1936, 517–40). The parasite is *Encyrtus infelix*, a Chalcid, and its host is the Coccid *Saisettia hemisphaerica*. The first three larval instars of the parasite are metapneustic. The spiracles are open and in actual communication with the atmospheric air. The egg has a hollow stalk which is left protruding from the dorsal body wall of the host. It is through this pedicel that the air enters the spiracles. The fourth and fifth instars are amphipneustic and the manner of respiration is entirely changed. The parasite becomes dependent on an elaborate structure developed by its host. The caudal processes degenerate, the fourth stage larva turns round in the scale insect and becomes invested with a closely fitting membraneous sheath, produced by the phagocytes and fine tracheal branches of the host. The sheath becomes attached to the main lateral tracheal trunks of the host in four or six places in the neighbourhood of the larval spiracles. An actual connection is established between the lumen of the host trachea and the cavity of the sheath. The spiracles are in functional communication with the tracheal system of the host. W. H. Thorpe, after testing various theories, comes to the conclusion that the production of

the sheath and the development of the tracheal attachments are adaptations for the respiration of the parasite. This phenomenon of a parasite causing certain tissues of the host to develop solely for its own use is most interesting and the investigator refers to similar instances occurring among the Chalcids. A parallel among other animals is the Copepod *Xencœloma* parasitic in *Polycirrus*; in this instance there is a real mingling of host and parasite tissue, both structurally and functionally.

A new type of larval respiratory organ has been described in *Atrichopogon trifasciatus* (Ceratopogoninæ) by E. T. Burtt (*Proc. R. Ent. Soc. Lond.*, A, **11**, 1936, 61–5). There are eight elliptical cushion-shaped areas dorsally on the larva of the second instar. These are devoid of the small warts found covering the rest of the body. These cushions have a double tracheal supply—one pair of branches entering laterally from the longitudinal trunk and another pair ventrally from the region of the commissure. These trachea spread out as fine parallel tracheoles, those from the lateral trachea passing over the surface directly, while those arising from the ventral trachea pass up to the surface of the hypodermis, branch in two and then pass in opposite directions on the surface. The cuticle in the region of the air cushions is thinner than over the rest of the body. There is also a pair of muscles attached to the cushion area which when touched becomes depressed, forming a trough. There seems little doubt that the cushion areas act as lungs and effect gaseous interchange with the atmosphere through a very thin film of water.

Hearing in insects is being studied by R. J. Pumphrey and A. F. Rawdon-Smith. In a recent paper (*Proc. Roy. Soc. Lond.*, B, **121**, 1936, 18–27) they have produced evidence to show that the anal cercus of the cricket and cockroach has a partly auditory function. The nervous response in the cercal nerve and in the mammalian VIIIth nerve runs closely parallel and so physiologically the anal cercus is of great interest. On the other hand, the typanic organ of the locust only resembles the mammalian ear in its relatively greater sensitivity to the higher frequencies of the auditory spectrum.

W. F. Morofsky (*J. Econ. Ent.*, **29**, 1936, 749–54) has made a survey of the insect fauna of some Michigan trout streams and has come to the conclusion that there is an increase in the fauna of improved, over unimproved, streams. In most cases there was a decided increase in the insect life and a decrease in other animal life in the improved areas.

In 1933 and 1934 seventy-five substances were used to attract oriental fruit moths in a peach orchard interplanted with apple

trees in Pennsylvania. A useful comparison of insects captured with the different baits is given by S. W. Frost (*Ent. News*, **47**, 1936, 64–8 and 89–92).

The insects found overwintering in fruit-tree trap bands are exceedingly varied. W. Speyer has, since 1933, been publishing notes on the Hemiptera and Coleoptera found hibernating in this position in the lower Elbe district. His latest contribution deals with the Lepidoptera (*Z. PflKrankh.*, **46**, 1936, 13–27).

D. L. Gunn and J. S. Kennedy (*J. Exp. Biol.*, **13**, 1936, 450–9) have described a closed ring apparatus in which there are two separate humidity gradients and also a modified " Aktograph " apparatus for finding the effect of different air humidities on the general activity of an insect.

A device for the rapid counting of large numbers of small insects has been devised by P. S. Milne (*Bull. Ent. Res.*, **27**, 1936, 269–71). This consists essentially of a turntable with a circular trough which takes the place of the stage on a low-power binocular microscope with a wide field.

ORTHOPTERA.—The relation of humidity and temperature to the development of three species of African locusts has been studied in detail by A. G. Hamilton (*Trans. R. Ent. Soc. Lond.*, **85**, 1936, 1–60). The different species each have different optima for their various stages, egg, hopper and adult. The optimal condition for each species is a succession of combinations of factors which are the most suitable for all its stages. MacLagan some years ago (1932) came to similar conclusions from his work on *Smynthurus viridis*.

K. H. L. Key (*Proc. R. Ent. Soc. Lond.*, A, **11**, 1936, 3–6) has made an apparatus for measuring the speed of locomotion of adult male locusts under the directing influence of a beam of light and under controlled conditions of temperature. Using this apparatus no difference was detected in the average speed of movement of starved (up to 98 hours) adult male locusts and normal control insects.

COLEOPTERA.—An interesting description of traps used to control the Japanese beetle in the United States and their application has been revised by F. W. Metzger (*U.S. Dept. Agric.*, Misc. Publ. No. 201, 1936, 12 pp.). Geraniol is the chief ingredient of the bait, while eugenol is also added. The geraniol-eugenol combination is made more efficient by the addition of phenyl ethyl alcohol, but this is not essential.

Flour beetles of the genus *Tribolium* are very hardy and able to exist on a wide variety of foodstuffs. World-wide commerce

transports them to new regions. A useful account of the members of this genus has been written by N. E. Good (*U.S. Dept. Agric.*, Tech. Bull. No. 498, 1936, 57 pp.). Maud J. Norris (*J. Animal Ecology*, 5, 1936, 19–22) has shown that the stored products beetle *Trogoderma versicolor* is quite comparable with the Mediterranean flour moth in its adaptation to life indoors. It is more specialised than the moth in that full fecundity and longevity occurs without food or water : the moth requires drinking water.

The life history, habits and distribution of *Heteroderes laurentii*, an American wireworm which is a pest of sweet potato, have been described by K. L. Cockerham and O. T. Deen (*J. Econ. Ent.*, 29, 1936, 288–96). This species has a one-year life cycle.

LEPIDOPTERA.—The available information concerning the feeding habits of adult Heteroneura has been collected and discussed by Maud J. Norris (*Trans. R. Ent. Soc. Lond.*, 85, 1936, 61–90). The foods used include nectar, over-ripe and rotting fruit, juices of sound fruit, exuding sap of plants, honeydew, honey, sugar baits, water and animal excreta.

A study of humidity effects on the development of the common clothes or webbing clothes moth (*Tineola bisselliella*) by Grace H. Griswold and Mary F. Crowell (*Ecology*, 17, 1936, 241–50) indicates that a relative humidity of 75 per cent. is most favourable for their development. At this humidity the life cycles of both males and females were shorter than those obtained at 20, 32, 43, and 93 per cent., the highest percentage of larvæ completed their development and the adults lived longest. One might therefore expect to find fewest moths about the house in the winter when the indoor atmosphere is dry and more moths in the spring and summer resulting from the more humid conditions of these seasons.

The effects of inbreeding on the fertility of insects has rarely been investigated. Maud J. Norris (*J. Genetics*, 32, 1936, 179–81) has kept records of the pedigree and fertility of twelve inbred generations of *Ephestia kühniella*, the Mediterranean flour moth. The main conclusion is that there is little permanent reduction of viability in egg number in spite of close inbreeding and a very uniform environment. T. Ahmad (*J. Animal Ecology*, 5, 1936, 67–93), in a study of the influence of ecological factors on this moth and its parasite *Nemeritis canescens*, has shown that at 23° C. and above, both on account of its quicker development and higher reproductive potential, the parasite can overtake the host. High temperatures favour the parasite but low ones favour the host.

A method for determining the time of spray application for codling moth control which has more than purely economic interest

has recently been suggested by T. J. Headlee (*J. Econ. Ent.*, **29,** 1936, 639–46). It involves the use of data concerning the accumulation of effective day-degrees of temperature in conjunction with emergence boxes and bait pans in the several climatically different zones in the U.S.A.

HEMIPTERA.—The Australian Jassoidea are being studied by J. W. Evans. A recent contribution is one dealing with the Bythoscopidæ (*Pap. and Proc. Roy. Soc. Tas.*, 1935, 1936, 61–83).

HYMENOPTERA.—A remarkable differentiation in the host relations of the male and female larvæ of various species of *Coccophagus* is noted by S. E. Flanders (*J. Econ. Ent.*, **29,** 1936, 468). As is often the case, the progeny of unmated females consists of males only. In the species investigated, the male sex was produced only hyperparasitically on individuals of its own or other parasitic species. Mated females produced only females and these developed only as primary parasites of lecaniine scales and mealy-bugs. Thus the males develop only as parasites of hymenopterous larvæ and the females only as parasites of an homopteron nymph or adult. In certain species the stimulus of fertilisation is followed by a marked change in the host preferences of the females. Before pairing they deposit their eggs in the body fluids of an immature primary parasite, when paired they oviposit in the body of *Lecanium.*

A fourth experimental study in insect parasitism by G. Salt (*J. Exp. Biol.*, **13,** 1936, 363–75) deals with the effect of superparasitism on populations of *Trichogramma evanescens.* This parasite is able to distinguish healthy from parasitised hosts and when few hosts are available it can restrain itself for 8 hours to the deposition of 5 per cent. of its available eggs. The discriminative ability apparently is perfect, but the restraint is limited and breaks down after a longer period of time or when several parasites are together. Then superparasitism occurs. Among the effects of this are that the number of hosts that escape steadily decreases, but even at high densities of parasites some hosts occasionally escape. Another effect is that an increasing proportion of the emerging parasites are males. This may be due to the survival of the males when larvæ of the two sexes are in competition.

Two papers by G. C. Ullyett on *Microplectron fuscipennis,* an important parasite of the Pine sawfly, *Diprion sertifer,* in Europe, have appeared. This parasite has been shipped to Canada for use against *D.'polytomum,* a close relative. The first of these is on host selection (*Proc. Roy. Soc. Lond.,* B, **120,** 1936, 253–91) and should be read in conjunction with G. Salt's paper on host selection in *Trichogramma evanescens* (see SCIENCE PROGRESS, No. 119,

1936, 50). The second paper deals with the physical ecology of *M. fuscipennis* (*Bull. Ent. Res.*, **27**, 1936, 195–217). Both the longevity of the adults and the extent of oviposition are found to be proportional to the saturation deficiency of the atmosphere. The actual and possible distributions of the parasite in Europe are discussed and a map is given showing the position as known at present. The possible distribution in North America, after liberations have been made, is considered.

DIPTERA.—The American species of *Psychoda* have been monographed by F. del Rosario (*Philippine J. Science*, **59**, 1936, 85–148). This genus includes the flies popularly known as "moth flies," and includes those which breed in bacterial films of sprinkling filter beds used for the purification of sewage. Although no adequate evidence has been given to warrant danger from these flies, they sometimes have occurred in such great numbers on filter beds that they have got in the noses and mouths of workers in such localities. There is one case on record of myiasis being due to the larvæ of *Ps. albipennis* (Patten and Evans, 1929). These flies have short life cycles and a great productivity, and so could be used with advantage in studies in heredity.

D. N. Roy (*Bull. Ent. Res.*, **27**, 1936, 423–9) has been investigating the rôle of blood in ovulation in the mosquito *Aëdes ægypti*. He has found that there exists a definite quantitative relationship between the weight of the blood meal and the number of eggs produced. It appears that the protein elements in the blood after absorption exercise twofold functions on the ovaries, the follicles are stimulated to activity and actual material is provided for the formation of the eggs.

The Chloropid populations on pasture grasses in Kansas have been carefully studied by D. A. Wilbur and C. W. Sabrosky (*J. Econ. Ent.*, **29**, 1936, 384–9). Over 11,000 Chloropids representing fifty-three species were collected in the regular semi-weekly collections made in 1933 and 1934. Three periods of emergence were found, the first from April 24th to June 1st, the second from July 10th to August 4th, and the third from August 23rd through October. The summer emergence in 1934 was conspicuous by its absence, due, it is claimed, to the heat and drought.

Some years ago Holdaway (*Nature*, **126**, 1930, 648–9) demonstrated how great a part competition played in determining the abundance of the different species of blow-flies. Only 231 individuals of *Lucilia sericata* reached the adult stage out of an initial population of roughly 50,000. This mortality was due chiefly to dipterous competition. The principal species of English blow-flies

which compete with *L. sericata* are *Calliphora erythrocephala*, *Phormia groenlandica* and *Sarcophaga falculata*. It was established by Graham-Smith (1916) and Wardle (1927) that there is a definite seasonal succession of these blow-flies. A. C. Evans now (*Parasitology*, **28**, 1936, 431–9) has studied the effects of temperature and humidity in determining the survival and development of the above-mentioned four species. The result of this investigation has been to show that in their resistance to high temperature and low humidities these species can be arranged in the opposite order to their seasonal succession ; the earliest to appear is least resistant and the last to appear is the most resistant. Thus the *Phormia* is the most resistant, the *Calliphora* least, and the *Lucilia* and *Sarcophaga* intermediate. The implication is that the reasons for their degree of resistance may be allied to or the same as those which determine their seasonal distribution. It has been shown that blow-fly larvæ fed on fatigued frog muscle grow to a larger size than control ones fed on resting muscle. G. Pugh Smith (*J. Exp. Biol.*, **13**, 1936, 249–52) has now shown that heat destroys the growth stimulating property of fatigued muscle and the blood of frogs whose muscles have been artificially fatigued acts as a growth stimulant. This substance does not accumulate in the liver. It is also shown that differential bacterial numbers on fatigued and non-fatigued muscle is not responsible for the different growth shown by fly larvæ as the bacterial population is equally dense in both types of muscle.

B. Jobling (*Parasitology*, **28**, 1936, 355–80) has made an important revision of the subfamilies of the Streblidæ and the genera of the subfamily Streblinæ. This author divides the family into four subfamilies, of which two are new. The Streblidæ of the Old World are contained in the Nycteriboscinæ and the Ascodipterinæ, while those of the New World belong to the Trichobiinæ and Streblinæ. The generalised characters, he finds, are of more importance than those produced by convergent evolution in the study of the phylogeny of these parasitic Diptera. Jobling further suggests that the group Pupipara should be nullified, as the group is not a natural one. This is not a new suggestion. The Pupipara comprise the Hipposcidæ, Streblidæ and Nycteribiidæ which are larviparous and the Braulidæ which is oviparous. The investigator considers that the Hippoboscidæ show close affinities to the Muscidæ and should be placed at the end of the Calypteræ close to the family which contains the genus *Glossina*. The Nycteribiidæ and the Streblidæ belong to the Schizophora. The Streblidæ should be placed among the Acalypteræ with the Nycteribiidæ next to

them. The Braulidæ should also be placed in the superfamily Acalypteræ.

An extraordinary and unusual looking fly from caves in Kenya Colony has been described by E. E. Austin (*Proc. Zool. Soc. Lond.*, 1936, 425–41). It has most of the features which Bezzi described as characteristic of cave-dwelling Diptera, such as the elongation of the arista, thickening of the proboscis, reduction of the eyes and wings. In addition the whole body and its appendages are extremely hairy and there are no macrochætæ with the exception of slender vibrissæ. Rightly it has been named *Mormotomyia hirsuta* (μορμωτος, frightful; μυια, fly). A new family Mormotomyidæ has been erected to harbour this creature.

OTHER ORDERS.—S. F. Bailey (*Canad. Ent.*, **48**, 1936, 95–8) has summarised the references to thrips attacking man. The first instance recorded was in 1902, and there have been several since.

H. D. Slack (*J. Animal Ecology*, **5**, 1936, 105–15) has been studying the food habits of caddisfly (Trichoptera) larvæ. Of twelve types under observation most were primarily phytophagous but only three entirely so. The remaining forms were omnivorous and one, *Rhyacophila dorsalis*, was predominantly carnivorous.

AGRICULTURAL PHYSIOLOGY. By ARTHUR WALTON, B.Sc., Ph.D., School of Agriculture, Cambridge.

LACTATION.—Controversy still sways round the endocrinological factors which govern the growth of the mammary gland and the secretion of milk. The view held by Turner (Turner and Frank, *Bull. Mo. Agric. Exp. St.*, 174, 1932) that theelin and progestin are both necessary for the development of the gland and that prolactin is responsible for the initiation of secretion has found most acceptance. However, Nelson and Smelser (*Amer. J. Physiol.*, **103**, 374, 1933) find that full mammary development and secretion may be obtained in the male guinea pig with theelin alone. Gomez and Turner (*J. Dairy Sci.*, **19**, 450, 1936) could find no action of theelin when injected into the hypophysectomised animal. They found also that milk secretion ceases after hypophysectomy, but is restored by injection of whole gland. de Fremery (*J. Physiol.*, **87**, 50P., 1936) finds that if œstradiol benzoate dissolved in adeps lanæ is rubbed on the surface of the udder of a goat there is gradual but complete development, and when prolactin is then injected there is a copious flow of milk.

The mammary gland is not governed solely by direct endocrine factors. Selye (*Amer. J. Physiol.*, **107**, 1934) and Selye, Collip

and Thomson (*Endocrin.*, **18**, 237, 1934) find that suckling maintains lactation through stimulus of the corpus luteum. It is possible that the action is indirect and through the pituitary, since Marshall and Verney (*J. Physiol.*, **86**, 327, 1936) show that the pituitary can be activated by exteroceptors acting through the central nervous system.

Edwards (*J. Dairy Research*, **7**, 210, 1936) has compared the efficiency of various breeds of dairy cattle. Efficiency is defined as the ratio of energy in the milk to energy in the digestible nutrients consumed. Among the best representatives of the various British dairy breeds there is little difference. There is a steady increase in efficiency with increase of yield and a decrease with fall of the lactation curve.

ARTIFICIAL INSEMINATION.—Walton and Prawochenski (*J. Heredity*, **27**, 341, 1936) report the successful export of ram semen by air mail from Cambridge to Warsaw.

WOOL PRODUCTION.—Bosman (*Onderstepoort J.*, **4**, 551, 1935) finds that pregnancy does not effect fibre fineness significantly, but there is a definite effect on both fineness and fleece density of lactation and suckling. Scott (*J. Dept. Agric. S. Australia*, **39**, 534, 1935) reports that considerable financial gains result from " rugging " sheep, *i.e.* covering them with canvas. The wool is kept free from dirt and burrs. In spite of the heat, the animals thrive well and put on fat, which indicates what a small part the skin of the sheep must play in thermo-regulation.

ARCHÆOLOGY. By E. N. FALLAIZE, B.A.

NEW FOSSIL ANTHROPOID FROM SOUTH AFRICA.—The outstanding event in the study of early man during the last six months is the discovery in South Africa of part of the skull and brain-cast of a fossil anthropoid, closely resembling the Taungs skull (*Australopithecus*), which, since its discovery ten years ago by Professor Raymond Dart, has been the subject of acute controversy. The new fossil was found by Dr. R. Broom, F.R.S., of the Transvaal Museum, Pretoria, in July last, in material from a blast in caves at Sterkfontein, near Krugersdorp. The parts of the skull which have been found include the upper part of the face and most of the base of the skull, a large part of each parietal and a considerable part of the occiput. In the same matrix was found the detached right maxilla with three teeth. The third upper molar was also found here, though it too was detached. Later examination disclosed that in the stone on the left side of the left orbit was the left maxilla in nearly perfect condition, with

both premolars and first and second molars present. As the sockets of the incisors and canine are also shown, the upper dentition is now known fully. The first molar is of the typical Dryopithecus pattern. The premolars and first molar are said by Dr. Broom to be " so human-like that there is scarcely any doubt that had they been found without the skull they would have been held by most to be human." The skull is that of an adult.

It will be remembered that when Professor Dart first announced his identification of *Australopithecus* on the evidence of the Taungs skull as neither gorilla nor chimpanzee, but as an extinct type of anthropoid so very near to the line of descent of man as virtually to constitute the " missing link," his conclusion was criticised on the ground that, the skull being that of an immature individual, he had been misled by the infantile resemblances to the human found in young anthropoids. The question has never been conclusively determined, though opinion generally has inclined to place the Taungs skull with the apes. As Dr. Broom's new fossil is unquestionably adult and resembles the Taungs skull sufficiently closely to warrant inclusion in the same genus, his discovery is of crucial importance in confirming Professor Dart's views, and definitely ranks *Australopithecus* on the human side of the line. The Sterkfontein skull, however, for which Dr. Broom proposes the name *Australopithecus transvaalensis*, is probably later than Taungs, belonging to the Upper Pleistocene, whereas Taungs is probably Middle or Lower Pleistocene, and it shows specific differences. Of the generic similarity and its human and non-apelike character there can be no question. Dr. Broom's general conclusion is that there were " in South Africa during Pleistocene times large non-forest living anthropoids—not very closely allied to either the chimpanzee or the gorilla, but showing distinct relationships to the Miocene, and especially to the Pliocene species of *Dryopithecus* (*Nature*, Sept. 19, p. 486 ; Oct. 24, p. 719).

UPPER PALÆOLITHIC CULTURES.—Both as a piece of stocktaking and as a basis for constructive criticism, Miss Dorothy A. E. Garrod's Presidential Address to the Anthropological Section of the British Association at Blackpool in September last, when she dealt with the Upper Palæolithic in the light of recent discovery, will be a landmark in the study of man in the Old Stone Age. She began by pointing out that the researches of the last twelve years, especially the great impetus given to study of the Stone Age cultures found outside Europe, have made it increasingly clear that the classification of de Mortillet, which was based entirely on evidence from Europe, needs revision, while the old division into

Lower, Middle and Upper Palæolithic has a chronological value only, and for purposes of typology the fundamental division is into hand-axe, flake and blade cultures. In regard to the blade cultures it is evident that a diversity of strains has been grouped together under the heading of Aurignacian. Miss Garrod referred to the recent work of Peyrony and her own and other researches in the Near East, which have led to a distinction between the blunted back blade, formerly classed as Lower and Upper Aurignacian, for which Peyrony has suggested the name Perigordian, and the Middle Aurignacian, in which the type implements are steep and rostrate scrapers, for which it is suggested that the old name of Aurignacian should be retained.

Miss Garrod now puts forward a tentative scheme as a result of her own experience in the Near East and of her study of recent excavations in Russia and North Africa, which indicate that the problem is extremely complex. The terminology suggested is for the Lower Aurignacian, Chatelperronian ; Middle Aurignacian remains Aurignacian proper, and the former Upper Aurignacian is divided into Lower and Upper Gravettian, corresponding to La Gravette and Font Robert respectively. For these the origins suggested are for the Chatelperronian somewhere in south-west Asia, possibly giving rise to the Capsian by way of the Kenya Aurignacian, and also passing northward to develop into the Gravettian, which is abundant in Russia ; while for the Aurignacian proper the Iranian plateau is a possible centre of dispersion.

There are thus three major provinces for the blade culture : the Capsian in Kenya and Little Africa, the Gravettian in northwest Asia and eastern Europe, both possibly derived from the Chatelperronian, but cut off from each other by the great Aurignacian province of the Near East. From the Gravettian and Aurignacian centres migrations pour into Europe and at the extreme limit give the classic French sequence. The industries of the end of the Pleistocene, Magdalenian, etc., appear to be local developments of one or other of these stocks ; while the Solutrian is an intrusive culture of Hungarian origin.

A bare summary of the main conclusions of Miss Garrod's address does less than justice to its masterly handling of a mass of complicated and detailed evidence, which in itself will allay any fear that she has shown a tendency to over-simplification. If this indeed be a failing it will, no doubt, be corrected by further research.

A MAGDALENIAN SITE IN SOUTHERN FRANCE.—It is interesting to turn, in the light of Miss Garrod's remarks on the development of the Magdalenian cultures, to the results of the excavation of the

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Magdalenian strata in the cave of Isturitz, Basses Pyrénées, by the Comte René de Saint Périer (*Archives de l'Institut de Paléontologie humaine*, Mém. 17). The main part of the cave consists of two chambers, the Hall of St. Martin (described previously, in 1931) and the Great Hall or Hall of Isturitz, of which the clearance of the upper strata was undertaken after the corresponding strata in the Hall of St. Martin had been examined. Both chambers were occupied simultaneously when the earliest Magdalenian deposits were laid down ; but it is evident that the occupation of the Great Hall was both more intense and more prolonged than that of the Hall of St. Martin. This is indicated by the number of arte-facts, which differ in certain respects in the two chambers, as well as by the stratification. In both chambers occupation was con-tinuous from Aurignacian, through Solutrian, to Magdalenian, and in the Great Hall only from the later Magdalenian to a culture, which is identified as Azilian, without any sterile deposits inter-vening. The hearths of the last-named culture had been dug in the uppermost strata of the Magdalenian of the Great Hall. In both chambers the Magdalenian begins with Magdalenian III of the Pyrenean type, corresponding to Marsoulas. The most impor-tant occupation is Magdalenian IV, again appearing in both chambers ; but Magdalenian V and VI occur only in the Great Hall. Hearths are of more frequent occurrence in the Hall of St. Martin, as are also the engraved plaques ; but the Great Hall, on the other hand, is richer in its art as a whole.

In the important period of occupation, Magdalenian IV, the chief characteristics are a cold climate fauna, in which the horse is predominant, but with a few retarded forms, such as *Rhinoceros tichorhinus*, *Ursus spelæus* and *Hyæna crocuta*, and a lithic industry, which is small and of indifferent execution. The various forms of this industry are lacking in definition, and the material from which the implements are made is poor, the flakes being irregular and bad. The bone industry is abundant ; but here again at times the technique is poor. Some of the material has scarcely been prepared at all for its purpose. The bone objects include needles, lance-heads and *bâtons*. Many of these artefacts have been perforated. The harpoons found in the Great Hall show signs of evolution.

The most striking feature of the site is the rich development of the art of this period, which in sculpture and engraving is very fully representative both in naturalistic and in conventionalised decorative forms. In both there is evidence of a close and accurate observation of nature, the decorative forms evidently being based

upon naturalistic forms. At the same time the great variation shown in powers of execution may be attributed to the possibility that the cruder and less finished examples are trial or practice pieces.

In Magdalenian V and VI the climate is still cold, with the horse predominating at first, but gradually giving way to the deer. Birds become abundant. There is a very marked change in the character of the stone industry. Barbed harpoons appear and objects of art become scarcer, the art form becoming simple graving, while both realistic art and decorative art weaken and are less varied.

In certain respects the latest level is of even greater interest than the later Magdalenian. The stone industry resumes the forms of the Aurignacian with characteristic delicacy of retouch. Circular scrapers determine the Azilian character of the culture, and this is confirmed by the harpoons, which become more and more evolved. With this culture all evidence of human occupation of the cave comes to an end.

CULTURAL RELATIONS OF MINOAN CRETE AND WESTERN ASIA.— Although it may appear late to speak of field work in the season 1935–36, the exhibit illustrating the Minoan civilisation, staged by Sir Arthur Evans in the Jubilee Exhibition of the British School of Archæology at Athens in October–November last at the Royal Academy, has served to emphasise the remarkable results achieved by Sir Leonard Woolley's archæological expedition to Syria. The material obtained from the three sites examined by the expedition has not only demonstrated a close relation with the Ægean, extending over a considerable period, but at Tell Atchana an inland site, it points unquestionably to a preponderating Minoan influence and a connection which is shown to date back so far as 1700 B.C. This is considerably earlier than any evidence then or previously known, although the work of French archæologists at Ras Shamra in Syria had already demonstrated that Crete had established a foothold on the Syrian coast by about 1400 B.C.; and Minoan painted sherds from Palestine, equated with Tothmes III, have carried back the importation of Minoan wares about a century before that date. From Tell Atchana come sherds of local fabric, which, however, present decorative motives indicative of Minoan influence, such as the bowl fragments (lent to Sir Arthur Evans for exhibition), in which creamy white rosettes on a dark ground fit on to the decoration of the fine egg-shell ware of the Cretan Middle Minoan II period. Sir Arthur, however, was able to show evidence of an even earlier connection. Thanks to the courtesy

of M. Claude Schaeffer and the Louvre authorities, he had added to his exhibits the latest find at Ras Shamra, namely part of a vase, the first-fruits of a still unexplored tomb, in the pure polychrome style of the Middle Minoan II period, equated with the reign of Senusert, which carries back the relations of Crete with the Syrian coast to the beginning of the twentieth century B.C.

AN EARLY BANTU SETTLEMENT IN THE NORTHERN TRANSVAAL, S. AFRICA.—In view of the importance of the Zimbabwe culture in past controversy relating to the development of culture in South Africa, exploration of early Bantu sites in accordance with the strict canons of archæological investigation, where such are applicable, is one of the most urgent desiderata of South African cultural studies. Hence the interest of the preliminary report on excavations which have been in progress since 1933 in the Zoutpansbery District of the Northern Transvaal, Union of South Africa, contributed to *Antiquity* for September by Professor C. van Riet Lowe, Director of the Bureau of Archæology, University of South Africa. Two sites have been under investigation ; Mapungubwe, a fortress with dry-stone walling on the top of an inaccessible sandstone cliff, with an associated cemetery, and an adjacent mound site, Bambandianalo. They are situated about 160 miles south-west of Zimbabwe and just over 50 miles due west of Messina, where there are extensive remains of pre-European copper workings. Unlike the numerous similar well-known ruins of the region and in Rhodesia, including the Zimbabwe investigated by Dr. D. Randall-MacIver and Miss Caton-Thompson, these two sites have not been disturbed by treasure hunters and unauthorised investigators ; and they are now safe from that danger, as the sites have been purchased by the Union Government and their investigation entrusted to the Archæological Committee of the University of Pretoria.

So far as the investigations have gone, the mound site, Bambandianalo, which is still being explored by Captain G. A. Gardner, is shown to be the earlier. It contains evidence of two cultures, of which the upper and later is closely related to, but not quite identical with, that of the fortress and cemetery site of Mapungubwe. The earlier culture Captain Gardner considers to be quite unrelated to the later. It belongs to a pastoral people whose use of metal was confined almost exclusively to copper. The pottery exhibits a marked diversity of form, showing an evolutionary process from a rough undecorated ware to a highly refined and decorative technique. In the burials several skeletons are found in surrounds of small stones, the whole being covered with inverted potsherds.

Animal bones were occasionally found with the human. One animal, possibly a cow, seems to have been buried ceremonially, as parts of the ribs, jaw and femur had been covered with inverted pots, like the human bones. The cylindrical beads are characterised by Mr. H. Beck as being extremely ancient. It is thought that these people, potters and pastoralists, may represent the earliest vanguard of the incursion of the Bantu-speaking peoples into what is now South Africa.

The people of the Mapungubwe site were skilled workers in metal, gold, copper and iron. Although the culture of this site is not quite the same as that of the upper strata of Bambandianalo, on both sites it appears fully fledged, and is quite distinct from the earlier culture of the latter. At Mapungubwe the deposits in places are over 20 feet deep. The arts and crafts are those of a Bantu-speaking people and are related in some degree to those of Zimbabwe II, though differing slightly. The pottery is some of the finest that has been found in the South African ruins. It is thought possible that this people may represent a mixed Sotho-Shona stock.

The culture of the cemetery was comparatively rich and varied. Seventy ounces of finely wrought gold ornaments were taken from the burial which led to the discovery of the cemetery, in which were twenty-three burials. These contained quantities of gold, glass, ivory, copper, iron, and bone ornaments and weapons, as well as several very beautiful earthenware bowls of funerary type.

This culture included many imported elements, notably a great variety of beads from the East and Chinese porcelain. It is concluded that in date the Mapungubwe site is medieval or post-medieval.

A detailed account of the excavations is in course of preparation.

NOTES

In 1924 Prof. T. Svedberg of Upsala calculated that it should be possible to induce measurable sedimentation of the particles in many stable colloidal solutions by subjecting these to high centrifugal forces. By 1926 success had attended his experiments and he was able to demonstrate for the first time the visible sedimentation of the red protein of blood, hæmoglobin, in homogeneous solution. During subsequent years his machines were continuously improved, generating greater centrifugal forces and serving to throw down smaller and smaller particles For instance, in 1926 a force 80,000 times gravity was used to measure the molecular weight of hæmoglobin. In 1929 forces of 165,000 times gravity were in regular use, corresponding to 55,000 revs. per minute of the rotor. These served to investigate almost all the well-known proteins and also polysaccharides, dyes and other complex organic substances and a number of inorganic colloids and complex inorganic salts. Nevertheless, higher forces were envisaged, and in 1935 the phenomenal speed of 145,000 r.p.m. was reached and the forces generated (approximately 900,000 times gravity) was found to cause measurable sedimentation of the sodium and chlorine ions in the solution. From the results of these experiments the molecular weight of sodium chloride was calculated and the value 57·4 obtained, compared with 58·4—the true value. Thus in the short space of ten years means were developed for studying the sedimentation of substances ranging from the larger colloidal particles to the smaller inorganic ions. It was thus established that there is no fundamental difference between colloidal and true solution, the distinction being purely a question of the relative sizes of the particles.

As experience accumulated with the larger centrifugal forces Svedberg and his assistants soon came to realise that these were very important from another point of view. It was found possible to separate the constituents of a mixture so rapidly that the sedimenting boundaries could be photographed individually and the

Showing the rotor with two cell holes, one for the cell proper (seen beside
the rotor) and one for the balancing cell.
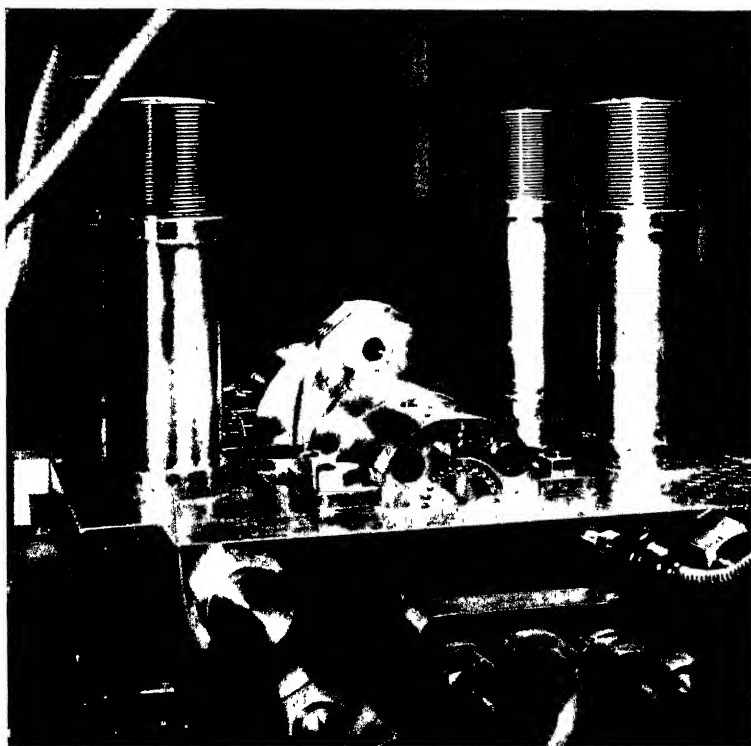
The centrifuge with the upper half of the casing removed. The four
steel bolts pass through the casing into the concrete foundations.
The rotor with the cell upwards is seen in the centre.

concentration of each component in the mixture calculated from measurements of certain properties of the boundary. For example, a protein mixture like blood serum can be placed in the centrifuge cell without previous chemical treatment of any kind, not even simple dilution, and from the photographs the number of proteins present can be seen and the concentrations of these calculated. This analytical property of the ultracentrifuge is now recognised to be of great biological value, particularly since it appears that many protein substances are very sensitive to *in vitro* manipulations and the results of chemical analysis of a fluid in many cases do not represent its true *in vivo* composition.

The earlier centrifuges embodied circular steel rotors with two holes, one for the cell proper and one for the balancing cell. Then an oval shape was used, and finally a rotor of the peculiar shape show in the photograph was found to resist best the stresses and strains involved. The rotor shown was made in Upsala and is the one now used at the Lister Institute of Preventive Medicine, London. It was tested at 73,000 r.p.m., and is employed regularly at 65,000 r.p.m., corresponding to approximately 300,000 times gravity in the cell. The diameter across the cell holes is approximately 11 inches. The performance of this rotor is regarded as the best yet obtained for analytical purposes. The analytical advantage of using greater forces is more than offset by the obligatory reduction in the height of fluid column in the cell. The rotor is made from chrome-nickel steel and much careful work is required to give it its final shape. The rotor is driven by high-pressure oil impinging on the minute turbines seen on each end of the shaft. The bearings are slip bearings with very fine clearances and pressure lubricated. In addition there are end-thrust bearings also with fine clearances.

The cell, shown beside the rotor, has an outer cylindrical shell of duralumin which is made a pressure fit in the cell hole. Inside this cylinder there is a cell body of ebonite compressed between end plates of optically polished crystalline quartz. By looking end-on at the cell the fluid to be centrifuged, usually about 0·2 ml., can be seen as it lies in a cavity in the ebonite between the quartz plates. Once in each revolution of the rotor the cell passes a quartz window in the steel casing and is illuminated by the light from a mercury arc. The light passes through the cell and through another window in the casing to a camera suspended horizontally from the ceiling. If the protein is coloured, as in the case of hæmoglobin, the process of sedimentation may be watched with the eye. Owing to the high frequency with which the pictures are received by the eye the cell appears to be stationary and a perfectly clear

view is obtained. If the protein is colourless, as is frequently the case, it will almost certainly absorb in the ultraviolet, and filters are used to select light of the absorbed wavelength from the arc with which to photograph the cell.

The steel casing is several inches thick and is made in two halves, each with a planed surface. When the surfaces are apposed and the nuts tightened down, the rotor chamber is vacuum tight. This is necessary because air must be excluded from the chamber or undue heating will occur. A rotary oil pump evacuates the chamber initially to a few millimeters of mercury pressure and then hydrogen from a cylinder is led in to maintain a pressure of 20 mm. Hydrogen is used because of its low frictional coefficient. It is also more satisfactory than a vacuum since it conducts heat from the surface of the rotor across the short gaseous path to the casing.

The turbine oil passes through a water cooler before reaching the turbines. The water comes from a refrigerating plant and is mixed with uncooled mains water to the desired temperature before entering the cooler. In this way the temperature of the rotor is kept constant throughout the run, or allowed to rise a degree or two at the most and very gradually. Abrupt changes of temperature give rise to convection currents in the fluid and the experiment is ruined.

A special long-focus objective lens is used and the photographs are taken from an adjacent room in which all the control equipment is housed.

A typical experiment is conducted as follows : A solution of the protein—for example, egg albumin, which has a molecular weight of 34,500—is put in the cell and covered with a layer of oil to prevent evaporation. In half an hour the rotor reaches 60,000 r.p.m. and sedimentation commences. Photographs and a full set of temperature, oil pressure and speed readings are taken every ten minutes. In two hours the protein has sedimented a distance of approximately 8 mm., and the rotor is then brought to rest in 20 minutes. The cell is 18 mm. deep, so that in the case of mixtures for analysis the experiment is often prolonged to get the maximum separation of the components.

Original Research Work on Tin (C. O. B.)

The valuable research work carried out under the auspices of the International Tin Research and Development Council, which is advancing our scientific knowledge of the metal Tin, is well illustrated by the technical publications Series A, Nos. 30–45. These contain reports of original work carried out by the Council's

investigators and for the most part have been previously presented during 1936 to various Societies interested in the work.

Tinned metals, such as tinplate and tinned copper, have received special attention, No. 30 dealing with the corrosion of tinplate and giving a review of the mechanism of this type of corrosion under conditions such as obtain in the cans used for foodstuffs, which subject is of very great importance at the present time. Among the practical remedies for tinplate corrosion given are coating with lacquer or with an additional thin coating of electro-deposited tin and improvement of the steel base. The corrosion of the metal tin is dealt with in No. 34, and special attention is given to the influence of oxygen. Tin coatings on steel are considered separately in this report. In order to study the continuity, structure and behaviour on deformation of the alloy on tinplate, it is necessary to remove the coating of tin without attacking the underlying layer of tin-iron alloy or exposed steel, and No. 37 shows possible methods of examination. The electrolytic method described consists of an anodic treatment in 5 per cent. sodium hydroxide solution at 30° C. under a controlled potential difference. The presence of pores and discontinuities in the coating of tinplate is one of the chief problems of tinplate manufacture, and No. 38 establishes a connection between the location of the pores, etc., and imperfections such as scruff bands and grease marks.

The hot-tinning of copper receives attention, No. 32 reporting an investigation on the porosity of such formed tin coatings on metallic copper. It is shown that the production of porous irregular coatings is associated with the presence of cuprous oxide in the copper. As a remedy the copper may be treated cathodically in dilute caustic soda solution. No. 33 also deals with tinned and soldered copper, and shows that in these cases there is always the compound Cu_3Sn in contact with the copper and a layer of Cu_6Sn_5 over it. This duplex structure tends to be detached in fragments as it is formed, leading to contamination of the tin in the bath and thus in the coating. This contamination is beneficial up to about 1 per cent., but excess of copper in the tin leads to rough coatings.

The constitution of metallic alloys containing tin is receiving attention and in this series of reports, No. 31 deals with the tin-rich antimony-tin alloys containing from 2 to 14 per cent. antimony. The solubility of antimony in tin is shown to decrease from 10·5 per cent. at 246° C. to 4 per cent. at 190° C., and to 3–5 per cent. at 100° C. The form of the solubility curves suggests that the mechanical properties of some of these tin-rich alloys might be sensitive to heat-treatment, and further researches are proceeding

in this direction. The alloys of cadmium and tin are dealt with in No. 43, and it is shown that the solubility of tin in cadmium is 0·25 per cent. at 176° C. The preparation of a reagent and the detection and colorimetric determination of tin are dealt with in Nos. 40 and 41. The opacification of enamels with tin oxide receives attention in No. 42, and the retarding effect of stannous salts on the oxidation of olein and oils in No. 45.

This review gives some idea of the range of work undertaken, and for those interested, copies of any of these publications may be obtained free of charge from the International Tin Research and Development Council.

Miscellanea

H.M. the King has approved of the following awards this year by the President and Council of the Royal Society : A royal medal to Prof. R. H. Fowler, F.R.S., for his work on statistical mechanics and allied departments of modern mathematical physics, and to Prof. E. S. Goodrich, F.R.S., for his work on the morphology of the excretory organs of the invertebrata and for his work on the comparative anatomy and embryology of the vertebrata.

The following medals have also been awarded : The Copley medal to Sir Arthur Evans, F.R.S., for his pioneer work in Crete, particularly his contributions to the history and civilisation of its Minoan Age. The Rumford medal to Prof. E. G. Coker for his researches on the use of polarised light for investigating directly the stresses in transparent models of engineering structures. The Davy medal to Prof. W. A. Bone, F.R.S., for his pioneer work on contact catalysis and his researches on the mechanism of combustion of hydrocarbons and on the nature of flames and on gaseous explosions. The Darwin medal to Dr. E. J. Allen, F.R.S., for his long-continued work for the advancement of marine biology, not only by his own researches but by the great influence he has exerted on very numerous investigations at Plymouth. The Hughes medal to Dr. W. Schottky for his discovery of the Schrot Effect in thermionic emission and his invention of the screen-grid tetrode and a superheterodyne method of receiving wireless signals.

The following were recommended as officers of the Society : Sir William Bragg, president ; Sir Henry Lyons, treasurer ; Sir Frank Smith and Prof. A. V. Hill, secretaries ; Sir Albert Seward, foreign secretary.

The Nobel prize for medicine for 1936 is shared by Sir Henry Dale and Prof. Otto Loewi for their work on the chemical trans-

mission of nervous impulses. The prize for chemistry has been awarded to Prof. P. Debye and the prize for physics is divided between Prof. V. F. Hess of Innsbruck for his work on cosmic radiation and Dr. C. D. Anderson of Pasadena for his discovery of the positron.

The International Union against Cancer has awarded a prize of 50,000 francs and 50 mgm. of radium to Prof. Kennaway and Prof. Cook of the Royal Cancer Hospital, Fulham Road, for their work on cancer.

On the occasion of the celebration of the tercentenary of Harvard University in September, the honorary degree of Doctor of Science was conferred on a number of famous men, including Prof. E. B. Bailey, Sir Arthur Eddington, Sir Joseph Barcroft, Prof. E. D. Adrian, Prof. R. A. Fisher, Prof. G. Hardy and Sir Frederick Gowland Hopkins.

Prof. D'Arcy Wentworth Thompson has been elected president of the Royal Society of Edinburgh.

The Rt. Hon. Lord Balfour of Burleigh succeeds Lord Linlithgow as chairman of the Medical Research Council, and Prof. John Mellanby succeeds Prof. E. D. Adrian who retires in rotation.

Sir Edward Poulton has been elected president of the British Association for the year 1937.

Dr. Charles Edouard Guillaume has retired from his post as director of the International Bureau of Weights and Measures at Sèvres. His successor is the assistant-director, M. Albert Pérard.

We have noted with regret the announcements of the death of the following well-known men of science during the past quarter : Prof. A. Anderson of University College, Galway, physicist, best known for the method of measuring self-inductance which bears his name ; Sir George Buchanan, formerly senior medical officer in the Ministry of Health ; Dr. J. B. Charcot, polar explorer ; Prof. H. L. Le Chatelier, For. Mem.R.S., professor of physical chemistry at the Sorbonne ; Prof. B. G. Cormack, emeritus professor of botany, Anderson College, Glasgow ; Mr. Sherard O. Cowper-Coles, expert on electrodeposition ; Mr. E. R. Deacon of amatol fame ; Dr. G. Forbes, F.R.S., formerly professor of natural philosophy in Anderson College, Glasgow ; Mr. J. W. Gordon, K.C., patent expert ; Sir George Hampson, formerly assistant keeper in the Natural History Museum, entomologist ; Dr. W. H. Harrison, lately of the Indian Agricultural Service ; Dr. R. C. J. Howland, professor of mathematics, University College, Southampton ; Dr. H. A. D. Jowett of the Wellcome Chemical Works, chemist ; Lieut.-

Col. R. Knowles, C.I.E., acting director of the Calcutta School of Tropical Medicine ; Prof. E. Lagrange of the École Militaire, Brussels, seismologist ; Prof. T. M. Lowry, C.B.E., F.R.S., professor of physical chemistry in the University of Cambridge ; Dr. P. Mitra, head of the Department of Anthropology, University of Calcutta ; Lord Moynihan, surgeon ; Dr. N. A. F. Moos, formerly director of the Colaba Observatory, Bombay ; Mr. R. D. Oldham, F.R.S., geologist ; Prof. W. A. Parks, F.R.S., of the University of Toronto, geologist ; Dr. E. E. Prince, of Canada, fisheries expert ; Mr. W. Rintoul, president of the Faraday Society, formerly a director of the research organisation of Imperial Chemical Industries, Ltd. ; Dr. B. Smith, F.R.S., director of the Geological Survey ; Prof. W. J. Sollas, F.R.S., of the University of Oxford, geologist ; Mr. C. F. Talman, of the U.S. Weather Bureau ; Mr. R. R. Webb, of St. John's College, Cambridge, mathematician ; Dr. Edward Weston, electrical engineer, of Weston instruments and Weston cell fame.

The Departmental Committee of the Ministry of Transport which, under the chairmanship of Dr. G. W. C. Kaye, is studying the noise made by motor vehicles, has issued a Report which contains proposals concerning the maximum noise which such vehicles may be permitted to make. It is agreed that a loudness of 90 phons is the limit for " tolerably noisy vehicles," and it is suggested that this overall loudness at a lateral distance of 18 ft. should be the maximum permitted for a private car running under full throttle at 30 m.p.h. (presumably on a low gear). A somewhat similar limit is laid down for racing cars, but a temporary tolerance limit is proposed for motor-cycles and commercial vehicles which ordinary experience indicates as the chief offenders.

The first *Report* issued by Dr. J. J. Fox, the new Government Chemist, shows that the work of the various laboratories under his direction continues to increase, the number of samples examined during the year ending March 31, 1936, being no less than 546,279 —an increase of 23,491 over the previous twelve months. The increase was due very largely to the re-examination of tea affected by fire at the wharves, but the increased importation of canned fruits created a good deal of extra work. There were increases in the tobacco, beer, wines and cocoa departments and a marked fall in the number of samples examined by the laboratory as a result of the silk duty. Scattered among the routine details of the Report are many points of general interest. For example,

the water-content of imported cheese varied from 27·0 to 66·8 per cent. and the proportion of fat from 7·9 to 40·4 per cent. No less than 15 per cent. of the samples tested were prepared from milk containing less than one-half of its fat. " As, however, there are no regulations relating to the marking of skimmed or partially skimmed cheese, no exception could be taken to any of the importations." The proportion of fat in imported tinned cream also varied very considerably; of ninety samples tested two contained 50–52 per cent. of fat, the other eighty-eight from 19 to 30 per cent.! The samples of beer taken by Officers of Customs and Excise from the premises of publicans and retailers only occasionally showed evidence of dilution—to be precise, 36 samples out of 606, though in five of these the dilution was equivalent to 3 gallons of water per barrel. The samples submitted for examination by the Post Office included, as usual, paper, pigments and gum used in the manufacture of stamps, postcards and embossed envelopes, which are systematically tested to ensure freedom from deleterious substances. Samples of nicotine preparations and insecticides are examined to ascertain whether they are sufficiently free from the other constituents of tobacco to allow of importation as duty-free goods. In this connection it is noted that an alkaloid, anabasine, derived from *Anabasis aphylla*, a weed growing in Eastern Europe and Northern Africa, is being produced on a commercial scale as a substitute for nicotine.

The National Physical Laboratory has issued under the title *Physical Constants of Pure Metals* (H.M. Stationery Office, 6d. net) a series of tables containing data for metals of the highest degree of purity prepared in the Metallurgy Department of the Laboratory, together with similar data for metals of a high degree of commercial purity obtained in the Laboratory and elsewhere and compiled as a supplement to the International Critical Tables. A process has been worked out for the preparation of pure iron (containing about 0·013 per cent. of other elements), and a batch of this gave the following results : density 7·871 $\pm$ 0·002 gm. per ml. at 19° C. ; thermal conductivity at 25° C. $0·18_9$ cal. cm.$^{-1}$ sec.$^{-1}$ deg.$^{-1}$ C. ; mean coefficient of linear expansion 0°–100° C., $0·0_4112$ deg.$^{-1}$ C. ; resistivity at 0° C. 8·8 and at 100° C. 14·5 microhm-cm. ; maximum permeability 14,360 (H = 0·5 œrsted). The melting point of a slightly less pure sample was 1527° $\pm$ 3° C. Values of most of the constants tested above were obtained over a range of temperature and less complete data have been obtained for chromium, manganese, beryllium, cadmium, magnesium and tin. The pamphlet

contains Bircumshaw's results for the surface tension of certain liquid metals and a number of melting points, latent heats of fusion, specific heats at low, normal and high temperatures, thermal conductivities and coefficients of expansion. It is noted that in the three places in which the unit appears electrical resistivity is given in "microhms per cm^3."

The U.S. Bureau of Standards *Circular* C409 contains an interesting and valuable account of the heat treatment and properties of iron alloys, illustrated by equilibrium diagrams and microphotographs. The heat treatment of "high-speed steels" (18 per cent. tungsten, 4 per cent. of chromium and 1 per cent. vanadium) illustrates the complication of the process. They are in general (1) heated slowly and uniformly to about 870° C., then (2) heated more rapidly to 1260° C. for a short time, (3) next quenched in air, and finally (4) reheated to about 590° C. and cooled in air to develop the so-called "secondary" hardness. The *Journal of Research* for June, 1936, contains a paper with an unusual title, "An alternating-current magnetic comparator, and the testing of tool-resisting prison bars." The object of the research was to devise a method for testing the hardness of the iron bars fitted to prison windows *in situ*. These bars consist of a hard component embedded in a softer iron, and investigation showed that bars which possessed good tool-resisting properties gave hysteresis loops of a rather different shape from those which could be cut with a hacksaw. An inductance-capacity bridge arrangement was therefore set up with a coil wound on the test bar in one arm and a similar coil wound over a standard bar in another, and the distortion produced by the hysteresis measured for the third, fifth and seventh harmonics which it imposes on the A.C. supply.

The results obtained did not prove to be a very reliable test of the hardness of the bars, but as the author (R. L. Sanford) states, his instrument should be of general use in the field of magnetic analysis.

ESSAY REVIEWS

LISTENING TO A VOLCANO. By G. W. Tyrrell, A.R.C.Sc.,
D.Sc., F.G.S. Being a Review of **The Eruption of Mt. Pelée,
1929-1932**, by F. A. Perret. [Pp. 125, with 71 figures, 1 diagram
and 1 chart.] (Publication No. 458, Carnegie Institute, Washington,
1935.)

In 1902 Mt. Pelée, the famous volcano of Martinique in the French
West Indies, broke into active eruption with such peculiar and
unexampled violence as to destroy with an incandescent cloud the
city of St. Pierre with all but two of its thirty thousand inhabitants.
This eruption was of special interest to volcanologists as it brought
prominently to their notice two new, or at least hitherto unappreci-
ated, phenomena ; namely, the production of heavy, horizontally
or downwardly directed clouds of incandescent gases—the *nuées
ardentes* of French and the *Glutwolken* of German investigators—
and the slow upward thrusting of domes, with protruding spines or
monoliths of highly viscous lava from the crater at the final stage
of eruption. After many years of almost complete quiescence Mt.
Pelée once again broke out in a major eruption during 1929, with
the same phenomena as in 1902 but on a somewhat less violent
scale. The volcano was thus more approachable and more amenable
to close study than in 1902.

It was fortunate for volcanology that Mr. F. A. Perret should
have been available for the study of this eruption. As he tells
us in the volume under review, Mr. Perret is a physicist who became
interested in volcanoes very early in his career, and took up their
study when in 1902 he suffered a breakdown in health for which
a complete change of scene and occupation was prescribed. His
full scientific equipment and a facility for graphic, lucid and dignified
writing worthy of his awe-inspiring subject, have made Mr. Perret's
work of especial value to physical geology ; and with this memoir,
and with his earlier volume on *The Vesuvius Eruption of 1906*, have
led to writings not only of scientific but of literary standing. The
Carnegie Institute of Washington has published this work in its
usual sumptuous quarto style, printed on good paper that ensures

the worthy reproduction of a long series of magnificent photographic illustrations.

Mr. Perret's attitude to his subject and his methods of work may be illustrated by a quotation from his "Author's Note":
"Carried out mainly from the viewpoint of the physicist, the investigation here described has been, as far as possible, *lived* on the mountain itself, day and night in a hut amid penetrating ash, incessant noise and pungent smells, with the ruddy glare of the crater above shining through the open door; its activities now and then accompanied by the roar of a cloud-burst torrent or the silent rush of a great air-cushioned *nuée ardente* passing by. Excursion, observation, photography and intermittent sleep were the observer's lot, with thought only for the problems of the hour. Then comes the far more difficult task of co-ordinating, illustrating, deducing and drawing conclusions, together with the venture of adding another book to the world's vast library of thought."

Tourists camping on the summit of Mt. Pelée in August 1929 heard and felt rumblings and slight earth tremors. Fumaroles then became more active and their gases more acid. Strong explosions at the summit of the dome were heard on September 16. These were the preliminaries to the major eruption which began a month later and lasted three years.

The general history of the eruption comprised two fairly distinct phases, an early violently explosive phase consisting of the emission of numerous incandescent clouds followed, with some overlapping, by a milder phase of declining energy marked by the building of a new dome crowned by short-lived spines or monoliths. On October 14 a formidable series of explosions began and continued to increase in intensity. Probably these early explosions were ultra-vulcanian, consisting mainly of materials derived from the evisceration of the dome of 1902 by the ascending hot gases. About the middle of November strong luminous effects were observed at the summit and true *nuées ardentes* soon followed. They attained their maximum development in the middle of December, some equalling or even exceeding in magnitude those of the 1902 eruption. As an example of their size and speed of movement it is stated that an incandescent cloud on December 16, 1929, reached a height of 6,000 meters above the dome and rushed to the sea, four miles distant, in 2½ minutes. Early in January 1930 incandescent lava rose to vents within the rapidly-enlarging cavity that had been formed by the early explosions in the dome of 1902. The evisceration of the dome led to the formation of a typical crater and ultimately to a new volcanic dome.

From the continuous observations that Mr. Perret was able to make after his installation at St. Pierre in January 1930 it soon became apparent that the eruptive activity was rhythmic. The *nuées* were expelled in groups of varying duration, and alternated with the nearly gas-free emission of lava from the crater, producing a dome surmounted with spines. This alternation of modes of activity is clearly shown on a diagrammatic time chart. February 2, 1930, marked the beginning of an important change in the characters of the incandescent clouds. The avalanche material, instead of rolling over the ground as heretofore, began to slide, gouging out a furrow along its course and leaving a kind of medial moraine in its path. This behaviour clearly indicated diminishing gas-content and auto-explosivity in the ejected lava, and heralded the complete cessation of true *nuées ardentes* towards the end of the month. Thenceforward growth of the new dome was continuous except for occasional partial fragmentation due to specially violent explosions. A series of twelve wonderful telephoto views taken at intervals from the same viewpoint provides a perfect moving picture of the growth of the dome from April 1930 to February 1933. Spines or monoliths of nearly solid lava were constantly protruded from the growing dome, and as constantly collapsed after a few days into short trails of huge fragments. The eruption came to a virtual close towards the end of 1932, having lasted for more than three years. Mr. Perret's " Narrative " of the eruption touches a multitude of topics of entrancing interest to the volcanologist. In the following " Analytical Section " he treats the same subjects in a more general way, and relates his observations to those made at other volcanoes.

Not the least remarkable feature of Perret's investigation was the use he made of the microphone. The value of this instrument in volcanological technique was conclusively demonstrated by Perret during the great 1906 eruption of Vesuvius, in which it was used to follow the course of the eruption and, to some extent, helped to predict future events. Since that time the microphone has been frequently employed by volcanologists. The instrument is adapted for direct contact with the ground, gathering and magnifying molecular sound from the rock. Similar instruments were used during the War to detect mining and countermining.

Perret installed a portable microphone at a depth of two meters in the south wall of the Rivière Séche, wires strung on poles connecting it with the Station at St. Pierre. A quotation will illustrate the use of the instrument : " The sensitiveness of this simple contrivance was gratifying. Even footsteps in its vicinity were plainly audible, and reception of the preliminary rumble and discharge of

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every *nuée ardente* was all that could be wished. Visitors to the station, fascinated by the device, would put the phone to the ear and cry out at the sound, which a few moments later would be followed by a *nuée* emerging from the cloud-capped summit and descending into the valley below."

During this eruption Perret developed a new diagnostic method involving the use of the microphone, which he calls " dome resonance." Constantly listening to the sound emitted by the volcano he soon became conscious that no matter what the *quality* of the sound was, its *pitch* was always the same for a given day. " Whether it was the whistling of steam through a crack or its rushing through a larger vent, whether it was the clinkery rattle of a block slide or the roar of a great avalanche, a sensitive ear could always recognise the identity of the pitch even when the associated tones were octaves apart. Thus the dome as a whole acted as a resonant shell imposing its fundamental vibratory note upon all the minor vibrations just as the length of a pipe determines pitch, whether the mouth-piece be a reed, a whistle or the lips." It occurred to Perret that by following the variations in pitch of the sounds from day to day changes in the mass of the dome could be detected. If the volume increased the pitch of the sound should become lower, if the volume diminished the pitch should rise. Accurate determination of the pitch by means of an adjustable pitch-pipe calibrated against a standard tuning fork was the next step. When these observations began early in November 1930 the pitch of the dome sounds was exactly B-flat. Three months later it had dropped to A-natural. By April 13 the note had fallen to G-sharp, on June 4 to G-natural; but on August 10 it had risen again to G-sharp and continued rising regularly for some time. Any change in the mass of the dome must be the algebraical sum of gains from the accretion of new lava and losses from the formation of talus. Hence in the earlier part of the above period the dome must have gained in volume, the accessions of new lava more than counterbalancing the loss by talus formation. During August this relation must have been reversed, the supply of lava falling short of the loss by talus. Later on it was found by this means that the volume of the dome had again increased, and its growth then went on with unimportant oscillations until virtual stability was reached.

Two kinds of phenomena characteristic of Peléean eruptions—the *nuée ardente* and the volcanic dome—were intensively studied by Perret during this eruption. In the 1902 eruption controversy centred on the question whether *nuées ardentes* were initiated explosively or non-explosively. Lacroix championed the former,

Anderson and Flett the latter view. Perret manages to reconcile the contradictory theories, both based on undoubted facts, by pointing out, as Flett[1] did in 1903, that Peléean lava is auto-explosive. The ordinary volcanic explosion is due to extraneous energy ; the lava is projected by the explosive force of gases coming from below. But in *nuées ardentes* the lava is self-explosive. "The gas-charged substance, when the critical point is reached, suffers a sudden vesicular expansion throughout its entire mass, an explosive process so distributed that it may merely lift the material soundlessly, without marked violence, from its pocket and down upon a slope, where it descends gravitationally as a hot avalanche." Finding a weak spot beneath, or at the periphery, of the heavy dome carapace, the blast is projected horizontally as a dense mass of highly heated, heavily gas-charged and gas-emitting lava, much of it finely divided, extremely mobile, practically frictionless and almost noiseless, as each particle is separated from adjacent particles by a cushion of compressed gas. Strong electrification may give rise to brilliant lightning flashes and thunderous discharges. As the emission of gas lessens the velocity of the projection diminishes, and it finally drops its burden of ash, scoria and boulders as a tumultuous inchoate mass of material marking the course of the hot avalanche.

When the incandescent clouds decline in power and frequency a milder phase of activity, involving the building of domes and spines, gradually usurps the stage, and marks the approaching close of the paroxysm. This type of activity is frequent in volcanoes emitting acid or intermediate lavas of rhyolitic, dacitic, trachytic or andesitic composition. A fundamental distinction, depending largely on the chemical composition of the lava, can be made between volcanic vents that remain open between the major eruptions, and those that become closed. Volcanoes emitting the viscous, easily-solidified lavas of the above petrographic types, become partially or wholly sealed with great facility after the initial explosive phase, owing to the loss of gases and the comparatively low temperatures that are induced. The waning propulsive power of the still accumulating gas in the crater is only sufficient to thrust up the viscous or nearly solid lava in the form of a dome. The dome grows by accretions of lava oozing from small vents or fissures, and subsequently breaking into blocks which slide down its flanks, and add to the ever-increasing annular pile of talus at its base, an accumulation which may, in time, almost overtop the dome itself.

[1] T. Anderson and J. S. Flett, Report on the Eruptions of the Soufrière in St. Vincent, in 1902, and on a visit to Montagne Pelée in Martinique, Part I. *Phil. Trans.*, Ser. A., Vol. 200, pp. 353–553, Plates 21–39.

The monoliths which protrude from the dome like the spines springing from the carapaces of certain Mesozoic reptiles, seem to be thrust through weak places in the dome by gas pressure from below, like tooth-paste through the orifice of its tube. A complete spine is composed of a solid tip, an outer shell, and sometimes a liquid interior. Lack of support on the lower side of the slope on which it may be placed may cause the collapse of one-half of the monolith, leaving a characteristic form flattened on the outer and rounded on the inner side, from the base of which a sluggish trickle of lava often emerges. In the nature of the phenomenon these monoliths are evanescent structures which quickly crumble and contribute their quota to the ever-growing pile of talus at the foot of the dome.

Throughout the whole of his investigations Perret has emphasised what may be called the social aspect of volcanological science. Here again a quotation best serves to illustrate his humane point of view. He says : " Instead of endeavouring to suppress it (*i.e.* the social aspect), I have rather emphasised the practical application of purely scientific observation at critical moments in threatened regions. I am convinced that the human side of this science is sometimes overlooked to the detriment of its own best interests, for under what stimulus could exact data be more intensively sought, or in what manner more effectively gained, than under the imperious necessity of diagnosing eruptive conditions correctly through scientific observation and deduction, thus helping to save human life or preventing enormous economic waste by timely warning. For good or ill, humanity has always gathered around volcanic centres, unmindful of the danger. But here at Pelée, in the very theatre of the frightful catastrophe of 1902, it was only natural that a threatened repetition of the tragedy in 1929 should have created panic conditions and should have called for the utmost that volcanological science could give in the way of opinion and advice."

That advice was badly needed and promptly given at Martinique in 1929, for the preliminary eruption of September 16 of that year had an extraordinary effect on the population of the island, mindful of the terror of 1902. Fear at once rose to panic, and the next day not thirty persons were left in St. Pierre. As nothing further happened for a month, the inhabitants began to return, but the explosions of October 14 resuscitated the panic, and other towns on the north side of the island were evacuated. When he arrived in Martinique late in December 1929, Mr. Perret was besought by the authorities for an opinion as to the possible future course of the eruption. He asked for a week to make a preliminary investi-

gation. He concluded that : "After an outburst of the Plinian intensity shown in the eruption of 1902, when the materials accumulated for centuries had been blown from the crater and conduit, a milder period of periodic eruption should ensue, each new outbreak proportional in violence to the length of the quiet interval during which consolidation of lava and obstruction of the vent had been going on. In a published report to the people of Martinique I therefore expressed the view that the most dangerous explosive effects of the eruption were already over ; that present ejections were discharged through the Notch of the crater rim down the valley of the Rivière Blanche ; that all indications pointed to a gradually decreasing dynamic intensity ; and that, under watchful observation of the volcano, industry might well be resumed."

This report was published and broadcast, and the inhabitants gradually returned to their homes and business. It is gratifying to record that his predictions were fully justified by the event, and that the authorities and the people of Martinique showed their gratitude to Mr. Perret by their enthusiastic personal, financial and material assistance in his later investigations.

LOGICAL POSITIVISM. By H. DINGLE, D.I.C., A.R.C.S., D.Sc., Assistant Professor of Physics at the Imperial College of Science and Technology. Being a Review of **Language, Truth and Logic,** by A. J. AYER, Research Student of Christ Church, Oxford. [Pp. 254.] (London : Victor Gollancz, Ltd., 1936. 9s. net.)

No intellectual problem is more urgent at the present time than that of understanding precisely the character and possibilities of science, and no attempt to deal with this problem deserves more attention than that which has been called "logical positivism." English literature relating to this doctrine (the headquarters of which are in Vienna) is very scanty, and any exposition in our own language is therefore to be welcomed. But Mr. Ayer's book is welcome on other grounds also. It is written with extreme clearness, is remarkably free from the errors of syntax which seem to afflict some of those who identify the analysis of language with philosophy, and is altogether an admirable account of one of the most significant philosophical movements of our time.

The general idea of logical positivism is somewhat as follows. All genuine propositions fall into two classes—analytic and synthetic. Analytical propositions, of which the statements of pure mathematics afford a good example, are valid *a priori* because they are simply tautologies ; they express relations inherent in the definitions of the symbols they employ. The evidence of their validity is that

they cannot be denied without self-contradiction. They state nothing about the world of fact. Synthetic propositions, on the other hand, are statements about the world of fact, and they are significant only if they can, in principle, be tested by appeal to experience. Many apparently synthetic propositions (*e.g.* " the world of sense is real " ; " God exists " ; " murder is wrong ") cannot be so tested, and such propositions are called " nonsense." All sequences of words come under the three headings—analytical propositions, synthetic propositions, nonsense ; the first includes logic, the second, science, and the third, metaphysics, normative ethics, æsthetics, theology, and other remarks.

Mr. Ayer modifies this doctrine a little by relaxing the criterion for synthetic propositions. If verifiability by experience is necessary, all general propositions and all propositions about the past must be called nonsense. He would not like that, so he calls a proposition synthetic if any observations are *relevant* to the determination of its truth or falsehood, even though such determination is essentially impossible. Thus, if any sense experience contributes to the *probability* of the truth of a proposition, that proposition is significant and synthetic.

The logical positivists have undoubtedly rendered a great service to thought by insisting on this clear-cut classification. Although some of its implications are startling, and at first even revolting, it appears to be unassailable when closely examined. But there is no clear justification for stigmatising one of the classes of propositions by the emotive word, " nonsense," which not only arouses prejudice in the reader but represents an actual contempt on the part of the logical positivists themselves. Both analytic and synthetic propositions are considered worthy of study—by logicians and scientists, respectively, while philosophy is the act of classification—but nonsense appears to deserve no consideration at all. Nevertheless, we cannot avoid it in actual life. Judging from the ingenuous and wholly charming way in which Mr. Ayer announces his intention of solving all the traditional problems of philosophy, we imagine that he is too young to have been faced with the problem familiar in the early days of the Great War, " Is it right to join the Army or not ? " We cannot refute him when he claims that the question belongs to a different class from the question, " Is sugar soluble in alcohol ? " but after we had decided that it was nonsense we had to set about answering it.

The fact is that Mr. Ayer cannot reject nonsensical statements without, by his own criterion, becoming nonsensical himself—a fact which Wittgenstein realised clearly enough. How, for instance,

are we to classify the sentence expressing his principle of verifiability ? It is not analytic, for we can deny it without self-contradiction ; neither is it synthetic, for sense observations are not relevant to its truth or falsehood. It is therefore nonsense. There is an engaging naiveté about Mr. Ayer's reason for rejecting the original criterion of absolute verifiability, which is simply that he does not like its consequences.

This nonsensical basis of Mr. Ayer's scheme is constantly perceived in various aspects as one follows his discussion. He tells us, for instance, that ethical judgments have " no objective validity whatsoever." But what is " validity " ? Clearly it is impossible to speak of ethical judgments as having no validity, and of synthetic propositions as having validity, unless one takes up a standpoint outside both and compares them on the common basis of " validability." What determines validability ? It cannot be verifiability, because that merely *defines* synthetic propositions and therefore offers no basis of comparison. Mr. Ayer is, in the same breath, denying the sense of valuing and practising the art of validating.

There appears to be a similar confusion with regard to the idea of certainty, which Mr. Ayer uses metaphysically. He gives a " proof " that no " genuine " synthetic proposition can be " absolutely certain " : " it is only tautologies that are certain." But what is certainty ? If it is that which characterises an analytic proposition, then a synthetic proposition by definition cannot admit of it, and " proof " is meaningless. If, on the other hand, it is something general which happens to be attached to analytic propositions but not to others, it is metaphysical. Mr. Ayer seems to have in mind some sort of feeling of conviction when speaking of certainty, but by what right this intrudes into his philosophy is not clear.

There are other difficulties which we can barely mention. For instance, Mr. Ayer justifies the statement, " Other people exist," but rules out the statement, " God exists," yet an analysis of his arguments shows that, on his own ground, the two statements should stand or fall together. Again, the problem of time is neglected—an inexcusable omission when one considers the possibilities of the proposition, " There will come a time when 2 plus 2 will equal 5." But while logical positivism is far from perfect, its defects do not, we believe, belong to its essence—if it will allow the word. It has a meaning of the greatest importance at the present time, and we owe a debt of gratitude to Mr. Ayer for giving us such an excellent account of it.

REVIEWS

MATHEMATICS

Interpolation and Approximation by Rational Functions in the Complex Domain. By J. L. WALSH. American Mathematical Society Colloquium Publications, Vol. XX. [Pp. ix + 382, with 3 figures.] (New York: American Mathematical Society, 1935. $5.00.)

In his preface the author suggests as an alternative title : " Generalizations of Taylor's Series " in the sense of representations of functions of a complex variable by series of polynomials or, more generally, of rational functions with selected poles. The subject matter and the lines of approach are clearly indicated by the chapter headings : I, II, Possibility of Approximation ; Analytic Functions ; III, Interpolation and Lemniscates ; IV, Degree of Convergence of Polynomials, Overconvergence ; V, Best Approximation by Polynomials ; VI, Orthogonality and Least Squares ; VII, Interpolation by Polynomials ; VIII, Interpolation by Rational Functions ; IX, Approximation by Rational Functions ; X, Interpolation and Functions Analytic in the Unit Circle ; XI, Approximation with Auxiliary Conditions and to Non-Analytic Functions ; XII, Existence and Uniqueness of Rational Functions of Best Approximation. There is also a bibliography of nearly 150 items of which 38 appear under the author's own name.

The investigations carried out during the last decade on the two main lines of approach, namely by interpolation on the one hand, or by extremal properties on the other, seem to have established a reasonably complete working theory, though there is no doubt that much remains to be done in the direction of greater precision. The present monograph claims to be written both for the beginner and the specialist, but the former would need a guiding hand in his first survey of the text. Nevertheless, Chapter III on interpolation by polynomials, and much of Chapter V on the Tchebycheff approximation may be read without special preparation and, introducing the beginner in a pleasant way to a fascinating subject, may tempt him to read further.

This is the 20th volume of the Colloquium Publications of the American Mathematical Society, a series of monographs by which the results of recent work have been made available promptly in a convenient form and at a reasonable cost. In time this, like other volumes of the series, will be replaced by a more definitive account ; for the present it is admirably designed for its purpose.

E. L. I.

Geometry of Time and Space. By ALFRED A. ROBB, Sc.D., D.Sc., F.R.S. [Pp. vii + 408, with 57 figures.] (Cambridge : at the University Press, 1936. 21s. net.)

THIS volume may be considered as a second edition of *A Theory of Time and Space*, published in 1914, and gives a more complete discussion of the idea of *conical order*. Its purpose is to show that the geometry of Minkowski space-time can be developed from a number of postulates without recourse to a system of co-ordinates. No spatial concepts are involved, and it is demonstrated that " spatial relations are to be regarded as a manifestation of the fact that the elements of time form a system in conical order : a conception which may be analysed in terms of the relations of *after* and *before*." Starting with the concept of conical order applied to a set of elements, the geometry of the set is developed by means of 21 postulates and some 206 theorems. It is shown that there occur lines of three different types (corresponding in relativity to time-like, space-like, and null lines), and that there can be formulated a definition of congruency for comparing segments of similar lines. This leads to the idea of " distance " along a line, and co-ordinates x, y, z, t are then attached to each element of the set in such a way that x, y, z are distances along three separation (space-like) lines, and t is distance along an inertia (time-like) line. The postulates and propositions can now be expressed in terms of algebra, and " the question of the consistency of the whole twenty-one postulates is reduced to analysis." Finally, the geometric relations are given physical interpretations, the co-ordinate system now having the properties usually associated with the Minkowski system.

This work will make interesting reading for the geometer who is attracted by the Euclidean style. The postulates and definitions are clearly stated, and most of the theorems quickly proved, good figures being provided where necessary. The relativist, however, will be disappointed, for there is no good reason for believing that the geometric relations can have the physical interpretations here given. In the first place, it would hardly be possible to devise experiments that could test some of the postulates. The second, and more serious, objection is that there is no apparent relation between the distances defined in this geometry and the corresponding laboratory measurements ; there is no practical method for comparing intervals of space or time corresponding to the geometrical definition of congruency.

A. G. W.

Graphical Solutions. By C. O. MACKEY, M.E., Assistant Professor of Heat Power Engineering, Cornell University. [Pp. viii + 130, with 53 figures and chart in pocket.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1936. 12s. 6d. net.)

THE subjects dealt with are : stationary adjacent scales, slide rules, intersection charts, alignment charts, and the fitting of empirical equations to experimental data. These are all of considerable use to the practising engineer, and it is very desirable that all students of engineering should receive some instruction in them as part of their mathematics course. In this book the methods are explained with great clarity, and illustrated by numerous worked examples, so that an intelligent student should easily be able to learn all that he requires without further aid. There are also very many exercises for the student to work out himself. The chapter concerned

with finding equations to represent empirically the results of observation is limited to non-periodic curves, and it is good to note that the author warns his readers of the dangers of using an empirical equation beyond the range of the data on which it is founded.

The mathematical knowledge demanded of the reader is limited almost entirely to simple algebra and logarithms, with a very little plane geometry.

A useful feature of the book is a sheet of logarithmic scales, enabling the reader rapidly to draw such a scale of any size up to 10 in.

The price of the book seems rather high.   J. C.

Analytical and Applied Mechanics. By G. R. CLEMENTS, Ph.D., and L. THOMAS WILSON, Ph.D., Professors in the Department of Mathematics, United States Naval Academy. [Pp. x + 420, with 343 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1935.   21s. net.)

THIS book is intended " to give a simple but vigorous discussion of the mathematical and physical theory necessary for a thorough first course in mechanics." For students with a strong aptitude for mathematics, the book certainly fulfils its intentions. After a preliminary chapter on vectors, the authors proceed to cover the usual ground in statics, the elements of elasticity, kinematics and dynamics. The treatment throughout is thorough, and the authors make free use of mathematics as required. The statements are always clear, but never novel, and are illustrated by numerous worked-out examples, in addition to 912 examples for students, with answers. This is a most valuable feature of the book. It can only be considered a " first-course " book if the student is proceeding to an honours degree in Engineering Science ; it is too advanced for the pass degree student. The book should be extremely valuable to teachers, and the reviewer, as a teacher himself, expects to find it a most useful addition to his library.

J. C.

ASTRONOMY

Astronomy. By ROBERT H. BAKER, Ph.D. Second edition. [Pp. xix + 522, with frontispiece and 286 figures.] (London : Macmillan & Co., Ltd., 1935.   16s. net.)

PROF. BAKER's *Astronomy* was first published in 1930, and secured very favourable notices in this and other scientific journals. Within a year two reprints were found necessary, and now, in the second edition, the author has taken the opportunity to bring it completely up to date by including the new material made available by the recent rapid advances in stellar astronomy. In order to avoid increasing the size of the book, an equivalent amount now judged to be of minor importance has been omitted.

It is primarily intended as a text-book for the use of students possessed of but slender equipment in mathematics or physical science. In America, where an elementary course in astronomy forms a popular feature of nearly every college curriculum, it should answer its purpose admirably and be in great demand. In this country, on the contrary, the study of general and descriptive astronomy is not encouraged, and the subject invariably goes hand in hand with mathematics. A text-book which can be read intelligently without a knowledge of even the elements of spherical trigonometry is hardly likely to commend itself to our educational authorities. For general reading

and reliable reference, however, the book should be found well worth a place on the book-shelves of all practical astronomers as well as of those whose interest in the science is confined to the arm-chair. The illustrations are many and well chosen, and the index is comprehensive.

R. W. W.

PHYSICS

Foundations of Physics. By R. B. LINDSAY, Ph.D., and H. MARGENAU, Ph.D. [Pp. xvi + 537, with 29 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1936. 22s. 6d. net.)

THIS excellent book is designed to fill the gap between popular discussions and highly technical pioneer books on recent mathematical physics. " The authors have tried to approach and discuss the fundamental problems of physics from a philosophical point of view ; at the same time they have not hesitated to use mathematical analysis where it is essential for clear understanding, since analysis is the language of physics. Much of the book is thus superficially similar in appearance to a text on theoretical physics. It must be emphasised, however, that it has been no part of the authors' plan to write such a text, and the book as written must not be confused with one." This is a just description. The titles of the chapters indicate the scope of the treatment—" The Meaning of a Physical Theory " ; " Space and Time in Physics " ; " The Foundations of Mechanics " ; " Probability and Some of its Applications " ; " The Statistical Point of View " ; " The Physics of Continua " ; " The Electron Theory and Special Relativity " ; " The General Theory of Relativity " ; " Quantum Mechanics " ; " The Problem of Causality." The temper and clearness of the discussion are admirable, and we can wholeheartedly recommend the book in its entirety to all whose mathematical attainments enable them to deal with tensor calculus and matrix theory, and in its descriptive parts (which are numerous, ample and separable from mathematical details) to all who are interested in the nature and significance of physical concepts. Special mention may perhaps be made of the treatment of causality, which is the most satisfactory that we have read and with respect to which the authors cannot be too highly commended for ignoring the popular idea that recent physical developments have something to do with human free-will. It does not follow, of course, that the discussion is so barren as to be beyond criticism. It seems to us, for instance, that the authors, in identifying causality with " consistency of nature," have not quite succeeded in expressing " the nucleus of what is understood by causality." This is perhaps evident when one reflects on what they consider would be a case of failure of causality : " Using the more intuitive definition according to which a state A is always followed by the same state B the failure is evident when we realise that state A may occur only once. Causality is then an empty phrase unless the universe is periodic." " Consistency " may be an empty phrase, but " causality " is not : if an earthquake occurred only once, we should still consider it the cause of certain damage. Or, to put the matter more generally—consider a state A' (preferably as similar to A as possible) which is invariably followed by a state B' : then the factors by which B differs from B' would be said to be caused by the factors by which A differs from A'. But such matters give scope for infinite discussion. It is sufficient to say that we know of no book of its kind that we can recommend with less reservation than this.

H. D.

Hydrostatics. By A. S. RAMSEY, M.A. [Pp. viii + 169, with 107 figures.] (Cambridge : at the University Press, 1936. 7s. 6d.)

THIS well-written text-book is intended primarily for first-year students in the Universities, but it is hoped that it will also serve for pupils taking advanced courses in schools and for scholarship candidates.

The subject matter is divided into nine chapters, one of which deals with gases and another with an elementary study of capillarity. The remaining chapters discuss centres of pressure, the stability of floating bodies, hydrostatic machines and some simple examples of fluid equilibrium.

A valuable feature of the book is the number of worked examples which it contains. Students will find these very helpful in elucidating the text and in supplying hints for working the large and varied collection of examples to be found at the end of each chapter. The book requires an elementary knowledge of the Calculus and of the simpler properties of conic sections, but it should be well within the compass of the students for whom it has been written. The treatment throughout is lucid and interesting ; the book contains many excellent diagrams and is well printed. It should fulfil a very useful purpose for students of Physics and Applied Mathematics.

SYDNEY MARSH.

Sound : An Elementary Textbook on the Science of Sound and the Phenomena of Hearing. By F. R. WATSON. [Pp. ix + 219, with 179 figures.] (New York: John Wiley & Sons, Inc.; London : Chapman & Hall, Ltd., 1935. 12s. 6d. net).

IN the words of the author, " This volume has been written for two classes of readers : first, those who want a non-mathematical treatment ; and second, students who have taken elementary mathematics and physics, but who desire further work in physics." Non-mathematical treatment is always bound to involve a lengthy presentation, to which rule the present volume is no exception : the first hundred pages are devoted mainly to the study of progressive and stationary waves in gases and on strings, including chapters on diffraction and the Doppler effect.

Chapters on such matters as musical scales, musical sounds and instruments, speech and hearing, and the acoustics of rooms are followed, curiously enough, by a consideration of the velocity of waves. Then after dealing with the energy of sound, reflection absorption and transmission, resonance and resonators, the volume closes with very brief accounts of some possible laboratory or lecture experiments. Examples requiring descriptive or easy mathematical answers follow each chapter.

A few statements seem to need modification or amplification, e.g. it is a little surprising to read that, " This variation in diffraction " (i.e. the sound entering through an open window spreads everywhere inside the room, whereas sunlight, similarly entering, scarcely spreads at all) " is probably the most marked difference between the action of light waves and sound waves." In the section dealing with resonators the statement that " a heated wire . . . is strongly affected " could be extended with advantage, whilst simplification can be the only justification for the sand particles on p. 101 settling down at the nodes.

Fundamental ideas receive very full exposition with the liberal aid of diagrams and the book can be commended to anyone desirous of studying as non-mathematical a presentation of the subject as is feasible.

R. E. G.

A Fugue in Cycles and Bels. By JOHN MILLS. [Pp. viii + 269, with 34 figures.] (New York : D. van Nostrand Co., Inc. ; London : Chapman & Hall, Ltd., 1936. 13s. 6d. net.)

DURING recent years there has been a curious but unmistakable drift together of electricians and the technologists of music, and the symbol of their gathering has been the sine curve.

Because electrical particles under electrical pressure exhibited the same reactions as elastic mechanical particles under mechanical pressure, the equations of one science became applicable to the other with no more than simple conversions of the formulæ.

The Author describes this state of affairs, what has led to it, and what he anticipates the outcome will be, but refrains from the use of mathematics.

The book is therefore for the general reader rather than for the technician ; nevertheless it is a valuable contribution to scientific literature in that it describes in clear terms how Science has applied itself to the analysis of Musical Sound.

The difference between an art and a science is that the one is the direct result of human skill with mind and muscle, while the other is the product of exact instrumental observation, and it is much to be doubted whether Science will ever be able to reduce to a series of graphs and equations all that is in even one art.

The Author appears to believe that any musical note can adequately be contained within the framework of a Fourier series, but what of that independent and not necessarily harmonic component, the " formant," present, for instance, in the tones of a violin and certain organ pipes ?

So long as he discusses the purely scientific the Author is on safe ground, but fully to co-relate Science with the Art of Music would require a separate book for every instrument of the orchestra and for every stop in the organ.

E. B.

A Class Book of Magnetism and Electricity. By H. E. HADLEY, B.Sc. [Pp. x + 512, with 432 figures.] (London : Macmillan & Co., Ltd., 1936. 6s. 6d.)

THIS text-book is intended for pupils taking the School Certificate and Matriculation Examinations. The number of text-books on the market which have been written for that purpose is already very considerable and in adding to their number the author is content that his book be judged by its fitness and general suitability. On the whole the subject matter follows the traditional order, Magnetism, Electrostatics and Current Electricity, and is quite free from " fads and fancies ". The descriptive parts of the text are well written and the author throughout has paid special attention to the theoretical aspects without a knowledge of which little progress is possible. There is a surprising amount of matter contained within its covers and the last 100 pages, dealing in the main with modern developments, are particularly interesting. Historical notes and exercises are added to each chapter. The book is excellently printed and is the product of an experienced writer and teacher. In the opinion of the reviewer it will prove to be a very interesting and useful text-book for elementary students.

SYDNEY MARSH.

Magnetism. By E. C. STONER, Ph.D. Second edition, revised and enlarged. Methuen's Monographs on Physical Subjects. [Pp. vi + 136, with 20 figures.] (London : Methuen & Co., Ltd., 1936. 3s. net.)

FROM a pedagogic point of view, magnetism is a very difficult subject, and students do not readily acquire a satisfactory knowledge of many aspects. There is little doubt that the best approach is made rather from the experimental than from the theoretical side, and that is why this little book is particularly valuable, for it may be so advantageously used to supplement an experimental treatment. Its several chapters can be practically taken as units and read in almost any order to suit the reader's fancy, and from it students may obtain an excellent introduction to modern theoretical magnetism.

L. F. B.

Electron Diffraction. By R. BEECHING, A.R.C.S., B.Sc. Methuen's Monographs on Physical Subjects. [Pp. viii + 107, with frontispiece and 39 figures.] (London : Methuen & Co., Ltd., 1936. 3s. net.)

WE welcome this book on Electron Diffraction. Mr. Beeching, who is at the Imperial College, has not only worked at the subject, but has also had the advantage of being in direct contact with Prof. G. P. Thomson, Prof. G. I. Finch and others, who have been largely responsible for initiating and developing the subject.

A brief outline is given of De Broglie's theory of 1924, and its subsequent applications. The experiments of Ramsauer on the scattering of electrons by monatomic gases (1921) are mentioned, and the noted work of C. Davisson and L. H. Germer, G. P. Thomson and A. Reid is described. Diffraction by transmission and by reflection are considered in turn, and the last two chapters are devoted to modern experimental methods and applications.

In the Preface, which is contributed by Prof. G. P. Thomson, the twofold interest of the study of Electron Diffraction is pointed out. On the one hand it supplies the most direct evidence for wave-mechanics, and, on the other, it provides one of the most promising lines of approach to the study of surfaces. The book illustrates well the wealth of information concerning the structure of surfaces that is obtainable by this means.

W. N. BOND.

Thermionic Emission. By T. J. JONES, M.Sc. Methuen's Monographs on Physical Subjects. [Pp. viii + 108, with 17 figures.] (London : Methuen & Co., Ltd., 1936. 3s. net.)

THE first three chapters of this book describe the early experimental work on the emission of electricity from hot bodies, summarise the theories that have been developed, and give some account of the modern experimental methods. Students who are seeking a general survey of the subject will find this account concise and up to date, with many references to original papers.

In the early experiments on thermionic emission, considerable difficulty was experienced in obtaining uncontaminated metal surfaces and in keeping a sufficiently good vacuum in the apparatus. Of recent years, however, experimental technique has so far advanced that the impurities can be controlled, and their effect on the emission investigated. The second half

of the book deals with the emission from metals, the surfaces of which are covered by one or more layers of other substances, the emission sometimes taking place into a rarified vapour. The book is packed with information and it should be of considerable use to anyone working at problems in thermionics. The subject has, however, become extremely complex, and would probably be left undeveloped, were it not for its very important technical applications.

W. N. BOND.

Phenomena in High-Frequency Systems. By AUGUST HUND. International Series in Physics. [Pp. xv + 642, with 359 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1936. 36s. net.)

THE science and practice of radio communication have made such progress of recent years that it is becoming increasingly difficult to present in a single volume an accurate and intelligible account of the present state of our knowledge on this subject. This difficulty is demonstrated very well in the book under review, which covers, in some considerable detail, the fundamental principles of radio communication and the technique of its many applications.

The work may be regarded as a companion volume to the author's previous book on *High Frequency Measurements*, which was reviewed in SCIENCE PROGRESS for April 1934 (p. 748). With this fact in mind it is natural to find that the present book does not deal with the principles of high-frequency laboratory measurements, nor with the subject of modulation, to which a chapter was devoted in the former book. The first two chapters of the new work describe the phenomena accompanying thermionic emission and the many and various types of valves which are used for the generation of electrical oscillations. The next two chapters, dealing with " Voltage and Current Changes " and " Phase Changes " respectively, are very short and could probably have been omitted without affecting the rest of the book. The subjects of frequency multiplication, rectification, amplification and the use of piezo-electric crystals for frequency control, are then dealt with and lead us to rather more than half the book. Next a chapter on " Electromagnetic Theory " is followed by one on the " Ionised Layer " and the part it plays in wave propagation. The concluding chapters of the book are devoted to the principles of directive antenna systems for transmission and reception, and the theory of networks and transmission lines. An appendix of 15 pages contains a useful selection of mathematical formulae and tables.

The book is not suitable for the student of elementary radio communication, but is perhaps appropriate for the expert and advanced worker who desires to have in one volume a comprehensive treatment of the theoretical principles of the subject. While the difficulties of the author who is sufficiently daring to undertake such a comprehensive task are appreciated, it must be remarked that some parts of the work are by no means easy to follow, not on account of the mathematical standard of knowledge demanded but rather due to the method of presentation. After perusing one or two chapters dealing with portions of the subject on which the reader may lay claim to some expert and detailed knowledge, he may be left with a feeling that the author has attempted to cover too much ground outside his own particular sphere.

The reviewer is forced back to the opinion that the presentation of an account of the detailed principles and technique of this vast and rapidly growing subject should be covered by a series of monographs rather than by a single weighty volume.

R. L. SMITH-ROSE.

An Introduction to the Theory of Elasticity. By R. V. SOUTHWELL, M.A., F.R.S. The Oxford Engineering Science Series. [Pp. x + 509, with 120 figures, including 2 plates.] (Oxford: at the Clarendon Press; London: Humphrey Milford, 1936. 30s. net.)

MOST teachers of engineering must have felt the need for an authoritative text-book which would bridge the wide gap between the subject commonly known as Strength of Materials and the mathematical study of elasticity. Honours and research students also have found the lack of such a book a drawback which has not been easy to overcome and the publication of the work now under review by one so eminently suited for the task as Prof. Southwell is a matter for congratulation. As an elastician, an engineer and an engineering teacher of considerable distinction, no one knows better than the author the needs of the student and he has met these in an altogether admirable manner.

The book is divided into fourteen chapters, of which the first seven and last two cover the ground needed by all engineering students. These include the conception of elastic behaviour, the theory of bending, torsion, etc. Chapters VIII to XII deal with the general analysis of stress and strain, the equations of elasticity and their solution and applications to specific problems. These are exactly what are needed by the more advanced student to serve as an introduction to the study of more specialised works dealing with the mathematical theory of elasticity.

The author's approach to his subject is unusual in one very important respect. It is customary to precede a study of the behaviour of structures by a discussion of stress and strain, and Hooke's law is always stated in terms of these quantities as a preliminary. Prof. Southwell, however, reverts to the original statement of the law that for any "springy body" deflections are proportional to the forces which produce them, and he then, without imposing any restriction as to the shape and size of the loaded body, examines the consequences of this statement. This leads at once to the principle of superposition, the reciprocal theorem and the theorems of Castigliano, and the student finds himself well launched upon the study of structural theory in the first few pages of the book. The elementary theory of stress and strain is not given until Chapter IV, and, startling as this innovation may appear at first sight, it has resulted in a very clear and logical presentation.

The theorems referred to above are presented in a convincing manner, and indeed an outstanding characteristic is the refreshingly new treatment of so many old and familiar problems. This alone made it a very real pleasure to read the book.

The inclusion of a large number of well-selected examples and exercises adds to its value, and it can be recommended without reserve to all honours students of engineering and physics. Research and post-graduate students in this branch of work and teachers of the subject will find it indispensable.

The production of the book is in every way excellent and author and publisher are alike to be congratulated on such an addition to the literature of engineering science.

A. J. S. P.

The Young Engineer. By J. N. Digues la Touche, M.Inst.C.E. [Pp. xii + 242, with 3 figures.] (London : The Technical Press, Ltd., 1936. 5s. net.)

This book is described as " a manual dealing with Indian practice designed to assist students of engineering generally, and particularly those requiring a knowledge of colonial methods."

The author, who was Chief Engineer of the Indian State Railways, has been at pains to place on record in this book many tips which should be of great benefit to the young engineer.

The book deals mainly with railway work and contains chapters on office routine, contractors, surveying, construction, earthwork, bridging, foundations, boring, masonry, plate-laying, ballast, fencing, maintenance, workshop practice, rates, systems of working, signals, a train ferry, and inspection trollies.

The treatment of the above subjects is for the most part quite general in the sense that the book does not profess to be an engineering manual. It aims, however, at placing before the reader numerous practical hints on points which he would be unlikely to find in standard technical works.

In certain essential respects Indian railway practice differs from British. For example, the majority of " Contractors " in India are, in the words of the Author, " merely financiers or importers of labour," and, as such, have to be treated in a totally different manner from their English counterparts. The chapter on this subject is of particular interest and should be of great use to any engineer proceeding to India for the first time to take up a post.

Survey books are, as a rule, somewhat reticent on the subject of surveying abroad where special difficulties have to be overcome, such as the training of native assistants, climatic and other troubles. Many useful hints and suggestions are given in this book on the subject. Similar remarks could be made about the contents of the remaining chapters. Altogether it is a book which should be in the hands of every young engineer, whether he is going to India or staying at home. The only real criticism which could be made relates to the title, which does not indicate the real scope of the book. It could be more accurately termed " Hints on Indian Railway Practice for the young Engineer."

G. H. H.

Practical Hints on the Installation of Heating Apparatus, with Brief Notes on Ventilation. By E. G. Blake, M.R.S.I. [Pp. viii + 119, with 29 figures.] (London : The Technical Press, Ltd., 1936. 2s. 6d. net.)

The title would lead one to expect really helpful information, of service to both designer, fitter and user of heating apparatus.

The scope of the book is good, but the arrangement is somewhat haphazard and chapter headings not explicit. The chapters on " Fireplaces " and " Troublesome Chimneys " are useful, and the remainder of the book

contains many good practical hints, but the scientific explanations given by the author are very misleading.

Concerning the circulation of hot water, the explanation on p. 3 is quite wrong, and is repeated on p. 5. The same error occurs concerning air on pp. 12 and 16. A glaring misstatement, concerning the resistance of a bend in an air duct, occurs near the foot of p. 11, while overleaf an unqualified rule for ventilation of a room, of " four air changes per hour," is given. The illustrations are clear, but Fig. 16, p. 70, is unfortunately drawn showing the tilt opposite to that described in the text.

The book may be of help to young apprentices, but further commendation is impossible, owing to its unscientific basis.

E. L. J.

CHEMISTRY

Inorganic Chemistry. By N. BJERRUM. Translated by R. P. BELL. [Pp. x + 317, with 17 figures.] (London : William Heinemann, Ltd., 1936. 7s. 6d.)

THIS book should be read by all chemists interested in general chemistry, for amongst books dealing with inorganic chemistry it is outstanding for the clear and concise way in which the fundamental principles are presented. Especially noteworthy are the sections dealing with the modernised theory of acids and bases and the associated subject of ionisation and reaction in solution, which reflect the authority of an acknowledged master in this field of work. Naturally in a volume of this size the author has been compelled to curtail the space allotted to certain aspects of the subject, and it should be mentioned that scanty treatment is given to the historical development of chemistry and to the metallurgy of the elements. The reviewer noted this statement on p. 21, line 5 from bottom, " . . . it is only those gases for which Henry's law holds that can be removed in this way " (viz. completely removed by boiling). Surely ammonia-free water can be obtained ? Again, p. 162, it might be advisable to point out that it is questionable whether amorphous carbon, as distinct from graphite, really exists. On p. 176, bottom line, occurs the following statement : " Later investigations *showed* that this is only exactly true at absolute zero." This seems misleading, as does the paragraph on p. 203, line 12, which begins, " Recent investigations have shown that even the small molecules in true solutions are able to scatter a very small amount of light sideways . . ." It would be more correct to say, " It has been known for a long time. . . ." Finally, the section dealing with photography (p. 268) does not maintain the up-to-date character of the rest of the book. However, these are small matters in a book which is highly reliable and most stimulating. Mr. R. P. Bell has given a fine translation which is published in a most attractive form.

W. W.

Sulfuric Acid Manufacture. By A. M. FAIRLIE. American Chemical Society Monograph Series, No. 69. [Pp. 669, with 192 figures.] (New York : Reinhold Publishing Corporation ; London : Chapman & Hall, Ltd., 1936. 48s. 6d. net.)

THE author is to be congratulated on performing a difficult task in a very able manner. He has endeavoured to produce a book on sulphuric acid

manufacture to be of practical use not only to the chemical engineer and to the manufacturer and consumer of sulphuric acid, but also to the University student. The size of the present work indicates that it is intended to be neither elementary nor exhaustive. In order to keep within the limits of a single volume (a highly desirable end in itself), the author has wisely resisted the temptation to expand the treatment of certain aspects of his subject, and pertinent information which is readily obtainable elsewhere has been omitted, abundant references being provided which will be found of great use to those who wish to seek fuller information on specific topics. The result is a volume of convenient size which is not merely a compendium, but which represents a real contribution to the literature of sulphuric acid and its manufacture.

The book, which comprises twenty-two chapters and fifteen appendices, commences with a brief historical survey, followed by a discussion of the physico-chemical considerations involved in the production of sulphuric acid. Construction and production materials are then dealt with, and then burners, roasters and furnaces. Both nitration and contact processes are described as practised in various parts of the world, together with the many interesting modifications in technique which have been introduced during the past decade. The chapters dealing with nitration processes include lead chambers and chamber substitutes, recovery of oxides of nitrogen, auxiliary equipment, and plant design and operation. Much of the subject matter relating to contact processes is excellent, and many readers will find that which deals with the controversies in Europe of special interest. A very full comparison of platinum and vanadium catalysts is made, and this section of the book is completed by a consideration of the operation and control of contact processes in general, and of some industrial installations involving special types of contact plant. The final chapter is devoted to the mixing and shipping of sulphuric acid, hazards and safety measures, costs and cost-accounting, choice of process, trends in the industry, and other matters of general interest. The appendices, which form a useful feature of the book, relate to hydrometers, physical and other data on sulphuric acid, brick shapes, sizes and weights of lead pipe and sheet lead, general conversion tables, and nomographs for finding specific volumes of sulphuric acid solutions.

It is hardly likely that a work of this kind will fail to provoke a certain amount of criticism; for instance, in regard to the emphasis laid on American practice and in regard to some of the views expressed as to the control of sulphuric acid plants. Taken as a whole, however, and bearing in mind the wide experience and high reputation of the author, this book is well deserving of the careful study of all who are concerned with the technical and administrative side of sulphuric acid manufacture in whatever capacity.

H. W. CREMER.

The Phenomena of Polymerisation and Condensation : A general discussion held by the Faraday Society, September, 1935.
[Pp. vi + 412, with frontispiece and numerous figures.] (London : Gurney & Jackson for the Society, 1936. 22s. 6d. net.)

THE Faraday Society has done a real service to physical chemistry by arranging a discussion on polymerisation. Unlike the subjects of many previous discussions, the phenomena of polymerisation have only received scant attention in the text-books and the number of monographs so far published

is small and somewhat specialised in their treatment. In this volume the subject is surveyed for the first time by leading investigators in an extensive manner.

It is not possible to mention all the topics brought up for discussion in this review. The papers contributed were divided into two sections, one dealing with general aspects while the other was more particularly concerned with individual polymerisation and condensation reactions. The natural classification appears to be (a) the mechanism of the growth of polymers and (b) the structure of the resulting product. Accordingly we find papers dealing with the kinetics of polymerisation in the gaseous and liquid phases, in two dimensions as well as in three. In these, attempts have been made, with some success, to find the nature of the collision types responsible for the initiation, growth and cessation of the growth of polymers.

Where kinetic evidence fails to yield further evidence it is fortunate that X-ray analysis provides confirmatory and complementary data on the structure of polymers. The advances made in this section are reported in a number of papers.

Apart from their purely academic interest, polymers are becoming of ever-increasing importance in industry, and hence it is necessary to determine, control and improve their mechanical properties. We find consequently a number of papers dealing with this aspect, which show incidentally how far the mechanical properties could be improved if methods were developed to modify the growth and mode of linking of the monomeric molecules.

Among the general papers may be noted a comprehensive introduction to the whole subject by E. K. Rideal, a review of the work done by W. H. Carothers, an account of the researches of Staudinger and his school, and a survey of the contribution made by X-ray analysis by J. R. Katz.

The book will therefore appeal to a wide circle of readers. It forms a valuable introduction to the study of polymers as a whole.

<div align="right">H. W. MELVILLE.</div>

Electrolytic Oxidation and Reduction : Inorganic and Organic.
By S. GLASSTONE, D.Sc., Ph.D., F.I.C., and A. HICKLING, M.Sc., Ph.D. Vol. IX of a series of monographs on applied chemistry edited by E. HOWARD TRIPP, Ph.D. [Pp. x + 420, with 31 figures.] (London : Chapman & Hall, Ltd., 1935. 25s. net.)

ALTHOUGH some reference to electrolytic oxidation and reduction processes is always made in text-books of electrochemistry, no detailed account of the whole field seems to have appeared since the publication of the third edition of Foerster's *Elektrochemie wässeriger Lösungen* in 1922. Much experimental work on such processes has been done since then and some revision of theoretical views has taken place, but few people will have realised that the extent of these advances already calls for a separate book on the subject. Perusal of the present volume not only shows that this is undoubtedly the case, but demonstrates the important relation of these electrolytic processes to all branches of chemistry. Electrolytic methods for the preparation of salts of metals in unusual valency states, of a number of complex salts, and of various per-salts, are likely to be of increasing value in industry as well as in the laboratory, and the same is true of the wide range of electrolytic processes for the oxidation or reduction of organic substances and for effecting halogenation and other substitutions. Recent studies of the influence of

various factors such as current density, temperature, agitation, nature of electrode, and composition of electrolyte, on current efficiencies have enabled the yields of the various products to be more precisely controlled and, in many cases, have thrown new light on the mechanism of the processes concerned. Specific catalytic influences have still to be postulated in order to " explain " certain effects, but a number of such difficulties have been cleared up by the discovery of evidence for the existence of definite intermediate stages in processes formerly believed to be simple. In view of the important contributions which the authors themselves have made to the solution of these problems, it is particularly fitting that they should have undertaken this detailed account and critical discussion of the whole field. As in all books with which Dr. Glasstone has been associated, the presentation is clear, the arrangement of the subject matter logical, the discussion well balanced, and the references to original literature (including patents) remarkably complete. The introductory chapters dealing with reversible electrode potentials, polarisation and overvoltage, and diffusion phenomena in electrolysis afford a thoroughly up-to-date survey of these fundamental questions sufficient for a proper understanding of the mechanism and characteristics of the processes afterwards discussed. Reference to the production of alkali and chlorine, hypochlorite and chlorate is deliberately omitted since these operations form a separate subject which has been extensively dealt with elsewhere. On the other hand, mention is made of the few cases of electrolytic oxidation and reduction in molten electrolytes and non-aqueous solutions. The book is well supplied with tables of data and with graphs, especially current density—potential curves, which summarise and illustrate the chief topics of discussion : a subject index is appended. The work can be confidently recommended not only as a comprehensive account of a somewhat neglected part of electrochemistry, but also as a valuable and highly stimulating correlation of a wide range of topics which will be of interest to research workers in all branches of chemistry and to those concerned with the application of electrolytic methods to chemical industry.

<div align="right">H. J. T. E.</div>

Flame. By O. C. DE C. ELLIS, M.Sc., Ph.D., F.I.C., F.R.P.S., A.M.I.Min.E., and W. A. KIRKBY, M.Sc., Ph.D., F.I.C. Methuen's Monographs on Chemical Subjects. [Pp. vi + 106, with 1 plate and 14 figures.] (London : Methuen & Co., Ltd., 1936. 3s. net.)

THIS useful monograph covers the subject well, though the literature surveyed is almost too extensive for condensation into such short compass. The authors are to be commended in treating most fully of those matters of which they have had first-hand experience ; but it must for all that have been difficult to compile a list of 160 " flame and combustion " references in which Bone receives only one mention and that in connection with a point of relatively minor interest.

This book will be useful to those with a working acquaintance of the subject, but the student will find some difficult reading, for example, on p. 37 ; at times a breakaway from the cold matter-of-fact style in which most scientific books are now written is refreshing, but it can be overdone.

Two paragraphs, one on p. 51 dealing with chain reactions and another on p. 58 discussing the relation of "surface" to ignition, are not lucid. With regard to the latter the impression is conveyed that the presence of a " cataly-

tic " surface must of necessity encourage ignition, whereas not infrequently the reverse is true, the surface tending to monopolise the oxidation.

I find, on p. 20, one small misstatement of fact : " Dugald Clerk (1886) was the first to obtain a record of the development of pressure during the passage of flame through an explosive mixture." Mallard and Le Chatelier no doubt earned a short priority.

<div align="right">G. INGLE FINCH.</div>

An Introduction to Physical Metallurgy. By LELAND RUSSELL VAN WERT, S.D. [Pp. x + 272, with frontispiece and 177 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1936. 18s. net.)

THE subject of metallography has made such remarkable progress in recent years that its text-books rapidly become out-of-date. The present work comes from the school built up at Harvard by Prof. Sauveur, who has lately retired from its direction, after doing much to advance the study in America, and to encourage originality amongst his students, many of whom have since made their mark. The author attempts to cover the whole of the properties of metals and alloys in a general way, discussing at each stage the relations between those properties and the constitution as determined by metallographic methods, but the methods themselves are not described, and except for a rather indistinct photograph of an X-ray tube and camera there is no account of apparatus. It is therefore the results of metallographic investigations with which the book deals, and in this the conventional lines are followed in the main, but opportunity is taken to introduce modern conceptions at each stage. Thus a general account of the metallic state is followed by a description of the various types of equilibrium diagram, illustrated by only two concrete systems. The review of the micrographic constituents of alloys, including photographs drawn from various sources, deals incidentally with questions of internal constitution, as expressed, for instance, by the Hume-Rothery rules. The effects of mechanical and thermal treatment on micro-structure occupy a separate section. Physical and mechanical properties are treated briefly, with examples drawn from alloy systems of technical importance. The treatment is interesting, and the work of physicists is used to throw light on metallographic problems. There are very few references to literature, and although the names of investigators are sometimes mentioned in the text, they are not always correctly given. (Thompson for Thomson, Ewing for Ewen, Houghton for Haughton, and Schreinmakers for Schreinemakers, are among those noted.) The term " metallicity " is ugly, and it is to be hoped that it will not be adopted. The author has, however, done his work well on the whole, and his readable style will make the book popular with students. A recently discovered unconventional photograph of Sorby, the undoubted founder of metallography, forms an interesting frontispiece.

<div align="right">C. H. D.</div>

Annual Review of Biochemistry. Vol. V. Edited by JAMES MURRAY LUCK. [Pp. ix + 640.] (Stanford University P.O., California: Annual Review of Biochemistry, Ltd., 1936. $5.00.)

THESE Annual Reviews of Biochemistry (this is the fifth of the series) have already won for themselves a high place in the esteem of biochemists and

also of many others whose interests extend into this wide and rapidly developing subject. Some of the pages are, of course, of interest only to the specialist; others are of much more general appeal, especially those which deal with subjects in which advance has been rapid and dramatic, such as the sex hormones and the vitamins. It is interesting to note that three of the twenty-six articles are devoted to the various aspects of the metabolism of bacteria and fungi, whilst another three deal with special branches of the biochemistry of plants. An article on Clinical Applications of Biochemistry is mainly concerned with certain abnormalities of calcium metabolism and the resulting bone lesions. The practice of leaving any one subject in the hands of one author for only two years ensures that the series of Reviews as a whole will give an all-round well-balanced reflection of current biochemical research. The present volume continues the tradition established by its predecessors of drawing its contributors from many nations. Perhaps the value of these volumes, especially as works of reference, would be still further increased if they were provided with a subject index. Its preparation would doubtless be a considerable task, but its value would increase as the series of reviews lengthens.

<div style="text-align: right">W. O. K.</div>

Essentials of Physiological Chemistry. By ARTHUR K. ANDERSON, Ph.D. [Pp. iv + 257, with 31 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. 13s. 6d. net.)

THIS relatively short text-book of biochemistry, which was designed primarily for students of dietetics and human nutrition, commences with a summary of those sections of physical and organic chemistry of special importance to the biochemist. This summary, which occupies approximately 100 pages, will doubtless prove of great value to those students who would otherwise find it difficult to obtain an equally concise and readable account of this very essential knowledge, and few will question the judgement of the author in emphasising so strongly the importance of a proper understanding of the principles and major facts of physical and organic chemistry. A feature which strikes the reader is the direct, downright, one might even say dogmatic, style. The author himself admits that he may have " erred in leaving the student with the impression that the explanations presented are final," when, in reality, the subject is still in a state of flux. The problem of dogmatism in text-books is undoubtedly one about which there is room for discussion. It is especially difficult when, as in the present instance, the text-book is one addressed to relatively elementary students. By sacrificing a certain judicial impartiality, the author has succeeded in presenting a very readable and easily intelligible account of his subject. In a book of this kind it is inevitable that points of detail will be found which appear open to criticism; for example, in the chapter on vitamins, there seems to be no mention of lactoflavin. In the index, no reference can be found to œstrin or œstrone and these terms are not apparently mentioned in the text. It would seem desirable to mention that these are other names for theelin, the ovarian hormone. In spite of these and a few other points of detail open to criticism the book will undoubtedly prove useful to a large and increasing number of students to whom the simple and clear method of presentation will make a strong appeal.

<div style="text-align: right">W. O. K.</div>

The Chemistry of Milk. By W. L. Davies, Ph.D., D.Sc., F.I.C. Vol. X of a series of monographs on applied chemistry edited by E. Howard Tripp, Ph.D. [Pp. xii + 522, with 26 figures.] (London : Chapman & Hall, Ltd., 1936. 25s. net.)

This book is stated to be " an attempt to gather together in a concise, ordered form, the results of all the relevant and reliable investigation on the chemistry of milk." It is divided into five sections, which deal respectively with the composition, the constituents, the physical chemistry and the nutritive value of milk and the chemistry of milk processing. The author has undoubtedly collected together and set out a vast amount of information on his subject and has added very materially to the value of the monograph by giving a bibliography at the end of each chapter. In all, some 1,400 references are cited in the text.

The value of the book appears to lie in its use as a work of reference for the research worker or for those engaged in the industry rather than as having an appeal to students of dairy chemistry. In places it gives the impression of being somewhat of a mixture of the elementary and of the very advanced chemistry of the subject. For the sake of the student one could wish that it had been written in a somewhat more flowing and readable style. The text is admirably illustrated by tables of data, and by graphs cited from the literature. In some cases the author has hardly made sufficiently clear in the text the meaning of some of these illustrations. The chapters dealing with such subjects as the factors influencing the composition of the milk, the causes of faults in milk, e.g. oiliness, the development of rancidity and tallowiness in butter, and the action of milk on metals, are well set out and fully discussed both from the theoretical and practical points of view.

The book is remarkably free from printer's or other errors and is supplied with satisfactory author and subject indexes.

W. G.

Perfumes, Cosmetics and Soaps, with especial reference to Synthetics. By William A. Poucher, Ph.C. **Vol. I : A Dictionary of Raw Materials together with an Account of the Nomenclature of Synthetics.** Fourth edition. [Pp. x + 439, with 36 plates and 3 figures.] (London : Chapman & Hall, Ltd., 1936. 25s. net.)

As may be seen from the sub-title, this book deals with the raw materials of the perfumery industry arranged in alphabetical order ; the amount of space allotted to the description of the different materials varies from a few lines to a few pages ; the information provided is of course not detailed but may be said to include most of the data necessary for characterising the substance in question and giving its source of origin. Judging by the fact that the book and its companion volume have both run into a fourth edition, it may be concluded that it supplied a need to those engaged with the perfumery and allied trades, for whom it is primarily intended. The general reader, however, who requires enlightenment on the materials used for the production of scents and cosmetics will likewise find in this book much useful information in easily accessible form. It is claimed for the present edition that it contains quite a number more of volatile oils and synthetic products than the previous one.

P. H.

GEOLOGY

Petrology for Students : An Introduction to the Study of Rocks under the Microscope. By ALFRED HARKER, M.A., LL.D., F.R.S., F.G.S. Seventh edition. [Pp. viii + 300, with 100 figures.] (Cambridge : at the University Press, 1935. 8s. 6d. net.)

THIS standard work, with traditions which have inspired students of its subject for some forty years, now reaches its seventh edition. The new text is substantially the previous (1923) edition—revised to incorporate references (about 25 in all) selected from the copious literature published during the past twelve years.

Though the interval spanned by the last three editions (1919–35) has been in a real sense a period of petrological and mineralogical renaissance, the new text adheres to its original purpose and treatment. It describes, without debating, leading rock-types, as exemplified by standard occurrences —preferably British, but supported, where necessary, by Colonial and American examples. It stands apart from disputation and hypotheses : its index-pages make no mention of magmatic (or other) differentiation, the reaction principle in petrogenesis, the stoping hypothesis, pyrometasomatism, hybrid igneous rocks, contamination, myrmekite and several other terms well burnished by recent usage.

But this conservative treatment is neither aloof nor rigid. Innovations are recognised, without exploitation, in so far as they rest on sound observational fact. Contamination, for example, is admitted (pp. 30, 36, 41, 56); so also is magmatic reaction as a mechanism (p. 76), though rather more emphasis is laid on reactions which occur between solid phases (pp. 76, 290). But there are no excursions into genetic hypotheses. Inevitably, the student is unconsciously led to adopt an implied hypothesis—that the igneous-rock types described are essentially of pure magmatic origin. Still, the student whose purpose is solely to familiarise himself with these types has little need to dabble in theories. This consideration doubtless weighed with the author, who states, in the preface, that the work is addressed especially to *geological* students who do not propose to specialise in Petrology.

A. BRAMMALL.

Road Aggregates : Their Uses and Testing. By BERNARD H. KNIGHT, D.Sc., Ph.D., Assoc.M.Inst.C.E. The Roadmakers' Library, Vol. III. [Pp. x + 264, with 112 figures.] (London : Edward Arnold & Co., 1935. 21s. net.)

IN common with other Highway Engineers, the author deplores the frequency with which the choice of road stones and aggregate has been swayed by the specious claims of " the lowest tender " rather than by considerations of true economy. The latter is best assured by stipulations regarding " quality " —that combination of physical properties best suited to the specific purposes in view.

It is but reasonable that preference should be given to materials known to have behaved well under service conditions. But this test of merit imposes a serious handicap on stones newly put on the market and therefore without service credentials. Yet the merits of an untried stone—relative to those of its seniors in service—can be assessed by well-conceived test-work of undoubted validity.

The scientific principles of routine tests are fully explained in Chaps. I–VIII of this admirable treatise. Appliances used for the various tests (crushing and impact, abrasion and attrition, etc.) are described in Chaps. V–VI and Appendix II. Published data for standard British (and some foreign) stones are critically surveyed in Chap. VII. By appeal to microscopy and geological principles, Chap. VIII shows how the service behaviour of a stone depends on the texture or fabric, the geological history, and the mineral (rather than the bulk) composition of the stone. The last eight chapters deal with the nature and uses of raw materials, codes of practice in the standardisation of aggregate, and various matters of engineering policy. Supplementary data appear in a concluding chapter (on silicosis) and three appendices.

The author's standing as both engineer and scientist is an assurance that the technical information assembled and co-ordinated in this volume is of the highest practical value to road engineers, while the petrologist will appreciate the application of microscopy to the concrete problems peculiar to this branch of applied science.

Lucid in style and presentation, well illustrated and accurately indexed, this volume is a valuable addition to " The Roadmakers' Library."

A. BRAMMALL.

Invertebrate Palæontology. By W. H. TWENHOFEL and R. R. SHROCK. [Pp. xvi + 511, with 175 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1935. 30s. net.)

THIS book is intended for beginners in the study of Palæontology. As such it has the initial advantage of being attractively produced. The text is well arranged and has sections dealing with structure, classification, range, history and ecology. It is illustrated with an abundance of figures of all kinds from simple line diagrams to carefully reproduced photographs. A number of three-dimensional anatomical diagrams provide an interesting and useful innovation. Taken as a whole the book is well conceived.

The practice of opening the study of a phylum, or of a smaller division, with detailed descriptions of representative examples, is dropped in favour of a general account of the structural features of the whole group, and of its subdivisions. In this the anatomy of living forms rightly plays an important part.

The authors hold the view that students must ultimately acquire familiarity with technical terminology, and therefore might as well do this first as last. The text is accordingly heavily sprinkled with terms and synonyms. They seem also to hold a similar view concerning classification and full lists of families and orders are usually given. Though such lists may have a value even at this early stage, it does not seem quite right to find in a book for beginners in Palæontology a classification of Insects complete with 44 orders and notes on each.

In other sections of the text the authors become less severe. They drop into lighter vein and introduce much matter of popular interest. Indeed, at times they seem to go to the other extreme and forget that they are writing a text-book of Palæontology. Many of the paragraphs on the Insecta seem to be irrelevant.

In a book which covers so wide a field mistakes seem to be inevitable. Among the illustrations in Fig. 29, Q is wrong, and in D the arrows for theca

and epitheca point to the same thing; in Fig. 143, the original of C has obviously been misunderstood, and in G the words ventral and dorsal are reversed in position. In the text it comes as a surprise to be told that until sixty years ago the Crinoidea were believed to be extinct, that two such totally different things as the caudal spines of Dalmanites and Olenellus are to be spoken of as the telson, and that the hyponomic funnel of the Cephalopoda is formed from the mantle.

The philosophical side of the subject receives scanty treatment in the form of several paragraphs which deal very crudely with the principle of recapitulation. To say that Spirula has " a planospiral, chambered shell coiled in a gyroconical manner " seems like a precise technical description; but such skill in the use of terminology does not atone for looseness in thought and expression—insects do not really " modify their bodies " for flying.

H. H. S.

Tertiary Faunas : A Text Book for Oilfield Palæontologists and Students of Geology. Vol. I : The Composition of Tertiary Faunas. By A. MORLEY DAVIES, D.Sc. [Pp. xii + 406, with 565 figures.] (London : Thomas Murby & Co., 1935. 22s. 6d. net.)

VOLUME II of Dr. Morley Davies' *Tertiary Faunas* was published in 1934 and reviewed in SCIENCE PROGRESS, Vol. XXIX, p. 760, 1935.

The present volume, which completes the work, deals with the structure and the diagnostic characters of some of the animals which compose the Tertiary faunas.

The Tertiary Foraminifera, Echinoidea, Lamellibranchia, Gastropoda, form the subject of lengthy chapters, and the Vertebrates a shorter section, the Scaphopods, Cephalopods, Brachiopods, Bryozoa and Anthozoa receive less extensive treatment.

Each chapter begins with an introduction in which the structure of the skeleton is described and the technical terms used in such descriptions explained. Other characters of the animals, their ontogeny and habits for example, are mentioned when they must be taken into account in the interpretations of fossils. The introduction then discusses the basis on which the classification of the group rests, and gives when necessary a comparison of different classificatory schemes which are in use. The bulk of each chapter consists of a systematic treatment of the group, so far as it is represented in Tertiary rocks. The characters of each family are listed and those of each genus are presented in a short but clear form. Finally the range of the genus in space and time is recorded.

Many especially interesting or important animals receive special treatment, their evolution being described. The accounts of the history of Nummulites, Clypeasteroides, Anomiids, and of Dibranchiate Cephalopods, will be found of interest by many whose interest in them is that of a zoologist.

The short section on Vertebrates is perhaps too condensed, but gives in a useful way the ranges of all the Mammalian sub-orders.

Each section of the book has its own short bibliography of works of reference and recent papers of value for their discussion of classification and similar points. And each has a glossary of technical terms.

A special feature of the book is the abundance of illustrations. These are all admirable line drawings, 565 in number, grouped into 71 whole-page plates, printed as part of the text, each faced by its own explanation.

The whole work is remarkable for its comprehensiveness (some 1,600 groups are dealt with), the precision of its statement, and its logical and orderly arrangement. How great an achievement it is can only be understood by one who has attempted to draw up short and intelligible diagnoses of groups of animals.

D. M. S. WATSON.

Geology and Ground-Water Resources of the Island of Oahu, Hawaii. By H. T. STEARNS and K. N. VAKSVIK. [Pp. xx + 479, with 33 plates and 34 figures.] (Territory of Hawaii, Division of Hydrography, Bull. 1, 1935. Price not stated.)

OF recent years the islands of Hawaii have leapt into importance because of their strategic value to the United States, but for nearly a century they have been one of the classical centres for the science of volcanology. This elaborate study of the geology and water resources of the island of Oahu, on which Honolulu, the capital of the Territory of Hawaii, is situated, by the geologist H. T. Stearns and the water engineer K. N. Vaksvik, is clearly of considerable importance both from the political and scientific points of view.

Part I, dealing with the geography and geology of the island, occupies 198 pages, and perforce treats mainly of volcanic rocks and especially with basaltic lavas. Oahu appears to be built of the products of two great shield volcanoes, Waianae and Koolau, of which Koolau was the later to become extinct. The Waianae Range is made up of three groups of lavas, the Koolau of two, both series having been erupted in the Tertiary with possibly an extension into the early Pleistocene. The erosional processes in this basaltic country and the resulting topographical forms are dealt with in a very interesting manner, and the account is well worth reading by students of geomorphology. After the deep Pleistocene erosion the island was submerged more than 1200 feet and the major valleys were drowned and alluviated. There are several strand lines up to 100 feet above present sea-level, which are due to more recent changes of oceanic level. During this period spasmodic eruptions producing many tuff cones and lava flows, mainly of nepheline-basalt, took place.

Part II, dealing with water resources, occupies the remainder of the book, 269 pages. The lava rocks of the island are highly permeable, and as the rainfall reaches a maximum of 300 inches per annum they carry immense quantities of underground water. Twelve artesian basins are the principal sources of domestic, municipal and irrigation supplies; and for the period 1928–33 the quantity pumped from these basins was 105,000 million gallons. Heavy pumping, however, causes contamination with sea water in wells near the coast, and further developments will tend to the sinking of deeper wells as far inland as possible. There are also innumerable springs which are sometimes tapped by means of tunnels. Springs provide an estimated amount of 100 million gallons a day.

Numerous minor topics of interest to the geomorphologist, volcanologist and hydrographer are treated in this fine memoir, which is illustrated by many excellent photographs, maps, sections and diagrams.

G. W. T.

BOTANY

Pollen Grains. By R. P. WODEHOUSE, Ph.D. [Pp. xvi + 574, with 14 plates and 123 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1935. 36s. net.)

DR. R. P. WODEHOUSE has performed a useful service by bringing together in a single volume the widely scattered knowledge respecting pollen in such a manner as to reveal the extensive lacunæ which remain to be filled.

Pollen is not merely of interest for its own sake, because the diversity of its form is closely connected with systematic affinity, but pollen has a particular interest to the ecologist and archæologist, owing to its resistance to decay, as a clue to the vegetation changes and climatic conditions of the prehistoric past. This aspect of the subject is treated, all too briefly, by Dr. Erdtman in Chap. IV.

For the student of atmospheric conditions pollen is of interest as an important constituent of dust. Chap. V gives a short account of atmospheric pollen based on the results of collections from the centre of Yonkers City. These data show that, though tree pollen is most abundant, that even of low-growing plants, such as grass and *Ambrosia elatior*, may be present in considerable amount.

To the medical man pollen is important as a cause of hay fever, and this aspect is dealt with in Chap. VI, where the author distinguishes three main periods of its incidence, namely, (i) in the early spring, due chiefly to Amentiferæ; (ii) in the early summer, due mainly to grasses; (iii) in the autumn, due mainly to members of the Compositæ.

There is an historical review of over 80 pages, but the greater part of the text, some 340 pages, is rightly devoted to the taxonomy of pollen grains and their description. This section comprises keys to identification and the descriptions include average dimensions. Although many of the salient types are figured, one would have welcomed a much larger number of illustrations, since the form of pollen grains is not readily conveyed by words alone.

In the author's general treatment of pollen-grain structure the influence of hereditary characters and of the internal and external environment are recognised, but by citing in illustration of the last the differences in pollen form of the insect-pollinated *Tanacetum* and the wind-pollinated *Artemesia*, the author appears to indulge in a Lamarckian interpretation for which there seems little, if any, justification.

The volume, which is produced in the admirable manner which we associate with the publishers, will be an essential reference book to students of this subject.

E. J. S.

Vergleichende Morphologie der höheren Pflanzen. Vol. I: Vegetationsorgane. Part I. By Dr. WILHELM TROLL. [Pp. viii + 171, with 104 figures.] (Berlin: Gebrüder Borntraeger, 1935. RM. 11.50.)

MORPHOLOGICAL expressions are the resultant of the interaction of the inherited potentialities of the organism and of the environment. The latter may inhibit, diminish or develop the manifestation of these potentialities. The familiar cockscomb so dear to the hearts of Victorian gardeners was a

cauliflower-like fasciated strain of Celosia, but special culture and especially rich nutrition were essential to the full development of its monstrous magnificence. Bergdolt's experiments with *Viola pinnata*, here quoted, showed that good culture not only increased the proportion of dichasial inflorescences, but resulted in the production of an appreciable number of tetramerous fruits, a potentiality not evinced by the unmanured plant.

Whilst this duality of morphological expression is theoretically recognised, in practice the implications are too little apprehended. It is perhaps for this reason that so little experimental morphological investigation has been carried out. Rightly envisaged, morphology is not mere description of static phenomena but a dynamic expression of their interacting causes. Teratological study rightly finds its place in such a method of approach as presenting a further expression, albeit unusual, of latent potentialities released by an abnormal environment or by pathological conditions. Admittedly their interpretation by the student of comparative morphology is beset with difficulty, but to regard them as of little or no significance is to neglect an important clue to the interpretation of morphogenetic response.

The elongation of the normally undeveloped internodes below the daughter bulb of Galanthus when the parent bulb is planted at an excessive depth raises the bulb to its normal level in the soil. An excellent example of this is shown in Fig. 73. The interest of such an abnormality is the more evident from its patent utility to the plant; many departures from the modal condition have, however, no obvious value, but one must not therefore infer that they have no physiological significance or that they are morphologically unimportant.

The present work, which will be completed in about three years, is a welcome addition to botanical texts, since it is some years since a general work on morphology has appeared. It is, however, unfortunate that the author is somewhat didactic in his treatment where a balanced presentation of divergent views would have been welcome. The present section concerns the morphology of the shoot as exemplified by a number of types which are described and illustrated by photographic and diagrammatic figures.

E. J. S.

Protoplasm. By W. SEIFRIZ, Ph.D. [Pp. viii + 584, with 179 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1936. 36s. net.)

IT is a distinct advantage that this book has been written by a botanist, not by a man whose speciality is mainly chemistry or colloid science. On reading it one really seems to see protoplasm and to get in touch with it ; it does not lead a rather precarious and shadowy existence as in some other books. A great number of excellent, well-chosen pictures enhance this strong point of the book.

On the other hand Seifriz, does full justice to all attempts to apply physical and chemical methods to the problems of protoplasm. He brings out very clearly some of its most enigmatical properties : at first sight it appears amorphous and irregular, but actually it possesses both a subtle and supple intricate organisation and structure. Then we are never and nowhere dealing with an equilibrium, but always with a stationary state. Protoplasm is, so to say, always on the tilt : it may readily liquefy or solidify, become peptised or be coagulated, membranes may turn strongly permeable

or impermeable, etc. In recent years the discovery of such phenomena as the behaviour and reactions of mono-molecular films, the orientation of anisotropic molecules and micelles, phase reversal in emulsions, thixotropy, has enabled us to visualise how structures and processes of this kind are possible at all.

H. F.

Plant Viruses. By K. M. Smith, D.Sc., Ph.D. Methuen's Monographs on Biological Subjects. [Pp. x + 107, with 11 figures.] (London: Methuen & Co., Ltd., 1935. 3s. 6d. net.)

It is impossible, of course, to cover in the space available all the aspects of virus in plants which deserve mention ; but within its limits this is an excellent selection from the ascertained facts. It suffers, perhaps, from an uncertainty in the author's mind as to the audience he is aiming at, e.g., the chapter on general technique seems out of harmony with the rest of the work and the space it occupies might have been better utilised. We would have preferred, too, more digestion of the data by the author and less mere setting down side by side of the discrepant evidence supplied by different workers. There are other signs of somewhat hasty compilation, which will, no doubt, disappear in a second edition.

Besides the introduction, there are nine chapters, dealing respectively with technique, transmission in nature, virus in the host, outside the host and in the vector, immunity, nature of virus, control, and some comparisons between the plant and animal viruses. The last is of especial interest to the plant worker, who has neither the time nor the opportunity to keep himself abreast of the important work that is being done on the animal side.

J. Henderson Smith.

Tropical Planting and Gardening. By H. F. Macmillan. Fourth edition. [Pp. x + 560, with 4 plates and numerous text figures.] (London: Macmillan & Co., Ltd., 1935. 25s. net.)

This new edition, of what is now a standard work of reference, will be appreciated not only by those actually engaged in tropical agriculture and horticulture, but also by those indirectly interested therein. Although written with especial reference to Ceylon, it has a much wider appeal, containing a wealth of information regarding plants cultivated in the tropics either for commerce or for ornament. The text is illustrated with over 360 photographic reproductions, mostly of growing specimens of the plants described. Following a section dealing with more general topics of culture, the main part of the text is occupied with notes on the individual species, classified under various headings according to their uses. A good index ensures that the sectional treatment is in no way disadvantageous.

The final section includes miscellaneous interesting data on a variety of subjects, such as seed weights, recipes, insect and fungal pests, perfumed plants, etc. The whole constitutes a work that can be commended both to those abroad and to those at home.

E. J. S.

Forest Bibliography to 31st December, 1933, Part I. Compiled and Published by the Department of Forestry, University of Oxford. [Pp. xviii + 78.] (Oxford : Hall the Printer, Ltd., 1936.)

THE Department of Forestry of the University of Oxford, under the able guidance of Professor Troup, has been steadily accumulating an excellent library of literature bearing on forestry. For facility in use the contents were classified by subjects and the system adopted and reproduced here worked well. In response to requests from various sources it was decided to publish the list of contents, and Part I has now appeared containing the list of publications, down to the end of 1933, on General Forestry and two subdivisions of Silviculture, *viz.* General Silviculture and Seeds and Seedlings.

The Bibliography " comprises a fairly complete list of papers, bulletins and other matter published in English and a considerable amount of literature published in French and German. Few publications in other languages are represented unless they contain summaries in English, French or German." Some 750 periodicals, bulletins, etc., are listed and the list of papers referenced occupies seventy-four pages. The references give the subject of the paper, the author's name, the date of the periodical and the volume and page where the article is to be found.

As to its usefulness, there can be no doubt. The need has long been felt of a work like this, not only to enable one to find out what papers there are on a given subject, but to know that the particular works cited may be consulted at Oxford. If the work falls short of the ideal in that it is a list of publications in the library of the Department of Forestry at Oxford and not a complete bibliography, we at any rate may rest assured that the more important works are included. Publishers and compilers alike are to be congratulated on its appearance, for it will find ready acceptance among all interested in forestry.

A. S. W.

ZOOLOGY

The Variation of Animals in Nature : A Critical Summary and Judgment of Evolutionary Theories. By G. C. ROBSON, M.A., and O. W. RICHARDS, M.A., D.Sc. [Pp. xvi + 425, with 2 coloured plates and 30 figures.] (London, New York, Toronto : Longmans, Green & Co., 1936. 21*s.* net.)

THIS is a most valuable book. The authors, unlike many writers on evolution and allied subjects, really know what they are talking about. Mr. Robson is keeper of Mollusca in our National Museum, and scientists holding such appointments see literally a thousand species where the average teacher of zoology sees only five or six. In the group Mollusca, for instance, in Zoological Practical work, at most six or eight types are dissected. Dr. Richards is that most valuable type of zoologist, a field naturalist. He has devoted a large amount of time and infinite patience to the study of the life of insects as it actually occurs in the open. The book which they have written will prove a storehouse of information to all interested in evolution, which is the root problem of biology, and especially to those who endeavour to carry further the investigations described in the book. If we may make a few friendly criticisms it is in the hope that when the work goes into a second edition the authors may be enabled to clarify and simplify their work.

In dealing with evolution, which means the gradual growth of one species into another, there are several preliminary questions which ought to be answered. Broadly speaking, animals and plants are propagated by unicellular germs and the type of species is kept approximately constant by the *powers of growth* inherent in the germ. Unless these powers change, no change in the type of adult can take place. Now the theory of " natural selection " is built on the assumption that inheritable changes in these powers of growth *in all directions* are constantly taking place. Is this really so ? The question, put to the test of experiment, has yielded a negative answer. Fluctuating inheritable variations do not occur. Therefore the whole basis of classical Darwinism is destroyed and natural selection, whilst it is effective as a cleanser of species from crippled and weak individuals, may be definitely ruled out as an explanation of evolution.

But sports—*i.e.*, sudden deviations from type—do occasionally occur, as every farmer and gardener knows, and these can be inherited. Their modern name is " mutations " and their supposed invisible causes are " genes." The abuse of this word fully justifies the bitter regret of its author, Johannsen, that he had ever coined it. Can sports furnish the material for evolution ? The answer again is decidedly no. All sports, as Mohr has shown, in dealing with the overworked Drosophsia, are inferior in vigour and viability to the original type, and if liberated in the wild would either be wiped out in competition with the type, or else if transported to some paradise like New Zealand where the type is absent would revert to the wild type—as the pigs transported there by Captain Cook in 1790 have gone back to the wild boar. Johannsen indeed characterised " genes " as " superficial " disturbances of the chromosomes.

What is left over ? The problem of evolution as it meets us in Nature is really the problem of adaptation. Now all animals and plants exposed to varying conditions are capable of a certain amount of adaptive change during their individual lives. Can they hand over any portion of this gain to posterity ? This we regard as the root-problem of all biology, and, it may be added, of sociology also.

The authors admit that the experiments of McDougall on the inheritability of acquired habits in rats furnish a positive answer. If they will look further they will find that other experiments, especially those on the feeding habits of insects, furnish similar answers and are in every way confirmatory of those of McDougall. But if this is true in the case of rats and insects it must be true in all cases, and it is *the* explanation of evolution. It clarifies recapitulation in embryology and lineage series in palæontology, for the essential framework of classification in both animals and plants, with its array of local varieties, species, genera and families, is so similar as to lead to the inevitable conclusion that one general cause must be the base of it in all groups of living beings. How the change in the " soma " of the adult is transferred to the powers of growth of the germ of the next generation, is of course the difficulty over which Weismann stumbled, but this difficulty is gradually being " liquidated " by the science of experimental embryology.

E. W. MacBride.

Sea-Trout of the Montrose District. Part II : Growth on the Scales of Recaptured Fish. Part III : The Migrations of Sea-Trout. By G. HERBERT NALL, M.A., F.R.M.S. Fishery Board for Scotland, Salmon Fisheries, 1935, Nos. II and III. [Part II : pp. 22, with 15 plates. Part III : pp. 24, with 1 map and 5 tables.] (Edinburgh : H.M. Stationery Office, 1935. 2s. net each part.)

PART II deals specially with the difficulties which arise in the reading of Sea Trout scales, and there are a number of very instructive photographs showing scales from marked fish and of the same fish after recapture at a later date. We have no hesitation in agreeing with the author that in some of these cases, without the knowledge gained by the marking and recapture records, the most experienced scale reader might be misled. This is not to say that the reading of normal scales is liable to much error, but that in some examples false checks, marked growth in winter-time, or the partial or even total disappearance of the Spawning Mark, give pitfalls which the reader of salmon scales is not so likely to encounter.

Part III not only deals with the Montrose area, but gives much information in a useful summary about the results of Sea Trout which has been carried on by the Fishery Board for Scotland for the last fifteen years. The Montrose district does not differ from others in showing that while the great majority of recaptures are made either in the same river or within some five miles or so of its mouth, there are other recaptures made over a wide range. Previous records have revealed fish wandering across the North Sea, or to Ireland. The furthest recorded from the Montrose district is 300 miles south to a point about 190 miles east of Hartlepool. Other Montrose fish turned up in the Tay, the Earn as far as Crieff, the Forth, and the Northumberland coast, or in a northerly direction as far as Beauly in Inverness-shire. Mr. Nall considers that the catching power in our rivers, as compared with the absence of very effective catching in the sea, gives an unreal appearance of river and estuary concentration. He regards Sea Trout as always gregarious and as moving widely in search of their food, while the evidence also is that, like the Salmon, they return to their native rivers to spawn. Scale readers will find these two papers of distinct value.

W. L. C.

Scale-Absorption in Salmon and Sea-Trout. By M. IAN CRICHTON, B.Sc. Fishery Board for Scotland, Salmon Fisheries, 1935, No. IV. [Pp. 8, with 4 plates.] (Edinburgh : H.M. Stationery Office, 1935. 1s. net.)

THIS investigation is welcome, as the subject of scale-absorption has not previously been described and its value in scale-reading is obvious. There is great difficulty in sectioning adequately hard hyalodentine embedded in soft surrounding tissue, and if a method could be discovered of reducing the hard material to a consistency approximating to that of the dermis, it is probable that great advantage would result to this class of investigation. In Mr. Crichton's paper, the absorption first of the hyalodentine and next of the fibrous layer is well brought out.

It is noticeable that the extreme of scale-absorption is in the male kelt and that it is accompanied by the extreme of dermis-thickening. The female condition seems to present a contrasting appearance, and this has not been

noticed in the paper. If absorption is a process accompanying the cessation of feeding and the development of the genitalia, it may be common to all the tissues other than the gonads, and it is possible that the change in the scale material may serve the metabolism of deeper layers even after the genital products have been expelled. Changes in calcareous tissues have been noticed to some extent in a paper about otoliths by Fulton (Fishery Board Reports). It is to be hoped that Mr. Crichton may be able to continue this interesting study.

<div align="right">W. L. C.</div>

Sea Trout and Trout. By W. J. M. MENZIES, F.R.S.E. With a Fore-
- word by G. HERBERT NALL, M.A., F.R.M.S. [Pp. 230, with 16 plates
and 3 figures.] (London : Edward Arnold & Co., 1936. 10s. 6d. net.)

ALTHOUGH this book is written for the " very light reader " and addressed primarily to the angler, it is well worth the attention of readers generally who wish to keep in touch with modern fishery research, for, in avoiding technical language, Mr. Menzies has sacrificed nothing of the scientific approach and the book is accurate in its facts and logical in its deductions. After a foreword by Mr. Nall, to whom so much of our knowledge of the sea trout is due, the book opens with an account of the distribution and historical development of the migratory (sea) and non-migratory (brown) races of trout (*Salmo trutta*). The author maintains that these races are quite distinct and that there are no intermediate stages existing at present, though he does not tell us how interbreeding between the two is avoided, or, if it occurs (as it probably does), what is the result.

After a brief account of the brown trout the life history of the sea trout is described in considerable detail. The story is more complicated than that of the salmon and is not nearly so well known, for it is only in the last fifteen years or so that it has been worked out thoroughly. The problem of local races is also dealt with ; this is a most extraordinary question, for not only does it appear that each river has its own race, but there are quite unpredictable differences between the trout of neighbouring rivers.

There is in this section of the book a good account of the methods of marking and scale-reading which will interest others besides anglers. Other chapters deal with the food, parasites and diseases of the fish, and finally the author applies his knowledge of the ecology of the sea trout to the practical problems of making and improving a sea trout fishery.

There is a brief but adequate bibliography and the plates are excellent.

<div align="right">F. T. K. PENTELOW.</div>

Studies on Insects of Medical Importance in South Africa.
Part III. By BOTHA DE MEILLON, D.Sc., F.R.E.S. Publications
of the South African Institute for Medical Research, No. XXXVIII.
[Pp. 86.] (P.O. Box 1038, Johannesburg, April 1936.)

THE first of these parts appeared in 1934 and, with one exception, all thirteen chapters so far published deal with Diptera. In Part I there appeared the first part of a thesis on South African Simuliidæ, of which the third part appears in the present volume. The second part of a thesis on the Eggs of some South African Anophelines, of which the first part appeared in Part I of this publication, appears in this number, and there are two parts of a paper on South African Ceratopogonidæ.

Of the two other papers, one is on the Systematic Position of *Anopheles listeri* De Meillon, while the other is on the Distribution of *Anopheles gambiœ* and *A. funestus* in Natal and the author discusses the factors concerned.

The volume contains a number of excellent plates from the author's drawings.

<div align="right">FRANK BALFOUR-BROWNE.</div>

Insect Pests of Glasshouse Crops. By HERBERT W. MILES, M.Sc., Ph.D., and MARY MILES, M.Sc. [Pp. 174, with 21 plates and 15 figures.] (Published by H. C. Long, " The Birkins," Orchard Road, Hook, Surbiton, Surrey, 1935. 8*s.* 6*d.* net.)

THE need for an authoritative book dealing with pests of glasshouse crops has long been felt. Glasshouse horticulture has developed enormously of recent years owing to the increased demand of the public for salads and out-of-season vegetables and cut flowers. The older use of glass houses, namely for growing decorative plants that cannot be grown without heat, has not diminished. But still there was no book in English dealing with the pests.

The present volume is divided into nine chapters and a foreword by Mr. J. C. F. Fryer, Director of the Plant Pathological Laboratory, Ministry of Agriculture, Harpenden. He points out how the range of pests differs in many ways from those found out of doors and how glasshouse conditions offer exceptional facilities for their control.

In Chapter 1 glasshouse conditions in relation to the occurrence and control of pests are stated. General soil pests, such as wireworms, fungus gnats, chrysanthemum and lettuce root maggots, root mealy bug, root aphis, millipedes, centipedes and the glasshouse symphylid, are discussed in Chapter 2. The next six chapters deal with caterpillars and leaf miners—aphides, capsids, and leaf-hoppers—white fly, mealy bugs, and scale insects—thrips and spider mites—eelworm pests—woodlice, earwigs, and miscellaneous pests. The last chapter is devoted to the consideration of general methods of pest control in glasshouses. There are two appendices, which give an alphabetical list of the chief glasshouse crops, with the associated pests and their characteristic injury, and a selected bibliography arranged under insect and more general sub-titles. Finally there is a good index.

This book is wholly to be recommended as being full of sound information, lucidly written and illustrated by some excellent plates. The manner of describing the pests and the detailed instructions for control, which in each case follow immediately after, show that practical experience together with scientific knowledge form the basis of this very practical book. One would have liked to have seen a better photograph of the caterpillars of the Gothic moth, and unfortunately the labelling of Figs. 72 and 74 on Plate 18 have become transposed. Some may be sorry to see the advertisements at the beginning and end of the book, but doubtless they have helped to fix the price, which is moderate as scientific books go. The actual format is pleasing and handy. The authors are to be congratulated on so ably filling the gap on the bookshelves of amateur and professional growers, horticultural instructors and county organisers, and more general students and teachers alike.

<div align="right">H. F. B.</div>

Practical Biology for Medical Students. By C. J. WALLIS, M.A. [Pp. xii + 247, with frontispiece and 98 figures.] (London : William Heinemann (Medical Books), Ltd., 1936. 12s. 6d. net.)

THIS new recruit to the vast army of elementary text-books of biology contains little that is new, being chiefly remarkable as a feat of assembly and compression. It covers elementary biochemistry, plant biology and animal biology in a single volume, and consists only of instructions for practical work, without any discussion of the significance of the results observed. The whole scheme " has been drawn up in accordance with the requirements of the syllabuses in Biology of the First Examination for Medical Degrees in the Universities of Cambridge and London and of the Conjoint Board of the Royal College of Physicians and the Royal College of Surgeons," with additional matter for Higher School Certificate Examinations in Biology.

The text is pithy but sound as far as it goes. The illustrations include, besides many old favourites, a number of new sketches which have been so hastily and inaccurately executed that the student will have to be warned against allowing them to influence his style, by teachers who use the book.

C. P. W.

MEDICINE

The Aims and Methods of Medical Science. By JOHN A. RYLE, M.A., M.D., F.R.C.P. [Pp. 44.] (Cambridge : at the University Press, 1935. 2s. net.)

THIS is an inaugural lecture by the newly appointed regius professor of physic in the University of Cambridge. Starting from the generally accepted definition that the aims of medical science are to increase and perfect our knowledge of disease in man and man in disease, Dr. Ryle proceeds to deplore the too great proportion of workers both in practice and in laboratories whose " standard of accuracy " and " use of evidence " are defective and whose judgement is crippled by the difficulties of the subject and the exacting conditions of their lives. Seeking the origin of these errors, he condemns the " cult of specialism," that is, excessive, premature and misdirected specialisms, for the subversive influence which they have had upon medical thought and action and education. Modern medical education lacks a training of reason ; evidence is collected but instruction in its use is neglected. The general physician is drifting to early specialisation, and a similar drift threatens the general physiologist and general pathologist. In spite of their many common interests, the physiologist and the physician are isolated from each other. The appointment of whole-time research physicians is good but intimate co-operation with the other scientific departments will remain essential and not always easy.

With Lewis's " Clinical Science," as defined, the professor is in agreement, but he is at issue with Sir Thomas Lewis when he states that the future advancement of Clinical Science will be more and more in experiment and that the long-tested contributions of observational medicine are no longer likely to prove as profitable. There are necessarily rather strict limits to experiment on sick persons, and the type of disease suitable to that kind of study are few in number. Dr. Ryle believes that by more precise methods of observation, together with the use of new instruments, the knowledge of man's ills can be as usefully served as by the experimental method.

P. J.

582 SCIENCE PROGRESS

Vitamins in Theory and Practice. By LESLIE J. HARRIS, Sc.D., D.Sc. [Pp. xx + 240, with 66 figures.] (Cambridge: at the University Press, 1935. 8s. 6d. net.)

THIS account of vitamins which Dr. Harris has written up from four lectures given by him at the Royal Institution, London, is a suitable and sufficient response to the request of many of his audience for a "book of the words." It is an attempt to express in popular language, often distractingly flippant, the meaning, origin, preparation and uses of the important vitamins as far as they can be generalised to-day. If the book should create an interest and perhaps a desire for serious study of a subject about which the average reader is but ill-informed, it will presumably have attained its object. As with most popular science the "reading without tears" is not so simple as the author pretends, the bunkers are artfully hidden. The science of vitamins is comparatively young, and though after many laborious years much has been overcome and we have gone far, there is yet a long way to go before we should dogmatise in the cheery manner of Dr. Harris. At the end of the book there is a most useful table which summarises the essentials of the preceding pages. Most of the illustrations are good but they have necessitated a disproportionate weight for so small a volume.

P. J.

A Textbook of Pharmacognosy. By J. W. COOPER, Ph.C., and T. C. DENSTON, B.Pharm., Ph.C. Second edition. (Pp. xiii + 522, with 78 figures and 16 maps.) (London: Sir Isaac Pitman & Sons, Ltd., 1935. 18s. net.)

HE who would be an expert pharmacognosist must defy the glib definition of a specialist—one who knows more and more about less and less—and know a little about a lot. His subject matter ranges from the nature of enzyme action to the mechanism of drug sales in Mincing Lane; it includes detailed knowledge of the physical characteristics of many plants and the simple chemistry of their organic constituents. The text-book under review began as a laboratory manual for practical work only, but the second edition includes much fresh material and is designed to meet the requirements of the new Syllabus for the Chemist and Druggist Qualifying Examination. The authors are to be congratulated on their theoretical introductions to the practical work, which disclose the principles underlying the manipulation of crude drugs. The chemical and biochemical principles involved in the extraction, preservation and purification of drugs are simply and concisely displayed, although the chemical purist might occasionally take exception to some small matters, e.g. the use of the word "ether" for an ester (p. 81). The description of natural drugs is admirably systematic and includes such matters as botanical and geographical sources, plant habit, collection, storage, constituents, standardisation, etc. Most of the important plants are illustrated by clear line drawings by Messrs. M. Riley and D. W. Shaw. The book is provided with an excellent index and the publishers have ably seconded the authors in producing a text-book which meets the requirements of the student; it is pleasant to read and easy to use for reference.

H. R. I.

The Integration of the Endocrine System. By SIR WALTER LANGDON-BROWN, M.D., F.R.C.P. (Horsley Memorial Lecture). [Pp. 54.] (Cambridge : at the University Press, 1935. 2s. net.)

IN this graceful tribute to the memory of Victor Horsley the lecturer indicates three lines of recent advance which are leading to a clearer conception of the integration of the complex endocrine system.

The diencephalon, especially the hypothalamus, has been shown to be the nervous structure concerned with the expression of the emotions ; this is illustrated by clinical symptoms in a case of teratoma of the diencephalon, where besides the blotting out of nearly all normal emotional expression there were also symptoms of polyuria, disturbance of temperature, and drowsiness due probably to compression of the pituitary. As might be expected, there is a close association between the diencephalon and the pituitary gland, that small, well-protected "miniature brain" in which lies the "mainspring of primitive existence." Recent work has closely related the diencephalo-pituitary apparatus to metabolic processes, to primary emotions and the sympathetic system. The inhibition of pituitrin through emotional disturbance is shown in cows that refuse to yield milk after the calf has been taken away, an inhibition of chemical stimulus by the diencephalon which can be overcome by injection of pituitrin ; loss of secretion of the anterior pituitary is followed by atrophy of the thyroid and reduction of the metabolic rate which can be prevented by injection of anterior pituitary extract.

The anterior pituitary secretes two types of hormone, "accelerating" from the eosinophil cells and "inhibitory" from the basophil cells. These hormones act on the other endocrine glands, causing them to make secondary hormones, some of which have been isolated in crystalline form and some have been prepared synthetically. The mechanism of hormones is not known, they are regarded as physical catalysts which are only activated when through the circulation they reach the organ of their destination ; they seem to be allied to the chemical mediator of all nervous impulse and tissue cells and may also be interchangeable in action.

The embryological observations on the "organiser" and the chemical entity (hormone) extracted from it emphasises the early connection of development with chemical and nervous mechanism. This chemical substance has now been formed synthetically and is found to be allied to œstrin, thereby linking together the stimulus of growth of the next generation, the growth of bone (vitamin D, ? calciferol) and the disorderly growth of malignant disease (carcinogenic substance in coal tar). A remarkable group of sterols.

We are grateful to Sir Walter Langdon-Brown for publishing this lecture ; it is an intellectual hormone.

P. J.

Food, Health and Income : Report on a Survey of Adequacy of Diet in Relation to Income. By SIR JOHN BOYD ORR, D.S.O., D.Sc. [Pp. 72, with 18 tables, 9 figures and 7 appendices.] (London : Macmillan & Co., Ltd., 1936. 2s. 6d. net.)

THIS is the first survey of the amounts spent on food per head by the people of Great Britain and the adequacy of the diet obtained. The results are certainly bleak and furnish more than an explanation of the high proportion

of C3 adults and the figures for rejection of recruits for the army on the ground of physical defects.   Briefly they are that $4\frac{1}{2}$ million people purchase a diet inadequate in every proximal principle of food, while half the nation take a diet inadequate in one or other constituent.   The work is a challenge to statesmanship, to the science of agriculture and to the economic policy of the country, and deserves widespread publicity.

Incidentally for the strictly scientific reader the appendices are as valuable as the rest of the book, and in view of the hieratic attitude often taken by statisticians to experimental work in the laboratory, Appendix V should be read carefully.   It throws an interesting light on the methods of statisticians.   This is written not with a view to questioning the results obtained, but to call attention to the underlying assumptions upon which statisticians erect such an imposing edifice.

<div style="text-align: right">V. H. M.</div>

Food Values at a Glance.   By V. G. PLIMMER.   [Pp. 94, with 35 charts.]   (London, New York, Toronto : Longmans, Green & Co., 1935.   3s. 6d. net.)

THE purpose of this book is to provide a summary of the general principles of nutrition and to supply the necessary information on the composition of foods by means of coloured charts, on which the nature and amount of each of the principal food constituents present is indicated by a band of characteristic colour.

The aim of these colour charts is to replace, as far as possible, the usual numerical method of giving the composition of a food in terms of percentages by a visual representation of its composition.

The task confronting the author is a difficult one, since there is a large amount of information which it would seem necessary to include if the reader is to be in a position, as is suggested, to plan either for individual meals or to cater on a large scale, and at the same time it is essential, if the book is to fulfil its function and reach those for whom it is intended, that the price should be moderate.

In the attempt which has been made to reconcile these demands the production and the lay-out of the book have unfortunately suffered.

The substitution of colour charts for columns of figures does not eliminate the use of figures entirely and the printing of these in many cases, and particularly of the fractional figures, is by no means clear.   More than a glance is often needed to distinguish between $\frac{1}{4}$ and $\frac{1}{2}$.   The same colours have been selected, no doubt with a view to saving expense, for indicating on some charts or sections of a chart the presence of certain food constituents, e.g. proteins, fat, etc., and on others for indicating foods containing specified vitamins.   Thus yellow is used in some charts to represent fat and in others to indicate that the food is one containing vitamins A and D. This, to the uninitiated, is likely to be a little confusing, and the colour scheme is further complicated by the variation in colour used to represent carbohydrate.   A shade verging on an olive green appears in some charts and this changes to shades of reddish brown in others.

These defects, though not serious in themselves, detract from the claim that the information is obtainable at a glance.   There is much in the book which should be of real value and interest to those for whom it is intended,

and the demand for it should be such that a new edition will be called for before long.

It may be hoped that the author will take this opportunity when it arises to consider some revision and simplification of the text. By a careful pruning of physiological terms and details and the omission of information more likely to be of interest to the expert than to those who have to deal with the daily menu, it should be possible to produce, without increased cost, a clear and attractive book, from which practical information with regard to food values could readily be obtained at a glance.

H. M.

HISTORY OF SCIENCE AND BIOGRAPHY

Osiris : Studies on the History and Philosophy of Science and on the History of Learning and Culture. Vol. I, Jan. 1936 (David Eugene Smith Presentation Volume). Edited by GEORGE SARTON, D.Sc., L.H.D., LL.D. [Pp. 777, with 22 plates, 35 facsimiles and 24 figures.] (Bruges : The Saint Catherine Press, Ltd., 1936. $6.)

THIS is the first volume of a new periodical, which is intended as supplementary to *Isis* ; *Osiris* is to contain the longer articles, each of which will constitute a special number, while *Isis* will continue as heretofore, as a quarterly publication containing shorter articles, reviews, notes and correspondence, with critical bibliographies.

The present volume is really a symposium in honour of the 76th birthday of that great pioneer in the History of Science, Professor David Eugene Smith, and it begins with an exhaustive bibliography of his writings and an account of his mathematical library, by Miss B. M. Frick.

There follow thirty-five articles by various writers, the majority in English ; but the international character of the publication is emphasised by the insertion of a number of articles in French, German, Italian and Spanish.

In the short space available here it is impossible to refer to more than a few of these articles, though all are worth reading. Some contain hitherto unpublished material, for instance, a number of letters of J. J. Sylvester, in a biographical notice by Mr. R. C. Archibald, which occasionally show the great mathematician in an unexpectedly human light and reveal an unknown love-passage of his earlier days, and a letter from Newton to Pepys on a problem of dice-throwing, communicated by M. J. Pelseneer. Mr. A. Pogo gives a description, with reproductions, of three unpublished Egyptian calendars from Asyut. There are two articles on Babylonian mathematics and a very interesting account by Mr. A. N. Singh of the use of series in Hindu Mathematics, which, if the dating is correct, places the use of arithmetic and geometric progressions as far back as the Vedic Age, and incidentally shows that " Gregory's " series for $\tan^{-1}x$ was really discovered by Talakulattura Nambutiri in 1432, nearly two and a half centuries before Gregory.

Of special interest to the English reader is the paper by Francis R. Johnson on " Thomas Digges and the progress of Astronomy in XVIth-Century England." Digges was a contemporary of Tycho Brahe, and, in common with most English mathematicians of the time, used his influence powerfully in favour of the Copernican Theory. He was the first to translate

the *De Revolutionibus* into English and actually improved upon Copernicus by introducing one new (and fundamental) conception, namely, that of the distribution of the fixed stars in depth in an infinite universe, instead of on a spherical boundary or firmament.

Mr. Johnson calls attention to the fact (still apparently unknown to many historians of science) that the tradition which ascribes the invention of the telescope to the children (or apprentices) of the spectacle-maker of Middelburg is a misleading one, and that, long before Lippersheim or Galileo, telescopes, on lines originally suggested by Roger Bacon, had been constructed in England, in particular by the father of Thomas Digges and by Thomas Digges himself and the famous Dr. John Dee.

If the standard set in the present volume can be maintained, those interested in the history of science will find *Osiris* a good investment. The price of this first volume is $6, but we gather that a reduction is made for members of the History of Science Society.

L. N. G. F.

Chemical Discovery and Invention in the Twentieth Century. By SIR WILLIAM A. TILDEN, F.R.S. Revised by DR. S. GLASSTONE. Entirely new (6th) edition. [Pp. xvi + 492, with 100 plates and 28 figures.] (London: George Routledge & Sons, Ltd., 1936. 15s. net.)

THE first edition of this well-known book appeared in 1917, at a time when a large section of the community had very little knowledge of the work of the chemist. Sir William Tilden set out to explain what remarkable beings chemists were, what strange tools they used, and what wonderful discoveries they made. It will be obvious that with the march of time some of the old wonders have become commonplace and have been superseded by newer marvels. The publishers are to be commended, therefore, for securing the services of a modern master in the art of expounding " Advances " in science to bring the book up to date.

Compared with 1917, however, a far greater proportion of the people have a fair knowledge of the work of the chemist, so that Sir William's style is much less appropriate than it was. It does not require much faculty for " higher criticism " to distinguish the sources " T " and " G " in the new volume; Dr. Glasstone assumes a wider knowledge of science than Tilden did and his chronicle is less tinged with wonder. Further, wireless broadcasting has provided a new and probably more effective way of popularising science and explaining its findings to the public in simple language. It seems to the reviewer, therefore, that the present book would have been still better with less of the old Tilden and more of the new Glasstone. An example of the difference between the two will be found in Fig. 38, retained from the first edition. This shows a square with sides of one centimetre in order to explain a cubic-centimetre; on p. 226, however, Glasstone simply, but adequately, defines $\mu\mu$ as the symbol for one-millionth of a millimetre.

Notwithstanding this general criticism, the book can be heartily recommended, first to the intelligent layman, then to the man of science who, specialising in another field, would wish to know what is being done in chemistry, next to the old chemist who would like to have a succinct account of theories developed since his student days, and finally to all chemists for the vision it provides of the grandeur of their science.

The book is well produced and illustrated, except for some of the old drawings of familiar pieces of apparatus, and the few misprints should not mislead the reader.

J. C. WITHERS.

James Watt : Craftsman and Engineer. By H. W. DICKINSON. [Pp. xvi + 207, with 18 plates, 14 figures and 2 woodcuts.] (Cambridge : at the University Press, 1936. 10s. 6d. net.)

READ this book and then go as soon as you can to the Science Museum at South Kensington and see for yourselves some of the actual handiwork of the great James Watt, a man whose name most of us first heard in our nursery, a man, as Mr. Dickinson most persuasively claims, whose individual effort in contributing to the changes in our material world still stands unsurpassed.

One of the central themes of this story of Watt's life is his love of working with his own hands, and the profound influence of his manual skill on his whole career. Interwoven with this is his constant struggle with ill-health, and the clogging pessimism it engendered in his outlook ; but what else could be expected after his year in London working till late every night in a draughty workshop, never daring to seek fresh air in the streets after work for fear of the press-gang !

So Mr. Dickinson traces the story of Watt's early struggles and disappointments, the great help his many true friends gave him, and the slow but sure conversion of the engineering world of those days to a realisation of the value of his inventions. His greatest work, the engine with the separate condenser, was not of rapid growth ; after eight years' experimental work, the outside world did not " value the engine at a farthing." Five years later he was erecting so many engines that writing to Boulton, his partner, he says, " I fancy I must be cut in pieces and a portion sent to every tribe in Israel."

The wealth of quotations from Watt's letters re-create the atmosphere of his struggles, his pessimism and his final triumph ; he tells how the savage Cornish miners eat the grease which he needs for lubrication. In a fit of depression he writes, " Of all things in life there is nothing more foolish than inventing," but his far-seeing friend Boulton, scorning a licence to build engines for three countries only, insists, " I find it very well worth my while to make for all the world." And so eventually the two of them did.

There is a good index, and the numerous illustrations, both plates and diagrams, add greatly to the interest of the book ; even the technical reader will be delighted with the addition of the wifely thimble as a vital part of the first model engine with a separate condenser.

The print and lay-out of the page is excellent, and if I am forced to produce an adverse remark I will ask the printer why in Watt's letters he makes the constantly recurring symbol " & " so annoyingly large.

Mr. Dickinson has written a most readable book, not only for the engineer, but for every man ; his familiarity with his subject is unquestioned, and his larger treatise written some years ago awaits those eager for more.

G. T. R. H.

MISCELLANEOUS

The Clash of Progress and Security. By A. G. B. FISHER. [Pp.
xiii + 234.] (London: Macmillan & Co., Ltd., 1935. 8s. 6d. net.)

PROFESSOR FISHER deals in this book with the readjustments which economic
progress makes necessary, and which are so frequently ignored in popular
discussion. Politicians, journalists and the general public seem incapable
of realising that progress is not only consistent with, but actually involves
a diminution in, the *relative* importance of certain industries. This will
often mean an actual decline in the numbers of persons engaged in those
industries. Thus, instead of lamenting over the decline of agriculture and
other staple industries, we ought to rejoice over the fact that inventions and
improvements in those industries have enabled us to produce the quantity
of food, etc., required with a smaller outlay, and so have released resources
for the production of other things, things which are ingredients in a higher
standard of living.

This situation is familiar to economists, and Prof. Fisher's main function
has been to show that such progress is incompatible with a social system
which is too rigid to permit the necessary readjustments. If people refuse
to abandon occupations to which they have been brought up, and to invest
their capital in new and uncertain enterprises, or if they are prevented from
shifting their labour and capital to new fields, then they must be prepared
to abandon the advantages which scientific progress offers to them. The
demand for security represents the principal obstacle to change, and this
demand is frequently backed by governments who do their best to maintain
or even to expand industries which ought to undergo contraction.

Prof. Fisher presents the dilemma and puts his case with great lucidity.
To some extent, perhaps, he has neglected his own principle, and his thesis
is in some directions over-developed, in others under-developed : there is
a certain reiteration. Popular prejudices and misconceptions, however, can
only be overcome by driving home fundamental economic truths. This
important but much neglected duty of economists Prof. Fisher has per-
formed in an admirable manner.

E. L. HARGREAVES.

To-morrow's Children : the Goal of Eugenics. By ELLSWORTH
HUNTINGTON. [Pp. x + 139.] (New York: John Wiley & Sons,
Inc.; London: Chapman & Hall, Ltd., 1935. 6s. net.)

THIS book is concerned with the principles of eugenics and their application
to social problems. It is expressed in irritating catechismal form which
tends to a discontinuity of reading and a forced sense of dogmatism ; it is
a compilation made for the directors of the American Eugenic Society by
Ellsworth Huntington with the assistance of material supplied by other
members of the society.

In spite of the enormous annual expenditure of many billions of dollars
on the health and care of children it is questionable if the results obtained
are large in proportion to what they ought to be. Whereas the improvement
in education and health has been great, there has been little or no improve-
ment in the average type of home from which the children come. The
percentage of children coming from the finer type of home is less ; there is
a differential birth-rate the correction of which should be the aim of eugenics,

the twofold science of heredity (applied genetics) and sociology, whose sphere is the general improvement of physical and mental traits inherited by mankind as a whole. The subject is complicated by two independent influences— nature and nurture—the innate inherited characters of genes with their accidental mutations, and the variations of environments, and these influences may act in sympathy or in antipathy. The achievements of both privileged and unprivileged people depend both on innate capacity and on opportunity.

Practical eugenics are (1) *negative*, the limiting the number of children in families where inheritance is undesirable and training poor ; (2) *positive*. increasing the number of children in those families where inheritance is likely to be desirable and training good. This involves the question of selection, discouraging of births by sterilisation, birth control appliances and abortion, and the encouraging of births by lessening the burdens of child-bearing and family upbringing. In order that the selective eugenic process may work to the best advantage as far down the social scale as possible, great changes in our social and economic systems will be necessary and it must be long before a perfect eugenic system can prevail.

In the appendix there is a good account of the mechanism of heredity and Mendelism. There is a useful bibliography of English and American writings relating to Eugenics.

P. J.

The Future of Marriage in Western Civilisation. By EDWARD WESTERMARCK, Ph.D. [Pp. xiv + 281.] (London : Macmillan & Co., Ltd., 1936. 12s. 6d. net.)

NEARLY fifty years have elapsed since Dr. Westermarck published the first edition of his *History of Human Marriage*, and, as the development of his thought has shown, it is a subject upon which he has never ceased to ponder ever since. His examination of the place of marriage in modern western civilisation is deserving of the most careful consideration. His line of approach is here, as in his previous work, that of the anthropologist. He considers the problem by applying to it the principles which he has already applied to the study of marriage among peoples of a less highly developed civilisation, that is in relation to the specifically human needs which it serves. He discusses in detail and at length the various defects which have been imputed to the institution as it now exists, and the various remedies which have been proposed to alleviate or abolish them. His conclusion, as perhaps might have been expected, is that in the long run, so long as affection and desire for companionship between the sexes and love of children endure, marriage and the family life will continue.

E. N. F.

The Mesolithic Settlement of Northern Europe : A Study of the Food-gathering Peoples of Northern Europe during the Early Post-Glacial Period. By J. G. D. CLARK, M.A., Ph.D., F.S.A. [Pp. xvi + 284, with 8 plates, 74 figures and map in folder.] (Cambridge : at the University Press, 1936. 25s. net.)

IT is almost a matter of common form for the reviews of archæological works to refer to the enormous strides which have been made in this group of

studies in the last ten or fifteen years. Though this may partake of the obvious, nowhere is it so well worth emphasis as in the investigation of that section of the Stone Age which follows the Palæolithic and is known either as the Epi-Palæolithic, or, as Dr. Clark prefers to call it with good reason, the Mesolithic Age. In other branches of pre- and proto-historic investigation discoveries made since the close of the Great War have added to our knowledge of the early history and cultures of peoples ancestral to the great civilisations to an extent which would have seemed an impossibility to the archæologist of a generation ago ; but in none more than in that dealing with the Mesolithic period is it possible to appreciate the results of the development of scientific method and technique in the study and interpretation of the data from which the archæologist reconstructs his story of a remote past. Rigorously tested inference from proved fact scientifically recorded has taken the place of speculation ; and these inferences are now interpreted and their implications extended in the light of relevant conclusions furnished by a number of branches of scientific study dealing with problems to which the material of archæological studies is now seen to be related. The development of archæology as a science was effected largely by the application of the methods and results of geological and palæontological investigation. To an extended employment of these auxiliaries are now added geography, climatology, and at the moment, perhaps, most helpful of all, palæobotany in the form of pollen analysis. Geochronology, through the examination of varves or deposits of silt as elaborated by de Geer, which may ultimately as a result of further research have a world-wide application, is as Dr. Clark indicates, on the way to providing an exact chronology for Northern Europe in place of the estimates alone available hitherto, which have been based upon calculations from the epochs of historic civilizations, these calculations themselves not always being above criticism.

It is not, however, in the scientific interpretation of its data alone that study of the Mesolithic Age has advanced. These data themselves have been extended in a remarkable degree. It is significant that in 1922, after the Yorkshire Maglemose " harpoons " had been exhibited at the Hull meeting of the British Association, so little was known of the Maglemose culture that there was justification for the appointment of a committee of inquiry to investigate whether they were genuine or not. A wider knowledge of the culture would at once have put the imputation out of court. Yet as Dr. Clark holds, the Maglemose is now known as the most widely distributed of all the Mesolithic cultures, extending over the whole of Northern Europe.

In this book Dr. Clark has followed up his earlier work, *The Mesolithic Age in Britain*, by an intensive study and in most instances personal examination of the material, which can be attributed to this period, from all the sites of Northern Europe. The larger canvas affords opportunity for broader treatment, of which Dr. Clark has not failed to take advantage. He takes the Mesolithic Age to extend from about 8300 B.C. down to about 3000 B.C., when it passes into the Neolithic. He divides it into three periods, each characterised by climatic changes, variation in sea-level and transformations of fauna and flora, the last-named passing from the stage, in which in a pre-boreal climate birch, pine and willow predominate, to the Atlantic, when alder and oak-mixed forest are dominant. After tracing the retreat of the ice-sheet and reviewing the system of chronology, he deals with the readjustment of land and sea levels and the adjustment of life to the changing

environment before passing on to the developments in culture which are associated with them.

Dr. Clark distinguishes three main cultures. Of these the earliest is that of the flake, characterised by tanged points with simple forms of microliths, which flourished over the whole of Europe during period I from Belgium to the Ukraine, at a time when the immigration of forest trees had not seriously modified the environment. For its affinities in the Late Palæolithic he looks to Font Robert and Predmost. The second is the axe culture, cradled in north Germany and Sweden, arising in response to forests and developing into Maglemose by a coalition of the microlithic Tardenoisian and some vestiges of Magdalenian artistic tradition. It flourished over the whole of the north European plain. Local development produced such variants as the Ertbölle and Lower Halstow, while survivals persisted into the neolithic. Lastly comes the microlithic Tardenoisian of the sand areas and highlands, derivative ultimately from Africa, which although extending from Britain to the Ukraine, as Dr. Clark points out, in its main development lies outside the area with which he is concerned. It will be noted that Dr. Clark's periods in a cultural sense are periods of overlap rather than of strict chronological succession.

Dr. Clark has interpreted a mass of difficult detail with surprising clarity. If there are still obscurities, as he is the first to admit, they are due to the character and imperfections of the material rather than to any failure in method or lack of skill in treatment. If we may venture to predict his book will become a classic.

E. N. F.

Objective Evolution. By C. PLEYDELL-BOUVERIE. [Pp. 233.] (London : Williams & Norgate, Ltd., 1936. 7s. 6d. net.)

THIS is a very difficult book to read, and still more difficult to understand, ranging, as it does, over a variety of highly disputable topics, psychological, sociological, economic and political. Its purpose is best presented in the author's own words. " This work presents a politico-cosmological ideal whose aim is the evolution of the efficiency of psychic reactions to the status of sense. Psychic sense registers intuitional reactions transcendental to the psychic order of time. Briefly, political organisation can assist the evolution of psychic efficiency to the point where conscious experiences occur in a new order. But this is only a step, for there are no terrestrial or astrophysical limits to the scientific evolution of psychological values, namely the evolution of creative consciousness regarded as a final cosmic reality " (Preface). Beginning in a questionable, and rather credulous, psychology, the thesis ends with an economic theory of " Social Credit," which presumably would make the development of new mental powers possible. But the author tells us that the economic conclusions were evolved independently of his " psychorelative theory of world emancipation." The style of the book is dogmatic, and, where hypothetical, appears to accept the hypothesis as fact. Difficult as it inherently is, the text is made more so by misprints, and in some cases misleading misprints. The author has a vexatious usage of ending a question with a full stop. And there is no index.

F. A.

The Freedom of Man. By ARTHUR H. COMPTON. [Pp. xiv + 152.] (New Haven : Yale University Press ; London : Humphrey Milford, 1935. 9s. net.)

THIS book is based on the ninth series of lectures delivered at Yale University on the foundation established by the late Dwight Harrington Terry. The author is a distinguished physicist who is " strongly impressed by the way in which science can be interpreted to give a moral and ethical outlook closely parallel with that taught by the highest types of religion." The lectures are concerned with the age-long conflict on the question, " Is man a free agent ? "—the insoluble dilemma of " Free will " and " determinism." The quantum theory by which the old principle of causality is abandoned, and that natural phenomena do not obey exact laws, is regarded by the author as the most significant revolution in the history of scientific thought, so much so that the old contention between physicists and philosophers has disappeared. The principle of " uncertainty " is discussed by studying the movement of a ray of light (photon) diffracted through a pin orifice and collected on photo-electric cells ; the device is interesting and ingenious, but it is unfortunate when a " dæmon " is brought in to assist in the experiment. The introduction of supernatural agents is rarely helpful even in literature.

The lectures on " Man's place in God's world " and " Death or life eternal " are over-weighted by personal bias. There is confusion in the significance of words like God, soul, thought, mind, brain, conscience, etc. These lectures also involve questions so impossible of scientific solution that it would be better perhaps they should not be discussed. Nevertheless, this is a stimulating book, well worth reading ; it is clearly written and full of great interest.

<div align="right">P. J.</div>

The Next Hundred Years : The Unfinished Business of Science. By C. C. FURNAS. [Pp. xx + 366.] (London : Cassell & Co., Ltd., 1936. 8s. 6d. net.)

SEVERAL years ago Sir James Jeans revived the layman's interest in science, and since the publication of *The Mysterious Universe* numerous books have appeared, attempting to explain various aspects of science to the non-specialist. Prof. Furnas, having the same object in view, has dealt with the subject in a new manner, endeavouring to forecast the future advances of science.

The author has first taken stock of our present knowledge of Biology, Chemistry, Physics, Engineering and Agriculture, which has enabled him to give popular explanations of many scientific theories and facts. He next shows how little we really know about many scientific phenomena, and how —especially in Biology and Medicine—we have so far explored only the fringe of these subjects. This leads the author to an attempt at prophesying what scientific achievements we can expect in the future, and what will be their effect on civilisation. The last section of the book is probably the most interesting, for the author realises that scientific discovery applied to industry will mean less employment, and therefore ultimately a change in our social and economic life. This section, entitled " Social Consequences," contains no new views or theories, but the author's argument for security for all and a liberal education for leisure is convincing.

It is a pity that a book with such a wide theme should be so strongly

biased towards American science. Every illustration is drawn from American sources, although the author could in some cases have obtained much wider and better examples if he had gone to other countries. All twentieth-century scientific research has apparently been done in America, and we are told the names and initials of the whole team who synthesised a protein, with the site and benefactor of their laboratory, whilst the Curies' life-work is passed over in a few words.

These defects, although annoying to the non-American reader, should not entirely spoil the book, and it can be recommended to any layman who is interested in the possible developments of science during the next three or four generations.

T. C.

Tylor. By R. R. MARETT, M.A., D.Sc., LL.D. [Pp. 220, with frontispiece.] (London : Chapman & Hall, Ltd., 1936. 6s. net.)

Pareto. By FRANZ BORKENAU. [Pp. 219, with frontispiece.] (London : Chapman & Hall, Ltd., 1936. 6s. net.)

THESE two books are the first of the " Modern Sociologists " volumes issued under the Editorship of Professor Ginsberg and Mr. Farquharson. The Editors are to be congratulated upon having secured Dr. Marett and Dr. Borkenau to launch their Series with studies of such interesting subjects as the anthropological work of Tylor and the sociological theories of Pareto. Each book, in its way, is admirable. Dr. Marett apologises to his readers, it is true, because he had to compose his " sketch " rather hurriedly, on holiday, and away from reference books, which made it necessary for him sometimes " to fall back on precious memories of walks and talks " with Tylor himself. But his *Tylor* needs no such apology. Knowing the sources and references as he does, it is just this intimate touch that gives its quality to what he has written. His little volume covers the whole ground, stressing always the sociological bearing of the anthropology ; and, solid as its treatment is, is eminently readable. *Tylor* will appeal to a far larger circle of readers than the professed students of the science of human and social affairs.

Dr. Borkenau's *Pareto* is, necessarily, a book of an altogether different stamp ; for Pareto's theoretical constructions, which the author analyses, presents and criticises with such judgment and skill, are in sharp contrast with the straightforward and empirical science of Tylor. No doubt Pareto wishes to apply a scientific method to the sociological data he examines ; but this is not the method that he actually follows, as is clearly shown in Dr. Borkenau's chapters on Logical and Non-logical Actions, Residues, and Derivations—Pareto's peculiar terms for something akin to intelligent and instinctive activities and rationalisations. More interesting is Pareto's treatment of the Theory and Circulation of Élites which, as the author remarks, " pierces the shades of egalitarianism," just as his Residues and Derivations give " a real knock-out blow to dying rationalism." But all this is social philosophy rather than social science ; and Dr. Borkenau feels that one might be inclined to doubt whether Pareto's sociology is worth serious study so far as any objective scientific value is to be found in its theories. Yet Pareto is important, since in his work " for the first time the powerful tendency towards a change of political machinery and social

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organisation since embodied in Bolshevism, Fascism, National-Socialism and a score of similar movements has found clear expression." The last two chapters of the book deal with Bolshevism and Fascism respectively in their social, economic and political aspects, as a test of Pareto's theory, and as an illustration of the working out in practice of his contradictory ideas. " Pareto can be best understood when designated the precursor of Fascism."

Each of these books has an introductory biographical chapter, a feature always useful, when revelatory of the formation of character as it is in these volumes, in helping one to understand the outlook and ideology of its subject.

F. A.

The Measurement of Population Growth : Methods and Results. By ROBERT R. KUCZYNSKI. [Pp. vi + 255.] (London : Sidgwick & Jackson, Ltd., 1935. 12s. 6d. net.)

THE student will carefully note the title and sub-title of this work to which the contents are strictly relevant. It is not a treatise on population— although it gives much valuable data on the subject. It is a treatise on the methods and results of present-day practices for the measurement of the growth of populations.

The policy of public health authorities and the interests of insurance corporations, both in England and abroad, have conspired to give the place of importance to mortality and its measurement, and fairly adequate means to this end have been developed ; but it cannot be said that adequate methods are available to measure the now more important factors in population growth of fertility and fecundity.

As soon as it is appreciated that the decisive factor in calculation has shifted from mortality to fertility an appraisal of the methods of measuring fertility becomes of practical importance. This book makes a valuable contribution to that process.

There are some facts that can never be determined, at least with that degree of accuracy which will satisfy the scientific mind. The most that can be hoped for is a compromise between uncertain factors and generalisations that will, as far as possible, eliminate the influence of unascertainable and uncertain elements. This applies with peculiar force to estimates of fertility and other vital statistics, whether actual or ratios. The compilers of vital statistics are torn between the need to keep pace with changing economic conditions and points of view, and the obvious necessity of presenting periodical data in comparable forms. Is the purpose of these data to give a rough and general picture of the trend of population growth and movements, or is it to afford a basis for precise scientific study ?

Scattered throughout the pages of this book is a valuable series of definitions—birth-rate, fertility, fecundity, total fertility rates, etc.—which it is essential that the student should know and understand. It would add to the value of the book if these definitions were collected and printed together and so help to clarify the mind of the reader.

There is no subject of such importance to the student of economics to-day as the subject of population growth, with its numerous and wide ramifications. The informative character of this book and its insistence on correct methods should do much to assist the student.

E. J. L.

The Eskimos. By KAJ BIRKET-SMITH, Ph.D. With a Foreword by DIAMOND JENNESS, M.A., F.R.A.I. [Pp. xiv + 250, with 32 plates and an endpaper map.] (London: Methuen & Co., Ltd., 1936. 15*s.* net.)

THE Eskimos have naturally excited a good deal of attention. They live under the severest conditions to which man is subjected. They have evolved a culture which is both interesting and, on its material side, remarkably ingenious. It is possible, though by no means certain, that they or their near relations inhabited Europe at the end of the Palæolithic period. Finally, whatever may or may not be the advantage of having a large brain, it is at any rate certain that the Eskimos alone of the non-civilized races have got a brain whose size on the average exceeds that of the European. Dr. Birket-Smith's book is extraordinarily complete. He discusses first the geographical conditions, and the physique and psychological characters of the Eskimo. Next there is a most interesting chapter on the language. This is followed by a consideration of the greatest problems with which the Eskimos have to contend, the search for food and the struggle against the cold. This is followed by a description of social organisation of the people, and their outlook on life. He concludes with an historical essay on the origin and development of the Eskimo culture and their relations with the Indians. Finally there is an important chapter on the relations of the Eskimos and the Whites. This chapter is of wide interest; the author compares the different methods of the various powers who control sections of the Eskimo and criticises the methods used, and his suggestions have a wider importance than the mere consideration of a single people, important as that is, because the general principles of colonial administration apply equally to the Eskimo and to any other primitive people. The importance of the study of the economic life of subject peoples is especially carefully discussed. It should be said finally that though the book contains a very great deal of specialist information it is one which can easily be read with interest by those who are not specialists, and is one which is likely to appeal to anyone who wishes to know what is the general trend among scientific anthropologists to-day, who are not committed to any particular school. Dr. Birket-Smith has been fortunate in his translator, W. E. Calvert; Prof. Daryll Forde has revised the translation, and few of the inevitable awkwardnesses of a translation remain. The photographs are excellent and there is a useful bibliography.

<div align="right">L. H. D. B.</div>

Asia: A Regional and Economic Geography. By L. DUDLEY STAMP, D.Sc., B.A., M.I.P.T. Third edition, enlarged and partly rewritten. [Pp. xxii + 704, with 372 figures.] (London: Methuen & Co., Ltd., 1936. 27*s.* 6*d.* net.)

THE earlier editions have already established this book as an indispensable work of reference for students of the geography of Asia. As such this latest edition will be even more valuable. Statistics have been brought up-to-date, and the results of much valuable work done in recent years on the Far East by American geographers (notably Trewartha, Hall, and Cressey) have been incorporated. In consequence the Far East now receives much more adequate treatment and the balance of the book is improved. Perhaps its most outstanding quality is its wealth of geographical infor-

mation expressed in maps and diagrams, of which there are 372, including many additions to those in the earlier editions. Almost all are clear and well executed, though one may note Fig. 308 as an exception.

In the preface to this edition Dr. Stamp formulates his concept of geography, and the reader is thereby encouraged to look for a systematic application of the concept in this book. This hope is not realised. The book is a skilful compilation from scattered and uneven material, but it is not welded into a unified geography of Asia in accordance with the concept of the subject set forth in the preface. In particular the view that geography "is concerned with the interaction between Man and his environment considered dynamically and not statically—that is with the sequence of human occupance" is far from implicit in the regional treatment. Little space is given to the study of settlement, although this reflects the geographical synthesis in such large measure.

A. E. S.

Rockets through Space, or the Dawn of Interplanetary Travel.
By P. E. CLEATOR. [Pp. 243, with 22 plates and 21 figures.] (London : George Allen & Unwin, Ltd., 1936. 7s. 6d. net.)

INTEREST in rocket propulsion has quickened within the last few years, and a number of books have appeared on the subject. As these are mostly of foreign origin, a book in English on the subject is welcome. A number of societies devoted to space-travel—or rather, to discussions on the possibility of space-travel—have been started, and the author is, in fact, President of the British Interplanetary Society. He plunges *in medias res* ; no aeronautics for him, his book is to be devoted exclusively to these new astronautics, and although it is true that he gives us a chapter on the evolution of the liquid fuel drive for rockets, he deprecates the suggestion that we should make experiments on land or even over the land to improve the efficiency of the rocket-motor before we venture further. He cries for the moon (and other members of the solar system) and nothing short of this will satisfy him ! Why waste time on laboratory experiments ! Let us put Jules Verne to the test forthwith, now that with jet-propulsion we have overcome the difficulty of locomotion in a vacuum !

One cannot help feeling that the President of the B.I.S. is trying to fly before he can walk. In this he is supported by Prof. A. M. Low, who, in a preface, says : " It was only prejudice that prevented radio from being employed in the time of Charles II." The writer may be wrong but he feels that it is something more than mere prejudice that prevents Imperial Airways from running excursions to Mars in the time of Edward VIII ! Before suggesting that this country has lagged behind others in research on this type of propulsion the author would have been well advised to consult files of the *Journal* of the Aeronautical Society and the *Transactions* of the Institute of Naval Engineers, in which he will find several references to this question, dating back, in the latter publication, to 1885, when Barnaby built the first jet-propelled ship for the Royal Navy.

Lest we seem over-sceptical of the realisation of the hopes of the author, we will hasten to add that he has produced a very readable book, and even those who do not expect interplanetary travel in our time will find much to interest them in it.

E. G. R.

The Science Masters' Book, Series II. Part II: Biology— Chemistry—Experiments for Receptions. Edited by G. H. J. ADLAM, O.B.E., M.A., B.Sc. [Pp. xvi + 267, with 97 figures.] (London: John Murray, 1936. 7s. 6d. net.)

THE first Science Masters' Book was a " scrap book," consisting of a number of experiments and hints on science teaching in schools, selected from *The School Science Review*. It has been so successful that a second series has now appeared, following along the lines of the first, but containing other material. The new arrival is naturally a pretty mixed bag, but contains much that is ingenious and much that is helpful. It keeps up the high standard of its predecessor, and should find its way into every school library.

<div align="right">G. P. W.</div>

Libraries for Scientific Research in Europe and America. By H. PHILIP SPRATT, B.Sc., A.I.Mech.E. [Pp. 227, with 1 plate.] (London: Grafton & Co., 1936. 10s. 6d. net.)

MR. SPRATT, who was until recently a member of the staff of the Science Museum Library, South Kensington, and is also an engineer, has travelled extensively in Europe and America. His book is the result of notes made in the course of his journeys and is confined in the main to statements of fact ; indeed, the author has resisted to a truly remarkable degree the temptation to record impressions and express opinions. But it is perhaps doubtful whether it would not have been better at the same time to exercise more discrimination in the choice of libraries visited, if the intention was to produce a book of real service to scientific workers. Mr. Spratt has written an admirable chapter on the Library of the Science Museum, followed by one devoted to " Specialised Science Libraries in London." Under this head he gives us very useful information about the Libraries of the Institutions of Mechanical Engineers, of Civil Engineers, and of Electrical Engineers, the Patent Office, and the British Library of Political and Economic Science at the London School of Economics, but he has entirely omitted others of equal importance to scientists in other fields. Of the great Library of the Chemical Society and of the various invaluable medical libraries, for example, there is no mention at all. On the other hand, Mr. Spratt has included a number of libraries of a quite general character, interesting indeed to a Librarian but surely hardly suitable in a work which professes also to be of service to scientific research workers. This is true in particular of several University libraries in the United States, which seem to have no greater claim to admission than have similar institutions in this country. Of the Archives Nationales in Paris, we are told that there is, among its other treasures, a letter of Parmentier on the cultivation of the potato, but nothing else likely to tempt the scientific worker to go there.

However, in spite of these defects, Mr. Spratt's book has the merit of bringing together a good deal of information which it has not hitherto been possible to find in one place ; he tells us what catalogues and other guides to its resources each library provides, what system of classification it adopts, what facilities it offers in the way of special rooms, access to the book-stacks, " carrels " for research workers, and the like.

<div align="right">J. W.</div>

Industrial Research Laboratories. A List compiled by the Association of Scientific Workers. Sir Halley Stewart Trust Publications III. [Pp. 104.] (London: George Allen & Unwin, Ltd., 1936. 3s. 6d. net.)

It is a little difficult to visualise for whom this book was compiled, or what limits were fixed as to the laboratories that should and should not be included in the list. There is no preface or foreword to guide one in this respect. Such a publication could prove very useful, and the following criticisms are made in the hope that a second edition will be called for, in which some of the deficiencies of this edition may be remedied.

There are no cross-references, and, on first consulting the book, the reader might imagine that the National Physical Laboratory was omitted. On looking through the book, one finds that all the laboratories which are under the control of the Department of Scientific and Industrial Research are listed under the heading " Department of Scientific and Industrial Research." It remains, however, somewhat difficult to pick out the laboratory required, since there is no differentiation of type to enable this to be done.

A number of important industrial research laboratories do not appear to be in the list at all. Thus, one of the biggest organisations, the British Cotton Industry Research Association, is not mentioned, nor are the British Refractory Research Association, the Research Association of British Paint, Colour and Varnish Manufacturers, the Imperial Bureau of Soil Science, nor the Laboratories controlled by the Ministry of Agriculture and Fisheries at Lowestoft and Alresford.

It is also difficult to understand on what principle a selection has been made of Consulting and Analytical Chemists. Either the list should have been considerably increased or such laboratories omitted altogether.

There does not seem to be any advantage gained by printing the research papers that have emanated from certain institutions, and giving a number of pages to these, while no research papers are shown from other institutions. It would have been better to omit these altogether. The space saved might have been given with advantage to the inclusion of the principal members of the staff at such places as Rothamsted Experimental Station, the National Physical Laboratory, the Geological Survey of Great Britain, the Chemical Research Laboratory, and other laboratories of the Department of Scientific and Industrial Research.

Incidentally the Laboratories of the British Non-Ferrous Metals Research Association are in Euston Street and not Euston Road as stated.

F. P. D.

BOOKS RECEIVED

(Publishers are requested to notify prices.)

Heaviside's Operational Calculus. As Applied to Engineering and Physics. By Ernst Julius Berg, Sc.D., Professor of Electrical Engineering, Union College. Second edition. Electrical Engineering Texts. New York and London : McGraw-Hill Publishing Co., Ltd., 1936. (Pp. xvi + 258, with 101 figures.) 18s. net.

Differential and Integral Calculus. Vol. II. By R. Courant, Professor of Mathematics in New York University, formerly in the University of Gottingen. Translated by J. E. McShane, Professor of Mathematics in the University of Virginia. London and Glasgow : Blackie & Son, Ltd., 1936. (Pp. x + 682, with 112 figures.) 30s. net.

Mathematics for the Million. A Popular Self Educator. By Lancelot Hogben. Illustrated by J. F. Horrabin. London : George Allen & Unwin, Ltd., 1936. (Pp. 647, with frontispiece and 197 figures.) 12s. 6d. net.

Elements of Probability. By H. Levy, M.A., D.Sc., F.R.S.E., Professor of Mathematics, and L. Roth, M.A., Assistant Lecturer in Mathematics, Imperial College of Science and Technology. Oxford : at the Clarendon Press ; London : Humphrey Milford, 1936. (Pp. x + 200, with 25 figures.) 15s. net.

The Realm of the Nebulæ. By Edwin Hubble, of the Mount Wilson Observatory, Carnegie Institution of Washington. Yale University, Mrs. Hepsa Ely Silliman Memorial Lectures. London : Oxford University Press, 1936. (Pp. xiv + 210, with 15 plates, 16 figures and 8 tables.) 12s. 6d. net.

Discrete Spacetime. A Course of Five Lectures delivered in the McLennan Laboratory. By Ludwik Silberstein, Ph.D., Consulting Mathematician, Eastman Kodak Co., Rochester, N.Y. University of Toronto Studies, Physics Series, 1936. Toronto : The University of Toronto Press ; London : Humphrey Milford, 1936. (Pp. 49, with 5 figures.) 4s. 6d. net.

Manual of Meteorology. Vol. II : Comparative Meteorology. By Sir Napier Shaw, LL.D., Sc.D., F.R.S., late Professor of Meteorology in the Imperial College of Science and Technology and Reader in Meteorology in the University of London. With the assistance of Elaine Austin, M.A., of the Meteorological Office. Second edition. Cambridge : at the University Press, 1936. (Pp. xlviii + 472, with iii + 225 figures.) 36s. net.

Weather Science for Everybody. By David Brunt, M.A., Professor of Meteorology in the University of London. Changing World Library, No. 2. London: Watts & Co., 1936. (Pp. xii + 170, with 6 plates and 20 figures.) 2s. 6d. net.

Earthquakes. By Nicholas Hunter Heck, Chief, Division of Terrestrial Magnetism and Seismology, U.S. Coast and Geodetic Survey, Department of Commerce, Washington. Princeton: Princeton University Press; London: Humphrey Milford, 1936. (Pp. xii + 222, with 88 figures.) 16s. net.

Great Earthquakes. By Charles Davison, Sc.D., F.G.S. London: Thomas Murby & Co., 1936. (Pp. xii + 286, with 12 plates and 97 figures.) 17s. 6d. net.

A Textbook of Physics. For Students of Science and Engineering. By Charles A. Culver, Professor of Physics in Carleton College. New York and London: Macmillan & Co., Ltd., 1936. (Pp. x + 816, with 525 figures.) 17s. net.

The Physics of Solids and Fluids. With Recent Developments. By P. P. Ewald, Th. Pöschl and L. Prandtl. Authorised Translation by J. Dougall, M.A., D.Sc., F.R.S.E., and W. M. Deans, M.A., B.Sc. Second edition. London and Glasgow: Blackie & Son, Ltd., 1936. (Pp. xiv + 396, with 325 figures.) 17s. 6d. net.

Elementary Physics. For Medical, First Year University Science Students and General Use in Schools. By G. Stead, M.A., F.Inst.P., Reader in Physics in the University of London (Guy's Hospital Medical School), University Lecturer in Physics as applied to Medical Radiology, Cambridge. Fifth edition. London: J. & A. Churchill, Ltd., 1936. (Pp. xvi + 559, with 430 figures.) 12s. 6d. net.

An Elementary Survey of Modern Physics. By Gordon Ferrie Hull, Professor of Physics, Dartmouth College. New York and London: Macmillan & Co., Ltd., 1936. (Pp. xxiv + 457, with frontispiece, 231 figures and 13 tables.) 20s. net.

The Renaissance of Physics. By Karl K. Darrow, Ph.D., Research Physicist, Bell Telephone Laboratories. New York and London: Macmillan & Co., Ltd., 1936. (Pp. x + 306, with frontispiece and 44 figures, including 16 plates.) 12s. 6d. net.

Properties of Matter. By F. C. Champion, M.A., Ph.D.(Cantab.), Lecturer in Physics, University of London (King's College), and N. Davy, M.Sc.(Lond.), Senior Lecturer in Physics, University College, Nottingham. The Student's Physics. London and Glasgow: Blackie & Son, Ltd., 1936. (Pp. xiv + 296, with 161 figures.) 15s. net.

Les Atomes. By Jean Perrin, Membre de l'Institut, Prix Nobel. Paris: Librairie Félix Alcan, 1936. (Pp. xxiv + 319, with 26 figures.) Frs.15.–.

Relativity Theory of Protons and Electrons. By Sir Arthur Eddington, M.A., D.Sc., LL.D., F.R.S., Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge. Cambridge: at the University Press, 1936. (Pp. viii + 336.) 21s. net.

Cosmic Rays Thus Far. By Harvey Brace Lemon, Professor of Physics, the University of Chicago. With a Foreword by Arthur Holley Compton. Drawings by Chichi Lasley. London : William Heinemann, Ltd., 1936. (Pp. 128, with 22 figures, including 5 plates.) 7s. 6d. net.

Wireless Engineering. By L. S. Palmer, D.Sc., Ph.D., F.Inst.P., M.I.E.E., Professor of Physics, the University College of Hull. London, New York, Toronto : Longmans, Green & Co., 1936. (Pp. xii + 544, with 353 figures.) 21s. net.

Electronics and Electron Tubes. By E. D. McArthur, Vacuum Tube Engineering Department, General Electric Company. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1936. (Pp. viii + 173, with 89 figures and 7 tables.) 12s. 6d. net.

An Introduction to Neon Lighting. By James Orr, M.I.E.S., and A. W. Forrest, B.Sc. Blackie's " Technique " Series. London and Glasgow : Blackie & Son, Ltd., 1936. (Pp. viii + 80, with 37 figures.) 3s. 6d. net.

Mercury Arcs. By F. J. Teago, D.Sc., M.I.E.E., Robert Rankin Professor of Electrical Machinery in the University of Liverpool, and J. F. Gill, M.Sc., A.M.I.Mech.E., A.M.I.E.E. Methuen's Monographs on Physical Subjects. London : Methuen & Co., Ltd., 1936. (Pp. viii + 104, with frontispiece and 49 figures.) 3s. net.

Handbook of Engineering Fundamentals. Prepared by a Staff of Specialists under the Editorship of Ovid W. Eshbach. Wiley Engineering Handbook Series, Vol. I. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1936. (Pp. xii + 1081, with numerous figures and tables.) 25s. net.

Practical Stereoscopic Photography. By J. Moir Dalzell, L.R.C.P., L.R.C.S.(Edin.). With a Foreword by W. L. F. Wastell, Hon. F.R.P.S. London : The Technical Press, Ltd., 1936. (Pp. xvi + 224.) 10s. 6d. net.

Inorganic Chemistry. A Survey of Modern Developments. By Sir Gilbert T. Morgan, D.Sc., Sc.D., LL.D., F.R.S., F.I.C., A.R.C.Sc., Director of Chemical Research, and Francis Hereward Burstall, M.Sc., A.I.C., of the Chemical Research Laboratory, Teddington. Cambridge : W. Heffer & Sons, Ltd., 1936. (Pp. x + 462, with 16 figures and 10 tables.) 15s. net.

Gmelins Handbuch der anorganischen Chemie. 8. Auflage. Herausgegeben von der Deutschen Chemischen Gesellschaft. System-Nummer 22 : Kalium, Lieferung 1. Berlin : Verlag Chemie, G.m.b.H., 1936. (Pp. vi + 246, with 7 figures.) RM. 28.50.

Synthetic Inorganic Chemistry. A Course of Laboratory and Classroom Study for First Year College Students. By Arthur A. Blanchard, Ph.D., Professor of Inorganic Chemistry, Joseph W. Phelan, S.B., late Professor of Inorganic Chemistry, and Arthur R. Davis, Ph.D., Assistant Professor of Chemistry, at the Massachusetts Institute of Technology. Fifth edition. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1936. (Pp. xii + 385, with 25 figures.) 15s. net.

Recent Advances in General Chemistry. By Samuel Glasstone, Ph.D., D.Sc.(Lond.), F.I.C., Lecturer in Physical Chemistry at the University of Sheffield. London: J. & A. Churchill, Ltd., 1936. (Pp. x + 430, with 25 figures, including 2 plates, and 77 tables.) 15s. net.

Recent Advances in Physical Chemistry. By Samuel Glasstone, Ph.D., D.Sc.(Lond.), F.I.C., Lecturer in Physical Chemistry at the University of Sheffield. Third edition. London: J. & A. Churchill, Ltd., 1936. (Pp. viii + 477, with 31 figures and 66 tables.) 15s. net.

Physical Chemistry. By Frank H. MacDougall, M.A.(Queen's), Ph.D.(Leipzig), Professor of Physical Chemistry, University of Minnesota. New York and London: Macmillan & Co., Ltd., 1936. (Pp. x + 721, with 97 figures and 130 tables.) 17s. net.

The Thermochemistry of the Chemical Substances. By F. Russell Bichowsky, Editor for Thermochemistry for the International Critical Tables, and Frederick D. Rossini, Scientist in Physical Chemistry at the National Bureau of Standards. New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1936. (Pp. 460.) 35s. net.

Differential Equations in Applied Chemistry. By Frank Lauren Hitchcock, Ph.D., Professor of Mathematics, and Clark Shove Robinson, S.M., Associate Professor of Chemical Engineering, in the Massachusetts Institute of Technology. Second edition. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1936. (Pp. viii + 120, with 4 figures and 1 chart.) 7s. 6d. net.

The Theory of the Properties of Metals and Alloys. By N. F. Mott, M.A., F.R.S., Professor of Theoretical Physics, and H. Jones, Ph.D., Lecturer in Theoretical Physics, in the University of Bristol. The International Series of Monographs on Physics. Oxford: at the Clarendon Press; London: Humphrey Milford, 1936. (Pp. xiv + 326, with 108 figures and 12 tables.) 25s. net.

Tungsten. A Treatise on its Metallurgy, Properties and Applications. By Colin J. Smithells, M.C., D.Sc., Member of the Research Staff of the General Electric Co., Ltd. Second edition. London: Chapman & Hall, Ltd., 1936. (Pp. viii + 272, with 183 figures, including 44 plates and 43 tables.) 25s. net.

Carbon Dioxide. By Elton L. Quinn, Professor of Chemistry, University of Utah, and Charles L. Jones, formerly Chief Engineer, American Dry Ice Corporation. American Chemical Society Monograph Series. New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1936. (Pp. 294, with 89 figures and 90 tables.) 37s. 6d. net.

Coal. Its Constitution and Uses. By William A. Bone, D.Sc., Ph.D., F.R.S., Professor, and Godfrey W. Himus, Ph.D., M.I.Chem.E., D.I.C., Lecturer in Fuel Technology, Imperial College of Science and Technology. With a supplementary chapter upon Fuel Economy and Heat Transmission in Industrial Furnaces. By Reginald J. Sarjant, D.Sc., D.I.C. London, New York, Toronto: Longmans, Green & Co., 1936. (Pp. xvi + 631, with 22 plates, 114 figures and 136 tables.) 25s. net.

Miners' Safety Lamps. Their Construction and Care. By T. R. Barnard, B.Sc.Tech., A.M.I.Min.E., M.A.M.E.E., Certificated Colliery Manager.

London : Sir Isaac Pitman & Sons, Ltd., 1936. (Pp. xii + 88, with frontispiece and 17 figures.) 4s. net.

Industrial Chemical Calculations. The Application of Physico-Chemical Principles and Data to Problems of Industry. By O. A. Hougen, Ph.D., Associate Professor of Chemical Engineering, University of Wisconsin, and K. M. Watson, Ph.D., Chemical Engineer, Universal Oil Products Co., Chicago. Second edition. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1936. (Pp. x + 487, with 97 figures and 21 tables.) 22s. 6d. net.

Essential Principles of Organic Chemistry. By Charles S. Gibson, O.B.E., M.A., Sc.D., F.R.S., Professor of Chemistry in the University of London, at Guy's Hospital Medical School. Cambridge : at the University Press, 1936. (Pp. viii + 548, with 38 figures.) 18s. net.

A Text-Book of Organic Chemistry. By Dr. Julius Schmidt. English edition by H. Gordon Rule, Ph.D. Third edition. London : Gurney & Jackson, 1936. (Pp. xxiv + 865.) 25s. net.

Outlines of Organic Chemistry. By E. J. Holmyard, M.A., M.Sc., D.Litt., F.I.C., late Scholar and Research Student of Sidney Sussex College, Cambridge, Head of the Science Department, Clifton College. Second edition. London : Edward Arnold & Co., 1936. (Pp. xii + 467, with 6 plates and 39 figures.) 7s. 6d.

Quantitative Organic Microanalysis of Fritz Pregl. By Dr. Hubert Roth, Assistant at the Kaiser Wilhelm Institute for Medical Research in Heidelberg. Third English edition, translated from the fourth German edition by E. Beryl Daw, B.Sc., A.I.C. London : J. & A. Churchill, Ltd., 1937. (Pp. xvi + 271, with 72 figures, including 1 plate.) 18s. net.

Semi-Micro Qualitative Analysis. By Carl J. Engelder, Ph.D., Professor of Analytical Chemistry, University of Pittsburgh, Tobias H. Dunkelberger, B.S., University of Pittsburgh, and William J. Schiller, Ph.D., Mount Mercy College, Pittsburgh, Pa. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1936. (Pp. x + 265, with 9 tables.) 13s. 6d. net.

Textbook of Quantitative Inorganic Analysis. By I. M. Kolthoff, Ph.D., Professor of Analytical Chemistry, and E. B. Sandell, Ph.D., Instructor in Analytical Chemistry, University of Minnesota. New York and London : Macmillan & Co., Ltd., 1936. (Pp. xvi + 749, with 116 figures and 47 tables.) 20s. net.

A Textbook of Elementary Quantitative Analysis. By Carl J. Engelder, Ph.D., Professor of Analytical Chemistry, University of Pittsburgh. Second edition. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1936. (Pp. xiv + 270, with 12 figures and 16 tables.) 13s. 6d. net.

Elementary Practical Chemistry. By Arthur I. Vogel, D.Sc., D.I.C., F.I.C., Head of Chemistry Department, Woolwich Polytechnic. London and Glasgow : Blackie & Son, Ltd., 1936. (Pp. 220, with 73 figures.) 3s.

The Essentials of Chemical Physiology. For the Use of Students. By the late W. D. Halliburton, M.D., LL.D., F.R.S., J. A. Hewitt, Ph.D., D.Sc., Senior Lecturer in Physiology, and W. Robson, Ph.D., D.Sc.,

Reader in Biochemistry, University of London, King's College. Thirteenth edition. London, New York, Toronto : Longmans, Green & Co., 1936. (Pp. xii + 350, with 56 figures, including 1 coloured plate.)   9s.

Laboratory Experiments in Physiological Chemistry. By Arthur K. Anderson, Ph.D., Professor of Physiological Chemistry, The Pennsylvania State College. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1936. (Pp. viii + 224, with 20 figures.)   7s. 6d. net.

Water Purification Control. By Edward S. Hopkins, Principal Sanitary Chemist, Bureau of Water Supply, Baltimore, Maryland. Second edition. London : Baillière, Tindall & Cox, 1936. (Pp. x + 184, with 48 figures and 17 tables.)   8s. net.

Principles of Structural Geology. By Charles Merrick Nevin, Professor of Geology, Cornell University. Second edition. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1936. (Pp. xii + 348, with 163 figures.)   17s. 6d. net.

La Géologie et les Mines des Vieilles Plateformes. By F. Blondel, Ingénieur en chef des Mines. Publications du Bureau d'Études Géologiques et Minières Coloniales. Paris : Société d'Éditions Géographiques, Maritimes et Coloniales, 1936. (Pp. 303, with 59 figures.)   Frs. 36.–.

Communications presentées à la Section de Géologie appliquée du Congrès international des Mines, de la Métallurgie et de la Géologie appliquée (VIIᵉ session, Octobre 1935). Vols. I and II. Paris : Section de Géologie appliquée, 1936. (Pp. viii + 1088, illustrated.)   Frs. 120.–.

Interpretative Petrology of the Igneous Rocks. By Harold Lattimore Alling, Ph.D., Professor of Geology, University of Rochester. New York and London : McGraw-Hill Publishing Company, Ltd., 1936. (Pp. xvi + 353, with 59 figures and 65 tables.)   24s. net.

Memoir on Fossils of the Late Pre-Cambrian (Newer Proterozoic) from the Adelaide Series, South Australia. By Sir T. W. Edgeworth David, K.B.E., C.M.G., D.S.O., M.A., D.Sc., Sc.D., F.R.S., and R. J. Tillyard, M.A., Sc.D., D.Sc., F.R.S., F.R.E.S., F.R.S.N.Z., C.M.Z.S. Sydney : Angus & Robertson, Ltd., in conjunction with the Royal Society of New South Wales, 1936. (Pp. xii + 122, with 14 plates, 1 figure and 3 tables.)   7s. 6d.

Rutley's Elements of Mineralogy. Twenty-third edition. By H. H. Read, D.Sc., A.R.C.Sc., Professor of Geology, University of Liverpool. London : Thomas Murby & Co. ; New York : D. van Nostrand Co., 1936. (Pp. viii + 490, with 128 figures.)   8s. net.

An Introduction to the Principles of Plant Physiology. By Walter Stiles, M.A., Sc.D., F.L.S., F.R.S., Mason Professor of Botany in the University of Birmingham. London : Methuen & Co., Ltd., 1936. (Pp. viii + 615, with 5 plates and 55 figures.)   27s. 6d. net.

Citrus Diseases and Their Control. By Howard S. Fawcett, Professor of Plant Pathology, Graduate School of Tropical Agriculture and Citrus Experiment Station, University of California. Second edition. McGraw-Hill Publications in the Agricultural and Botanical Sciences. New York and London : McGraw-Hill Publishing Co., Ltd., 1936. (Pp. xvi + 656, with 187 figures, including 15 plates.)   36s. net.

Diseases and Pests of the Rubber Tree. By Arnold Sharples, A.R.C.Sc., D.I.C., late Head of the Pathological Division, Rubber Research Institute of Malaya, and formerly Government Mycologist, Department of Agriculture, S.S. and F.M.S. London : Macmillan & Co., Ltd., 1936. (Pp. xviii + 480, with frontispiece, 4 coloured plates, 70 figures and 11 diagrams.) 25s. net.

Growth Hormones in Plants. Authorised English Translation of " Die Wuchsstofftheorie und ihre Bedeutung für die Analyse des Wachstums und der Wachstumsbewegungen der Pflanzen." By P. Boysen Jensen, Professor of Plant Physiology, University of Copenhagen. Translated and revised by George S. Avery, Jr., and Paul R. Burkholder, with the collaboration of Harriet B. Creighton and Beatrice A. Scheer, Department of Botany, Connecticut College. McGraw-Hill Publications in the Agricultural and Botanical Sciences. New York and London : McGraw-Hill Publishing Co., Ltd., 1936. (Pp. xiv + 268, with 64 figures and 16 tables.) 21s. net.

Fifty Years of Field Experiments at the Woburn Experimental Station. By Sir E. John Russell, D.Sc., F.R.S., and Dr. J. A. Voelcker, C.I.E., M.A. With a Statistical Report by W. G. Cochran, B.A., Rothamsted Statistical Department. The Rothamsted Monographs on Agricultural Science. London, New York, Toronto : Longmans, Green & Co., 1936. (Pp. xviii + 392, with 4 plates, 42 figures and 146 tables.) 21s. net.

English Farming, Past and Present. By the Rt. Hon. Lord Ernle, P.C., M.V.O., Hon. D.C.L.(Oxon), Hon. Fellow of Balliol College. Fifth edition. Edited by Sir A. D. Hall, K.C.B., M.A., F.R.S., Director, John Innes Horticultural Institution. London, New York, Toronto : Longmans, Green & Co., 1936. (Pp. xvi + 559.) 15s. net.

Rapports et Procès-Verbaux des Réunions du Conseil Permanent International pour l'Exploration de la Mer. Vol. CI : Report of the Proceedings of the Special Scientific Meetings held in May 1936 at Copenhagen. Parts 1, 2 and 3. Copenhagen: Andr. Fred. Høst & Fils, 1936. Part 1 (pp. 14) Kr. 1.00 ; Part 2 (pp. 63, illustrated) Kr. 3.00 ; Part 3 (pp. 90, illustrated) Kr. 4.00.

Forest Insects. A Textbook for the Use of Students in Forest Schools, Colleges, and Universities, and for Forest Workers. By R. W. Doane, Professor of Biology, Stanford University ; E. C. Van Dyke, Professor of Entomology, University of California ; W. J. Chamberlin, Professor of Forest Entomology, Oregon State College ; and H. E. Burke, formerly Senior Entomologist, U.S. Department of Agriculture. American Forestry Series. New York and London : McGraw-Hill Publishing Co., Ltd., 1936. (Pp. xii + 463, with frontispiece and 234 figures.) 25s. net.

Salmon of the River Nith—1935. By P. R. C. Macfarlane, B.Sc., Assistant Inspector of Salmon Fisheries of Scotland. Fishery Board for Scotland, Salmon Fisheries, 1936, No. III. Edinburgh : H.M. Stationery Office, 1936. (Pp. 16, with 7 tables.) 9d. net.

Notes on Sea-Trout from the Dee Tidal Nets, Aberdeen, 1935. By G. Herbert Nall, M.A., F.R.M.S. Fishery Board for Scotland, Salmon Fisheries,

1936, No. IV. Edinburgh : H.M. Stationery Office, 1936. (Pp. 22, with 4 plates.) 1s. 6d. net.

Neuroembryology. An Experimental Study. By Samuel R. Detwiler, Professor of Anatomy, College of Physicians and Surgeons, Columbia University. Experimental Biology Series. New York and London : Macmillan & Co., Ltd., 1936. (Pp. x + 218, with 107 figures and 9 tables.) 16s. net.

Inheritance and Evolution. By W. F. Wheeler, M.A., Senior Biology Master, King Edward VII School, Sheffield. With a Foreword by E. B. Ford, M.A., B.Sc. London : Methuen & Co., Ltd., 1936. (Pp. xii + 116, with 39 figures.) 3s.

The Student's Manual of Microscopic Technique. With Instructions for Photomicrography. By J. Carroll Tobias. London : Chapman & Hall, Ltd., 1936. (Pp. xviii + 210, with 79 figures.) 10s. 6d. net.

Biology and the New Physics. A Plea for a Consistent Philosophy of Life. By C. J. Bond, C.M.G., F.R.C.S., F.L.S., Fellow of University College, London. London : H. K. Lewis & Co., Ltd., 1936. (Pp. 67.) Cloth, 2s. 6d. net ; paper covers, 1s. 6d. net.

Bones. A Study of the Development and Structure of the Vertebrate Skeleton. By P. D. F. Murray, M.A., D.Sc. Cambridge : at the University Press, 1936. (Pp. x + 203, with 45 figures, including 8 plates.) 8s. 6d. net.

The Principles of Bacteriology and Immunity. By W. W. C. Topley, M.A., M.D., M.Sc., F.R.C.P., F.R.S., Professor of Bacteriology and Immunology, University of London, Director of the Division of Bacteriology and Immunology, London School of Hygiene and Tropical Medicine, and G. S. Wilson, M.D., F.R.C.P., D.P.H., Professor of Bacteriology as applied to Hygiene, University of London, London School of Hygiene and Tropical Medicine. Second edition. London : Edward Arnold & Co., 1936. (Pp. xvi + 1645, with 276 figures and 192 tables.) 50s. net.

Nutritional Factors in Disease. By William Robert Fearon, M.B., Sc.D., F.I.C. Monographs of Medical and Surgical Science. London : William Heinemann (Medical Books), Ltd., 1936. (Pp. xiv + 141, with 9 tables.) 7s. 6d. net.

Food and the Principles of Dietetics. By Robert Hutchison, M.D., LL.D., F.R.C.P., Consulting Physician to the London Hospital and to the Hospital for Sick Children, Great Ormond Street, and V. H. Mottram, M.A., Professor of Physiology at King's College of Household and Social Science, University of London. Eighth edition. London : Edward Arnold & Co., 1936. (Pp. xxviii + 634, with 32 figures.) 21s. net.

Absorption from the Intestine. By F. Verzár, Professor of Physiology of the University of Basle. Assisted by E. J. McDougall, Ph.D. Monographs on Physiology. London, New York, Toronto : Longmans, Green & Co., 1936. (Pp. xii + 294, with 70 figures, including 6 black and white and 6 coloured plates.) 21s. net.

The Improvement of Sight by Natural Methods. By C. S. Price, M.B.E., D.O., late Captain Australian Army Medical Corps, Principal of the

School of Eyesight Training (London). Second edition. London : Chapman & Hall, Ltd., 1936. (Pp. xii + 240, with 30 figures.) 5s. net.

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SCIENCE PROGRESS

THE BEILBY LAYER

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I SUPPOSE that man's interest in polishing was first aroused by the discovery that the blade of a cutting or piercing weapon was much more efficient and durable when it had been carefully worked and smoothed. It also looked better, and man's innate love of adornment was doubtless an added incentive to practise the new art. It was not until the invention of the telescope, however, that any serious enquiry seems to have been made into the real nature of the changes involved in converting a rough, dull surface into a smooth reflecting layer. As telescopes and other optical instruments, such as microscopes, prisms, spectacles, etc., were developed and brought more and more into use, the art of polishing rapidly improved to such an extent that by the end of the nineteenth century the skilled craftsman made surfaces which appeared perfectly smooth and free from fine scratches, pits or slight waviness, even when tested by such delicate methods as those depending on light interference.

In Galileo's time the polishing of glass was a matter of grinding to the required shape with a coarse material such as sand, followed by finer and still finer abrasive powders. Campana who succeeded him as a maker of telescopes made object glasses up to 3 or 4 inches in diameter and discovered a method of polishing which no one else could then equal, so that for a long time he was the leading maker of telescopes in the world. After his death the secret of Campana's success was found to lie in the fact that, after roughly shaping the lens by grinding, he lined the roughing tool with paper, thus providing a yielding support for the rouge used in the final polishing. Later, scientists experimented in the making of their own lenses ; Robert Hooke, for example, in his *Micrographia* (1667) describes how he made some microscope objectives

by drawing out a piece of broken Venetian glass into a thin thread and holding this in a flame until a bead had formed at the end. On the bead he ground a flat surface with a whetstone and polished it on a smooth metal plate with " tripoli " powder. But even at the time when Sir Isaac Newton was studying the properties of light, polished surfaces were so far from perfect that he waxed enthusiastic about some " magnificent " and " absolutely priceless " prisms obtained from abroad, although from his description their surfaces were still much pitted. He evidently made experiments on the polishing of his optical surfaces, since he remarks on the polishing of mirrors, and appears to have been the first to make the important advance of using pitch as a backing or support for the abrasive used in the final process. So great was the advantage of a pitch backing that its use soon became widespread ; thus Herschel (the elder) in 1774 used a pitch polisher for telescope speculum metal mirrors, some of which were so large that several men or even a special machine were required to handle the polishing tool.

In making a polished article one begins with a block or moulded shape of the desired material. This is first ground or machined to the rough shape required ("roughing " or " turning ") then finer and finer abrasives are used or machine cuts taken until the surface is smooth ("smoothing," "fine cut " or " honing "). Finally comes the more delicate process of polishing or burnishing. Burnishing is usually confined to metals and is effected by rubbing with a smooth hard metal tool, generally of steel. In polishing, a fine abrasive such as rouge is spread over a backing of pitch or some other resinous yielding material which roughly fits the surface to be polished and is supported in a so-called polishing tool. The fine abrasive particles become embedded in this soft surface when a rotatory motion is given to the " tool " in contact with the " smoothed " surface, so that they exert a polishing rather than the coarser scratching action which would result if the backing were so unyielding as to force the hard rouge grains to cut or break through obstructions. The abrasives used in " roughing " and " smoothing " are crushed and sieved minerals of various degrees of fineness, such as quartz (sand), corundum (emery), garnet, diamond, etc., which may be in loose powder form, or bonded with cement to make wheels or hones, or glued to cloth or paper. For polishing, fine-grained, hard powders such as alumina (aluminium oxide), rouge (iron oxide), magnesia, putty powder (tin oxide), chromium oxide, etc., are used ; their hardness depends largely on the method of their preparation. Thus " jeweller's rouge " is made by calcining

ferrous sulphate, whilst the exceedingly fine powder of ferric oxide obtained by collecting the "smoke" from an iron arc has been found to be quite useless as a polishing agent.

The earliest authoritative view on the nature of the polished surface appears to be due to Newton [1] : "For in polishing Glass with Sand, Putty or Tripoly, it is not to be imagined that those Substances can, by grating and fretting the Glass, bring all its least Particles to an accurate Polish; so that all their Surfaces shall be truly plain or truly spherical, and look all the same way, so as together to compose one even Surface. The smaller the Particles of those Substances are, the smaller will be the Scratches by which they continually fret and wear away the Glass until it be polish'd; but be they never so small they can wear away the Glass no otherwise than by grating and scratching it, and breaking the Protuberances ; and therefore polish it no otherwise than by bringing its roughnesses to a very fine Grain, so that the Scratches and Frettings of the Surface become too small to be visible." Thus Newton did not suggest that the surfaces are comparable in smoothness with the surface of a liquid. Subsequent writers accepted this view ; Coddington, [2] John Herschel (the younger) [3] and David Brewster, for example, all used practically the same words as Newton in describing the nature of polish.

Lord Rayleigh, [4] in a lecture on "Polish" at the Royal Institution in 1901, seems to have been the first to notice that, in polishing with a pitch backing, patches are formed on the polished surface which under the microscope appeared quite structureless apart from accidental scratches and flaws ; and he thought that the final stage of the process consisted in rubbing down the whole surface to the level of the deepest pits. Thus he says, " The mere fact that no structure can be perceived does not of itself prove that pittings may not be taking place of a character too fine to be shown by a particular microscope or by any possible microscope. But so much discontinuity as compared with the grinding action, has to be admitted in any case, that one is inevitably led to the conclusion that in all probability the operation is a molecular one, and that no coherent fragments containing a large number of molecules are broken out. If this be so, there would be much less difference than Herschel thought between the surfaces of a polished solid and of a liquid." By weighing, Rayleigh found that the thickness worn away, or rather the difference in level between the original mean surface and the final plane, was about $3\cdot6 \times 10^{-4}$ cm., or about six mean wavelengths of light ; but he stated that, since polish on this glass was by no means perfect,

about ten wavelengths would normally be removed in polishing. He also found that polishing a particularly finely ground glass surface lowered it by some two or three wavelengths, but goes on to note that, " It may be well to emphasise that the observations here recorded relate to a *hard* substance. In the polishing of a soft substance such as copper it is possible that material may be loosened from its original position without becoming detached. In such a case pits may be actually filled in, by which the operation would be much quickened. Nothing suggestive of this effect has been observed in experiments on glass."

About the same time, in 1901, the late Sir George Beilby [5] began a remarkable series of studies on the effects produced when solid surfaces are disturbed in various ways, in particular by grinding, polishing and etching. Equipped with a good microscope, in the use of which he had first made himself thoroughly proficient, he examined closely the changes effected by polishing the surface layers of a wide variety of substances. His experiments, simple though they were, led to many striking results and threw an entirely fresh light on the nature of polish. The conception of the polish layer as being the result of a flowing of the material, whereby the surface becomes coated with an amorphous or glass-like layer, is due very largely, if not entirely, to Beilby. After twenty years, in 1921, he published a connected account of his many experiments in a book called *Aggregation and Flow of Solids*, which deserves to find an honoured place in the classic literature of Science. By general consent Beilby's name is now associated with the flowed layer formed by polishing.

Although the idea of a smearing action during grinding or polishing was readily acceptable in the case of soft and ductile metals, it was at the time generally regarded as almost unbelievable that this could occur on hard and brittle substances. Yet Beilby demonstrated that materials. like speculum metal, antimony, glass and quartz showed unmistakable signs of such a smearing or flowing of the surface layer when being polished. Thus speculum metal, an alloy of copper and tin, is so hard and brittle that it cannot be worked with ordinary tools ; in some states it is as brittle as glass and can easily be shattered with a hammer, yet it is a remarkably easy substance to polish. Beilby found that the effect of polishing with rouge at right angles to a fine needle scratch on a speculum metal surface was to cause a flowing of the surface, so as to fill up and hide the scratch, the final polish being so excellent that no detail could be seen in the microscope, apart from the outlines of crystal grains below the surface.

That the ease of flowing of speculum metal did not depend on the presence of the eutectic alloy in which the crystals were embedded was clearly shown by similar experiments repeated with a well-formed crystal of antimony, also a brittle metal prone to bad splintering when scratched. Polishing across grinding scratches with rouge on a wash-leather backing resulted in a roofing over or filling of the furrows as though with a viscous liquid until a very smooth surface resulted. Etching with a potassium cyanide solution removed this surface skin and again revealed the scratches, thus showing that they had been covered over or filled in, and not simply removed by a wearing away of the surface to the level of the valleys. By gentle polishing with fine linen stretched over a hard flat surface and moistened with one of the ordinary commercial brass polishing liquids, Beilby was further able to show that gas bubble pits in a copper surface became covered with a copper film, so thin as to be translucent, and that many fine scratches were obliterated by their being filled with flowed copper.

Perhaps even more striking were Beilby's experiments with crystals, such as calcite, where he found that polishing caused a flowing of the surface with surprising ease. Etching a fresh cleavage face with dilute hydrochloric acid revealed no new structure or markings ; yet if such a face were scratched and then polished until all signs of the scratches had been removed, etching once more revealed the original scratches. Beilby observed that the polish layer was harder and more resistant to abrasion than an unpolished surface. He also found that sodium nitrate crystals grown on a polished calcite cleavage face were orientated, *i.e.* lined up, or, as Newton would say, " faced the same way," in a direction determined by the calcite crystal structure, and explained this as due to an orientating influence of the underlying crystal exerted through the polish layer. He evidently did not suspect that the polish layer, the flowing of which he had demonstrated so clearly, had in any way changed from the amorphous state on cessation of the polishing action. In further experiments with glass and quartz he detected a similar though not quite so extensive surface flowing with polishing, leading to the obliteration of scratches which reappeared, however, after etching the polished surface with hydrofluoric acid.

Many other similar results convinced Beilby that, whenever the texture of a surface was altered by polishing, an amorphous glass-like layer was built up by the smearing over the surface of material caused to flow in an amorphous state by rubbing. Thus he postulated the formation of an amorphous layer whenever two crystal

faces are rubbed together, and explained the hardening of metals produced by cold working as being due to the formation between sets of slip planes of a tough amorphous cementing layer inhibiting further slip. It was this extension of Beilby's theory which attracted most attention and led to its being so vigorously attacked, particularly by continental workers, that doubt was also thrown upon the validity of his conclusions as to the amorphous nature of flow and of the polish layer in general; so that since Beilby's time others, notably J. W. French [6] and F. W. Preston [7] returned to the study of glass polishing, which plays so important a part in the ultimate efficiency of optical instruments. French confirmed that in grinding glass, abrasion results in a splintering or shearing action with the production of conchoidal fractures, and by a microscopic examination came to the same conclusion as Beilby, namely, that polishing caused a flowing of the surface layer. On the other hand, Preston in 1922, while agreeing that grinding causes a conchoidal fracturing and removal of material in scratches or furrows, thought that by polishing these were merely levelled down through fine abrasion by the polishing agent, and in support of this view he drew attention to the Twyman effect. The Twyman effect is the bending of a thin piece of glass " greyed," *i.e.* " smoothed " on both sides, which occurs as soon as one of the sides is polished. Preston thought that this was due to small glass fragments being forced into the grinding scratches and acting like wedges. On the whole, Preston inclined to the view that the polishing of glass was really a matter of ultra-miscroscopic abrasion and not a molecular or even nearly molecular effect.

In recent years the increasing interest taken by the engineer in the structural changes involved in the " running-in " of bearing surfaces in machines, particularly in the internal combustion engine, has provided a fresh stimulus to the study of the nature of polish. At the same time remarkable developments in modern physical theory have placed in our hands a new and extraordinarily powerful method of examining the structure of surfaces. Soon after the War it was realised that X-rays, light, heat and wireless rays, long known to be electromagnetic wave phenomena and differing from each other only in wavelength, could sometimes behave like swiftly moving particles; a duality in behaviour which was quite incomprehensible to the physicist of little more than a decade ago. By one of those sparkling flights of genius, so characteristic of Gallic wit and invention, Louis de Broglie [8] was led to suggest that any and all moving particles should also sometimes behave as if they had systems of waves associated with them. The simplest

way of putting this bold idea to the test of experiment was to see if electrons would move through the interstices of a natural crystal as if they were waves. The experiment was tried by C. J. Davisson and L. H. Germer [9] in the United States and by G. P. Thomson and A. Reid [10] in Aberdeen. It came off ! De Broglie was right ; a stream of high-speed electrons fired through films consisting of minute crystals of gold and other metals were diffracted, *i.e.* deflected in different directions, just as if they had been X-rays of very short wavelength ; and yet they were definitely not X-rays, which might perhaps have been supposed to have been generated by the impact of the electrons on the gold, because they could be deflected by a magnetic field just as easily after as before passing through the gold film. Now, since the way in which the gold atoms are arranged in gold crystals had long since been discovered by X-rays, Thomson [11] was able to test the relationship, which de Broglie had predicted as holding between the kinetic energy of the electrons in his beam and the wavelength of their associated wave systems, by measuring the angles through which the electron beam was deflected ; and finding this relationship accurately true, he was quick to realise that the phenomenon of the diffraction of electrons provided the scientific worker with a new method of discovering the atomic arrangements in unknown structures.

The utility of many things depends so much more on surface properties than upon their internal structure that the need has long been felt for some means, more searching than the microscope, which would enable the submicroscopic details of surface structure and atomic arrangement to be studied. Owing to their high powers of penetration, anything that X-rays might tell us about the surface of a massive body is hopelessly hidden and submerged in the flood of detailed information which they yield about the structure beneath the surface. But electron diffraction shows this surface structure, because the depth of penetration of even swiftly moving electrons is so slight, that any information obtained in studying their diffraction by solid masses of material must necessarily be confined to a revealing of the structure to a depth of only a few atoms.

The surface to be examined, or rather the thin surface layer of a depth to which fast electrons can penetrate without loss of energy, may be available either as a detached film or *in situ* on the massive supporting body or substrate of which it forms part. In the former case, a fine pencil or beam of electrons travelling at a uniform speed approaching half that of light is fired through the

film. The directions of the diffracted rays are recorded photo-graphically and are determined by the easily measured speed of the electrons and by the way the atoms are arranged within the film. When the surface film cannot readily be detached from the substrate, it is examined with the beam just grazing the surface. In this case the shadow thrown by the massive impenetrable sup-port obscures rather more than half the diffraction pattern, but in transmission the whole pattern surrounding the main electron beam is visible.

Thomson [12] was the first to put the new technique to good use in studying the structure of polish. With R. C. French [13] he found that metal surfaces which, being normally crystalline, diffracted the electrons into a series of sharp rings just like the rings of the well-known X-ray powder diffraction pattern, gave, after polishing, patterns of faint diffuse halos (Plate II, Fig. 6) such as are obtained by either X-rays or electrons from liquids like mercury and amor-phous materials like glass. It was naturally concluded that these facts pointed to the surface layer of a highly polished metal as being amorphous, that is glass-like, in the sense that the atoms are not regularly arranged as in the crystalline state but jumbled together just as they would be in a suddenly solidified liquid. A few months later, however, F. Kirchner [14] showed that it was possible to prepare crystalline metal surfaces which, owing to the very small height of crystal which could be penetrated by the electron beam, gave ill-defined rings or halos indistinguishable from those which Thomson and French had obtained from polished metal surfaces. This result led Kirchner to the view that polishing is a matter of rubbing away crystalline projections and is thus a mere levelling of the surface by abrasion, a view which is essentially similar to that put forward by Newton and the modern antagonists of Beilby's ideas. About this time other workers appeared in the field, of whom some ranged themselves on one side and some on the other, a position of stalemate which continued until it was discovered in the laboratories of Applied Physical Chemistry at the Imperial College [15] that the polish layer on metals has the remarkable property, not possessed by the corresponding crystalline metal surface, of being able to dissolve crystals of a foreign metal at room temperature. The experiments consisted of depositing a film of tiny crystals of a metal on the cool surface of some other metal which was swept by a beam of electrons. While the crystalline film was being formed, the way in which the electrons were diffracted by the crystals was observed on a fluorescent screen, and any changes taking place in the diffraction phenomena could be closely

PLATE I

B   A

FIG. 1.—Electron diffraction patterns showing the dissolution of zinc crystals in a polished copper surface.

A — One second after deposition of zinc ; the pattern is characteristic of a well-formed crystalline zinc layer.

B — Two seconds later ; the sharp rings indicating crystal structure have practically disappeared, showing that the zinc crystals have gone into solution.

FIG. 2.—Pattern from a conchoidal fracture surface of a single crystal of sapphire.

FIG. 3.—Pattern from the recrystallised polish layer on sapphire. The diffuseness of the lines as compared with the definition of Fig. 2 shows that the orientation of the recrystallised layer is slightly imperfect.

FIG. 4.—Pattern from a conchoidal fracture of spinel. The electron beam was nearly parallel to a cube edge. The cubic symmetry of the spinel crystal is beautifully revealed by the intersection at 45° of the main sets of bands.

PLATE II

FIG. 5.—Pattern obtained from an imperfectly polished spinel. The Beilby layer is so thin that the spots produced by crystalline peaks rising up into the layer are still partly visible.

FIG. 6.—Diffuse halo pattern typical of the Beilby layer on well-polished spinel, and on many other surfaces.

FIG. 7.—Pattern obtained 'from silicon carbide after removal of the amorphous skin which otherwise so obscures the crystal surface that only halos are obtained.

FIG. 8.—Pattern from silicon carbide showing faintly through the halo pattern due to a very thin amorphous skin formed by mild oxidation in a Bunsen flame.
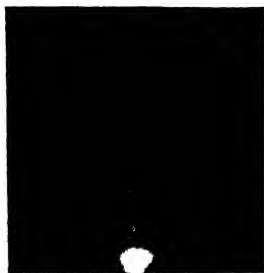
FIG. 9.—Pattern from polished cast iron. The two very diffuse halos are due to the cast-iron Beilby layer, and the vertical row of faint, rather broad streaks is characteristic of graphite crystals orientated with their slip planes parallel to the surface.

followed. The metal surface or substrate was either highly polished or consisted of a mass of minute crystals. It was found that a film of zinc crystals freshly deposited upon a polished copper surface at first gave rise to a brilliant and well-defined electron diffraction pattern characteristic of crystalline zinc ; but that this pattern quickly faded until, after a few seconds, it had completely disappeared, thus showing that, somehow or other, the crystalline structure of the zinc film had been destroyed (Plate I, Fig. 1). In one experiment, twelve successive layers of zinc crystals were deposited on a polished copper surface, and with each layer except the last the diffraction pattern vanished at a rate decreasing with each successive layer. Thus the pattern yielded by the last layer but one was still faintly visible four minutes after deposition, but had completely vanished after five minutes, whilst, after an initial period of weakening, the pattern from the last layer remained unchanged, even after four hours. Evidently the crystalline zinc layers had been dissolved by the polished copper surface and had formed a solution which, with the last zinc layer, had finally become saturated. On the other hand, a single zinc film deposited on a crystalline copper surface gave a pattern which was permanent, in that even after one and a half hours it showed neither loss in brilliancy nor any other change in appearance. Similar results were obtained with zinc, lead, silver and tin crystalline layers deposited on surfaces of copper, iron, zinc, lead and gold ; if the surface had been polished, the crystals in the deposited layer disappeared, but they remained permanently on crystalline substrates. In these experiments where the diffraction patterns of a crystalline metal film, freshly deposited on a cool, polished surface of some other metal, were seen to fade, it was as if we had been observing through a sort of super-microscope the solution of crystals floating on the surface of, and being dissolved by, a liquid. Thus this discovery of the remarkable solvent powers of the polish layer, a property which is not shared by the corresponding crystalline surface, shows that the polish layer on metals is liquid-like in character and is therefore in all probability amorphous.

The obvious corollary that the surface flow induced by polishing, and the occurrence of which had been so clearly demonstrated in Beilby's experiments, was likewise one of material rendered amorphous by the action of polishing, received independent support from a series of experiments carried out by F. P. Bowden and K. E. W. Ridler [16]. When two different metals are joined together in a closed circuit and one junction is hotter than the other an electric current, the so-called " thermoelectric

current," flows; and by measuring this current the difference in temperature between the two junctions can be determined. In this way Bowden and Ridler were able to measure the temperature of formation of the Beilby layer on a metal surface whilst it was being burnished, that is polished, by friction against another metal. They found that the temperature of the polish layer quickly rose during its formation to a limit equal to the melting point of the metal and was independent of any further increase in the vigour of the polishing action. Thus this result suggests that the material which flows during polishing can hardly consist of broken-off crystalline fragments, but must be liquefied, or semi-liquid, and therefore amorphous matter which is smeared over the surface.

It is natural to suppose that the depth of the polish layer or, as we may now well term it, the Beilby layer is influenced by the vigour of the polishing action, and several observations have been made which confirm this. H. G. Hopkins [17] and C. S. Lees [18] found that the thickness of the Beilby layer on lightly hand-polished gold and copper surfaces was about 30 Å., one Ångstrom being equal to 10^{-8} cm. Thus, since the average approximate diameter of an atom is roughly between 1 and 2 Å., the Beilby layer produced by light polishing is only about 15 to 30 atoms deep. A piece of cigarette paper is about one-thousandth of an inch in thickness and would therefore be about a quarter of a million atoms thick. It is not easy to demonstrate the solution of zinc crystals in such a thin Beilby layer as that formed by a light hand-polishing of a copper surface, [19] but the effect is very striking when the polishing is carried out by means of a vigorous machine buffing, and this fact shows that the depth of the Beilby layer depends upon the way in which polishing is carried out. Again, in these laboratories [20] we examined the working surfaces of some aeroplane engine cylinder sleeves. The freshly honed, unused sleeves had, as was easily seen in the electron diffraction camera, a crystalline structure, but after they had been "run-in" we found that the working surfaces were covered with a Beilby layer of steel which was so thick that several rubbings with a fine emery paper were necessary to break through the layer and thus to expose the underlying crystalline structure; though with a lightly hand-polished specimen of the same steel a single light stroke with fine emery paper easily cut right through the Beilby layer. Thus we see that the essence of the process of "running-in" an engine is a sort of vigorous polishing action which leads to the formation on the working surfaces of an exceptionally deep Beilby layer of amorphous metal.

The chief reasons why a Beilby layer should be formed on bearing surfaces before these are subjected to heavy loads are twofold. Firstly, the Beilby layer is harder and generally tougher than the corresponding crystalline surface. Also, freshly machined or honed surfaces are far from being smooth, but have many sharp little crystalline peaks projecting above the mean level of the surface. These give rise to high localised stresses because the load between the bearing surfaces is concentrated on a few almost point-like areas, instead of being more or less evenly distributed over a wide area. The result is that the oil film, an important function of which is to prevent actual metal-to-metal contact, is easily pierced and the high temperatures generated by the ensuing metallic point-to-point friction result in a localised melting and welding together of the two surfaces which, under severe conditions of loading of freshly machined surfaces, leads to "pick-up," *i.e.* the tearing of metal from one surface by the other. When "running-in" is effected under conditions of light load, however, the crystalline projections are caused to flow without appreciable "pick-up" occurring, and the resulting amorphous material is spread over the surface, filling up the hollows and thus tending to form smooth surfaces between which point contact is replaced by contact over wide areas. The consequent far more even distribution of load permits of a proportionately heavier total loading of the bearing as a whole.

The experiments which led to Beilby's discovery of the surface flow produced by polishing had been carried out not only with metals, but also with a variety of non-conducting crystals such as quartz, fluorspar and particularly, as mentioned above, with calcite ; and he had come to the conclusion that the polish layer is amorphous, not only on metals but also on non-metals as well. Indeed, the phenomenon of surface flow is particularly easily demonstrated with calcite. For example, a calcite crystal can be readily cleaved, and one can inscribe with a sharp needle a cross or some other easily recognised figure on such a cleavage surface. If this surface be now carefully polished, all signs of the scratched figure will completely disappear, but on etching the polished surface with dilute hydrochloric acid the figure will reappear, thus showing clearly that polishing has caused material to flow over the surface and fill up the irregularities. It came therefore as a great surprise when H. Raether [21] and H. G. Hopkins [22] found by electron diffraction that the structure of the polish layer on a calcite cleavage face was not amorphous, as had been supposed by Beilby, but was crystalline and so perfectly orientated as to form in effect an integral part of the main calcite single crystal. Since the occurrence

of surface flow proves that the polishing of calcite is not merely a rubbing away of projections, this discovery raised the question as to whether the single crystal structure of the final polish layer is due to a recrystallisation of surface-flowed amorphous material in line with the crystal structure of the main underlying calcite crystal, or whether we have to deal with an ordered deposition of crystalline fragments or sheets transferred from one part of the polished surface to another.

The key to this problem was supplied by experiments recently carried out in these laboratories, [23] in the course of which it was found that, though the polish layer was crystalline on the cleavage face of calcite, it became less and less crystalline, the more inclined the polished surface was to a cleavage plane, until, on surfaces remote in direction from all cleavage planes, the polish layer was wholly amorphous. Thus these results showed that the surface flow was one of amorphous material which, when the polishing action had ceased, immediately recrystallised if the polished surface was a cleavage plane, but tended to remain amorphous on surfaces steeply inclined to any such plane. Further experiments showed that whether the Beilby layer recrystallised or not depended upon forces exerted by the underlying crystal surface. Thus the amorphous Beilby layer formed by polishing on a calcite surface steeply inclined to all cleavage planes was found to recrystallise when heated, and the diffraction patterns changed in such a way as to show that recrystallisation proceeded from inside the Beilby layer towards the outer surface.

It was the fortunate discovery in these laboratories [24] of a remarkable chemically formed Beilby layer which afforded the clue to the correct interpretation of the changes occurring in electron diffraction patterns when the amorphous Beilby layer on calcite crystals is heated and thereby caused to crystallise. The natural and cleavage faces of single crystals give electron diffraction patterns consisting of spots, and black and white lines. Quite apart from the amazing wealth of information which these patterns disclose about the atomic structure of such surfaces, they reveal a beauty of line and shade most satisfying to the artistic eye. Natural crystal faces are, as a rule, relatively rough and imperfect and are rarely, if ever, even microscopically smooth. This explains why, when Germer [25] found that the quite exceptionally smooth and beautifully formed surfaces of silicon carbide crystals only gave the diffuse halo pattern which we now associate with the amorphous state, he thought that this was in some way due to the extraordinary smoothness of these crystals ; and he felt confirmed in

the correctness of this view when he obtained diffraction patterns characteristic of a single crystal surface from the carbide crystal after it had been roughened by etching in a corrosive bath of molten caustic potash. By this time, however, we in this laboratory had become so used to regarding the halo pattern as indicative of an amorphous layer that it seemed to us that some other explanation must be sought to account for the halo pattern obtained from an unetched silicon carbide crystal. Now, silicon carbide is made by heating sand and coke to a high temperature until a reaction takes place to form the carbide. The product is then allowed to cool, and it occurred to us that, on cooling, air would be drawn into the furnace and might thus oxidise some of the carbide. If this were so, then silica would be formed as the solid product of combustion, and it seemed reasonable to suppose that the carbide crystals might well be covered with a thin film of silica with an amorphous structure like glass. The matter was easily put to the test of experiment. We found that if a silicon carbide crystal giving a halo pattern was rubbed on emery paper, which is too soft to have any effect on the carbide but easily scratches silica, no visible effect was produced ; nevertheless, the crystal now gave a beautiful pattern characteristic of single crystal structure (Plate II, Fig. 7). In other experiments carbide crystals were treated with liquids such as hydrofluoric acid or caustic soda solutions which have no effect whatever on the carbide, but are known to attack and dissolve silica ; and here again it was found that after this treatment carbide crystals which had previously given halo patterns, now yielded the lines and spots typical of the crystal structure of silicon carbide. It seemed evident from these results that the halo patterns obtained from untreated carbide crystals were due to a thin skin of amorphous silica formed as a result of superficial oxidation occurring while the silicon carbide charge cooled in the furnace during manufacture. This view was confirmed by further experiments in which carbide crystals previously treated to remove the silica skin were slowly oxidised by heating in air (Plate II, Fig. 8). It was seen that, as oxidation progressed, the single crystal pattern gradually became submerged in, finally to give way to, the halo type of pattern (Plate II, Fig. 8). Hence, when we found that progressive heating of the Beilby layer on a calcite surface remote from any cleavage plane brought about exactly the reverse sequence of pattern changes, it was at once clear that crystallisation must have proceeded from inwards and towards the outer surface of the amorphous layer and was thus governed by forces exerted by the underlying calcite crystal.

Recently we[26] have examined a wide variety of polished non-metallic single crystal surfaces with results which are not only of theoretical interest but also promise to be of immediate practical value. Thus we have found that whilst polished surfaces cut in quite arbitrary directions from many types of crystal, such as quartz, diamond, sapphire (Plate I, Figs. 2 and 3), garnet, chrysoberyl, epidote, olivine, sphene and others, yield electron diffraction patterns of spots and lines characteristic of a crystalline polish layer integral with the underlying crystal, many others, notably beryl, zircon, tourmaline, cassiterite and hæmatite, give halo patterns which show that the polish layer in their case is amorphous. Other polished crystals like brown beryl, moonstone, orthoclase and cordierite give composite patterns in which that due to crystal structure shows faintly through the halos, thus showing that the Beilby layer is so exceedingly thin that some electrons can penetrate without losing energy to the crystalline surface below. Spinels gave either halo patterns (Plate II, Fig. 6), or halos and faint, barely distinguishable spots (Plate II, Fig. 5). Except in the case of the diamond, these results show that where the polish layer is crystalline this is due to a ready and spontaneous re-ordering of the molecules, which have been disarranged by polishing, in strict alignment with those of the underlying crystal; whilst the fact that the polish layer on other crystal faces remains amorphous shows that the flowed layer is reluctant to recrystallise; a reluctance which the forces exerted by the underlying crystal are unable to overcome. The polish layer on diamond appears to stand in a class by itself. This is the one case in which we have never succeeded in finding the slightest trace of a surface flow produced by polishing which would appear in this instance to result in attrition, that is a levelling down and smoothing out of protuberances by simply rubbing them away.

The fundamental difference disclosed by these experiments between the polish layers on sapphire and spinel has, as we shall now see, a significant bearing on the problem of wear in the internal combustion engine cylinder. The high engine speeds aimed at nowadays call for a great reduction in the weight of the reciprocating parts and have thus been responsible for the replacement of the cast-iron piston by aluminium, particularly by the so-called " Y " alloy consisting largely of aluminium with a small proportion of copper. This alloy is not only very light but also conducts heat well and has mechanical properties of strength such as are required in the internal-combustion engine. Unfortunately, although such a piston, in spite of the relative softness of its metal, suffers little or no wear during

its life, the much harder cylinder wall against which it slides is worn away more rapidly than would have been the case with a cast-iron piston. It has been supposed that this is due to the embedding of grit particles in the softer aluminium piston. But like so many plausible explanations this one proves to be incorrect. The aluminium piston is normally kept from actual metal-to-metal contact with the steel or cast-iron cylinder lining by a film of lubricating oil. Of course, when piston and cylinder are new, the surfaces are crystalline and thus have many little peaks projecting above the general level of the surfaces ; but, as we have seen, the " running-in " process smooths these down and covers the surfaces with a Beilby layer, which has no sharp projections to pierce the separating oil film under the conditions of heavy duty. Now, in actual fact, the aluminium piston surface is not metallic, but is covered with a hard thin film of oxide which forms spontaneously on the exposure of any fresh aluminium surface to air and protects it from further oxidation. Indeed, it is quite a common engineering practice to thicken this oxide layer by the process known as anodic oxidation. The oxide layer is amorphous, so that it is already a Beilby layer, and one would expect it to be an admirable surface for a reciprocating part in the engine. We have, however, recently discovered that, when an aluminium piston is " run-in " or polished, this amorphous layer is largely converted into a film of minute, irregularly arranged sapphire crystals. But it will be remembered that the sapphire polish layer is crystalline. Hence " running-in " forms tiny crystalline and exceedingly hard projections of sapphire on the aluminium piston, and these are ready to pierce the lubricating oil film and scour grooves in the cylinder wall. The results of our experiments with non-metallic single crystal surfaces pointed, however, to a remedy for this undesirable state of affairs. Although the sapphire polish layer is crystalline, we had found that on spinel to be amorphous. Spinel is magnesium aluminate with varying amounts of aluminium oxide in solid solution, and differs but little in hardness from sapphire. We found also that a suitably oxidised magnesium-aluminium alloy surface was covered with a mixture of aluminium and magnesium oxides which on polishing interacted to form spinel with a permanently amorphous and therefore smooth Beilby layer. Laboratory experiments have already shown a reduction in wear with such " spinelised " surfaces as compared with anodised aluminium, and engine trials are now being carried out with pistons coated with an exceedingly thin layer of " spinelised " magnesium-aluminium alloy.

Cast iron plays an important rôle as a bearing metal, especially at high temperatures. It is true that the modern aircraft engine cylinder is fitted with hardened liners, and that the old-type cast iron piston has had to make way for the lighter aluminium one ; nevertheless as piston-ring material cast iron still reigns supreme and indispensible in spite of its otherwise undesirable mechanical properties. The study of polish on cast iron [19] tells us why this is so. When a smooth surface of cast iron of the quality used for making piston rings is lightly rubbed with a fine grade emery paper and then examined in the electron diffraction camera, a pattern of graphite is obtained and not, as might have been expected, one characteristic of iron crystals. This particular graphite pattern is a rather unusual one in that it consists of one or more rows of rather diffuse blobs which we recognise as being due to a layer of graphite crystals completely covering the surface and so orientated that their slip planes are parallel to it. Thus these simple experiments show that the effect of even very fine abrasion on cast iron is to cause the occluded graphite crystals to be spread out over the surface in such a way as to act as a lubricant and to protect the cast iron from abrasion. Even when cast iron is polished instead of abraded, the resulting diffraction pattern clearly shows the graphite spread out over the Beilby layer (Plate II, Fig. 9). The importance of this " graphoid " film lies in the fact that graphite is an excellent lubricant, since the carbon atoms are bound together in sheets which can slide over one another very easily; furthermore, unlike an oil, it retains its lubricating properties, even at the high surface temperatures encountered in the internal-combustion cylinder. It will be realised, of course, that the supply of graphite is limited, and when inclusions near the working surface become exhausted, further supplies can only be tapped as a result of surface wear. This explains why it has nowadays become a usual practice to augment the natural graphite supply contained in the cast iron by the addition of colloidal graphite to the lubricating oil.

In closing, I should like once more to emphasise that the fundamental basis of our present knowledge of the subject of this article was provided by Beilby. He took up the quest practically where Newton left off and with means at his disposal which gave him no direct access to the realm of submicroscopic phenomena he had set out to explore ; nevertheless, his genius for experiment more than counterbalanced the deficiencies of his equipment and gave him, before all others, the first real insight into the nature of polish.

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DISCRETE SPACETIME

A Kinematographic View of the Physical World.[1]

By LUDWIK SILBERSTEIN, Ph.D.

THE term "discrete" is, perhaps, less familiar than is its opposite, "continuous," which stands for "gapless" or "uninterrupted." Both concepts were already familiar to the ancient Greeks, and Aristotle has given a fairly satisfactory definition of them. But I need not tire out the reader by attempting to define them here in a rigorous, abstract logical way. A few examples will serve our purpose much better.

A heap of pebbles form a *discrete manifold* of elements, namely of pebbles. Suppose you have arrayed them into a single row and labelled them by ordinal numbers, 1, 2, 3, and so on. Then between some two pebbles, as, for example, the third and the sixth, there are other pebbles, the fourth and the fifth, but between some others, say the second and third, there is no element. It is just this that characterises a discrete manifold. In much the same way, a library is a discrete manifold of books and a museum a discrete manifold of exhibits, no matter how heterogeneous between them and how numerous, a hundred or a million. Such a manifold may even be, virtually, infinite, and yet remain discrete, as when there are ever and ever new accessions to a library or a museum. So also is a crowd of civilians or an army of soldiers. All these are palpably discrete manifolds, perceived and recognised as such by everybody. Some other entities, as a volume of hydrogen gas or a tankful of mercury, apparently continuous, are recognised as discrete manifolds, *viz.* of H_2 molecules or Hg atoms, only after a considerable scientific training.

An atom itself, originally thought of as an indivisible unit, somewhat like a hard kernel or tiny billiard ball, is in more recent times viewed as a group of electrons, separate specks of negative

electricity, surrounding a positive nucleus, in fine, as a discrete manifold of many parts, especially in the case of heavier atoms, as, for example, uranium with its 92 electronic satellites.

It would be easy to quote many other discrete manifolds, but the examples already quoted ought to suffice.

On the other hand, if I were asked to produce an example of a *continuous* manifold, I should feel embarrassed, because—as a matter of fact—I do not know of any, actual or palpable ones, that is.

It is true that the modern scientist, nay, any of the older ones since, at least, the discovery of the infinitesimal calculus as a powerful mathematical tool, would invoke as continuous manifolds a geometrical line, a surface, three-dimensional space itself, and time. But all these cases are purely conceptual.

It is particularly interesting in our connection to quote (from Maxwell's *Matter and Motion*) Newton's own definition of " absolute time," which runs thus : " Absolute, true, and mathematical time is flowing at a constant rate, unaffected by the speed or slowness of the motions of material things."

The " constant rate " with the assurance of " unaffectedness " is just meaningless (for " rate " is the increase of something with respect to something else, and what is the latter ?), but " flowing " undoubtedly implies continuity, not discreteness.

But what do we know of the continuity of time in its experimental aspect, even of our inner feeling or awareness of time—duration—while we watch some events or wait for something to happen ? And when we try to find the meaning of such a " time " by a most careful self-contemplation with as complete an exclusion of extraneous sensations as is humanly possible, we will find that we are either counting our discrete pulse beats or, perhaps, spasms of a nascent toothache or watching some succession of shreds of pleasant or painful reminiscences, which by some involuntary conceptual supplementing may be cemented into sets of states merging into each other, into continua, but which actually are discrete arrays of elements, sometimes quite heterogeneous and distinctly contrasted between themselves.

Or take that other most familiar conceptual, but not tangible, continuum, the geometrical line, a straight-line segment, as we know or believe to know it from Euclid. Such a segment consists, according to Euclid, of an infinity of points. As Clifford remarks, this follows from Euclid's explicit postulate : " Every line can be bisected." In fact, if it consisted of a finite number of points it could not be bisected whenever this number were odd. We may add that for a line segment to be always bisectable it would suffice

if it consisted of a denumerable infinity of points, as, for example, are all integers, but the newer geometers have been more lavish and decided to consider a linear segment as a strictly continuous manifold of points, setting them in a one-to-one correspondence with the elements of what is technically called " the linear continuum " which is isomorphic, for example, with all real numbers, including the irrationals, as $\sqrt{2}$, and the non-algebraic ones, as e or π.

So far the geometrical line as a one-dimensional continuous manifold of points. The complete *space* of our abstract geometers, and the more concrete physicists and astronomers, is considered as a *three*-dimensional continuous manifold, its points being labelled by a triad of numbers, x, y, z, each of which can assume any real value, whole, fractional, irrational or non-algebraic, like π. It will be useful to mention that a manifold, whether continuous or discrete, has by itself neither metrical properties, nor dimensionality. To endow it with the latter, its elements must first be ordered in some way or other. To illustrate, think, for instance, of a library, a manifold of books. If we wish, we can make it just one-dimensional, by numbering through all the books, regardless of their subject, language, etc., by simple labels, 1, 2, 3, etc., which with the arrival of new accessions may be prolonged indefinitely. Or else we may take account of the year of publication, numbering in a simple fashion the books published within each year Y ($N = 1$, 2, 3, etc.) and thus convert the library into a two-dimensional manifold, each book receiving a double label $\boxed{Y, N}$, as 1932, 49. Or, taking into account the subject and the language, we may give our library three or four dimensions, and so on. Similarly does the dimensionality of a continuous manifold, *e.g.* the conceptual space, depend on the method chosen for ordering its elements, points. Thus, for instance, Hilbert of Göttingen and Moore of Chicago were able to bring all the points of a square (which on the usual ordering principle is two-dimensional) into a one-to-one correspondence with the points of a line segment, a manifold of one dimension. They have achieved this result by spreading over the area of the square certain " crinkly " curves.[1] In the same way a cube, commonly treated as three-dimensional, can be made one-dimensional, all its points being labelled by the values of a *single* real variable.

These remarks may suffice for illustrating the concepts of discrete and of continuous manifolds and of their dimensionality.

[1] For details, see the original papers or J. W. Young's *Lectures on Fundamental Concepts of Algebra and Geometry*, New York, 1911, p. 167.

Turning now to our main subject, we must ask ourselves what are the experimental facts concerning the physicist's space and time.

Within a straight segment, say 1 cm. long, we can mark distinctly a good number of "points," dots or scale divisions, and fix the intervals between them, but we certainly cannot lay hands upon (*i.e.* observe directly or by photography) anything less than perhaps a hundredth of a micron, even with the aid of the best microscope. Similarly, with regard to time. We can mark or register graphically, even automatically, pairs of instants separated by a second, a tenth and, with some elaborate appliances, a much smaller fraction of a second. A Siemens spark chronograph [1] enables one to register with fair reliability time intervals down to one-millionth of a second. But this perhaps is, and may remain, our experimental limit. Such short times as 10^{-18} sec. attributed to some X-rays (of wavelength equal to 3 A.) as their period, and even 10^{-15} sec. attributed to light, are not direct, but inferential, indeed, very elaborately so.

It is true that our increased technical means have enabled us to push the lower limits of space and time intervals farther and farther down. But this does not authorise us to assert that space (length) and time are indefinitely subdivisible, that the physicist's space and time co-ordinates are continuous. The need for such a caution has of late been particularly emphasised by the fact that our usual, large-scale physics, geometry and chronometry have turned out not to be applicable to the atomic or subatomic world.

Habitually, all phenomena or events are considered as placed (located) in some kind of a pre-existing conceptual space and time, both viewed as continua. As against this I should like to impress upon the reader's mind what seems to me a perfectly sober, matter-of-fact view, namely, that "space" and "time" or their Minkowskian union "spacetime," is ultimately nothing else but a system of *fourfold labels*, any such label $\boxed{x, y, z\ ;\ t}$ being attached to every event, in much the same way as labels are affixed to the books of a library, with their repetitions entered into a complete catalogue.

As a matter of historical fact, the procedure of cataloguing natural phenomena, events, adopted by scientists, especially since the discovery of the Infinitesimal Calculus, that powerful tool, at least for treating *macroscopic* phenomena, consisted in assigning to each of the four co-ordinates the whole range of real numbers, a full continuum. In other words, the exact scientists have through-

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[1] Which mainly consists of a fixed platinum point and rapidly revolving steel wheel as terminals of an electric spark gap.

out, especially since the advent of the Calculus, used *continuous* fourfold labels in registering or in attempting to predict natural phenomena, that is to say, labels

$$\boxed{x, \ y, \ z \ ; \ t}$$

with x, etc., each a mathematical continuum.

Now, my question is whether all these labels are not much too many, whether they are all actually indispensable, or whether, say, a *denumerable* infinity of x's, etc., would not suffice and even turn out, perhaps, more appropriate for the treatment of natural phenomena, particularly *on a small scale*, atomic and subatomic.

And I propose, accordingly, to consider a discrete system of whole-number labels

$$\boxed{x, \ y, \ z \ ; \ t}$$

where x is liable to assume only the values 0, ± 1, ± 2, and so on, and similarly for y, z, t. " Discrete Spacetime," as used in the title of this article, stands for the totality of all such labels.

To begin with, we may place ourselves in the position of a unique observer or his platform, postponing for a while the relativistic discussion of various platforms.

The time-part of our labels calls then but for a few remarks.

The familiar concepts of " before " and " after " can be taken for granted or else, agreeably to formal logic, can be considered as our " undefined terms." Such being the case, we may simply say that each *instant t* has only two *next neighbours*, one *before* or preceding t and one *after*, or following upon t, and give these two neighbours the labels $t - 1$ and $t + 1$. And the distance apart or the length of the interval between any two instants t_1 and t_2 will be simply the absolute value $| t_1 - t_2 |$ of their difference.

The remaining triple xyz of our labels, however, or the manifold of space-points, if it is to be endowed with three dimensions and metrical properties (distance, etc.), presents a rather complicated and subtle problem. For the points have not only to be ordered in some way or other, but each point must be assigned a certain number of " next neighbours," which is necessary for any reasonable definition of " distance " (least number of " steps," *i.e.* passages from neighbour to neighbour). Such also is essentially the case for a submanifold of two dimensions.

For the present, therefore, only *one*-dimensional space will be considered, that is to say, including the time, a *two-dimensional* world, x, t. This will, at any rate, serve as a good introduction to the whole subject. Similarly to the time instants, each point x will

now have only two next neighbours, $x - 1$ and $x + 1$, and the distance between two points x_1 and x_2 will be $| x_1 - x_2 |$.

KINEMATICS OF A PARTICLE

A macroscopic body, even an atom, must, of course, be supposed to occupy a plurality of points at the same instant. Possibly an electron occupies but one point at a time. But we need not, at this stage, decide such questions. Our " particle " will, simply by definition, occupy at any instant (t) *one and only one point* (x). It will thus be analogous to the " pieces " on a chessboard, each of which occupies but one place (square) at a time.

It seems reasonable to make the following assumption: *A particle cannot skip a position (point)*, that is to say, if at the instant t it is at the point x, then at $t + 1$ it can only be at either

$$x \text{ or } x + 1 \text{ or } x - 1.$$

Let the difference of x (for a particle) at two consecutive instants be denoted by Δx, *i.e.*

$$\Delta x = x_{(t+1)} - x_{(t)}.$$

Then our assumption will be expressed by

$$\Delta x = 0, \pm 1 \quad . \quad . \quad . \quad . \quad \text{(A)}$$

We may call $| \Delta x |$ the microscopic *velocity* of the particle, reserving the name " velocity " (v) without qualification for the average change of x per unit Δt in a full period, if this consists of a moderate number of instants; *e.g.* if for $t = 0, 1, 2$, etc.: $x = 0, 1, 1, 2, 3, 3, \ldots$, and $v = \frac{2}{3}$. The assumption (A) will then read: The microscopic velocity of a particle is either nil or 1.

As a consequence of assumption (A) there is a natural *upper limit* to all possible velocities of a particle, namely, $| \Delta x | = 1$, corresponding to the equation of motion $x = t$ or $x = - t$. The particle passes each instant to the next point. If it lingers two or more instants at a point, its (macroscopic) velocity v is necessarily smaller than unity.

This explains in a simple way the hitherto mystical property of the light-velocity in vacuo ($c = 3 \cdot 10^{10}$ cm./sec.), *e.g.* in Relativity, as the limit of all admissible velocities.

Accordingly, the velocity of light will in our treatment be *unity*, while all other velocities will be fractions, $v < 1$, always rational. If a particle lingers n instants at each point before passing to the next, its velocity is $v = 1/n$, *e.g.* $\frac{1}{2}$, $\frac{1}{3}$, etc. If the particle lingers, intermittently, one and two instants at successive points, its velocity is $v = \frac{2}{3}$, as shown in the graph (p. 632), where

a "world-point," *i.e.* a pair of x, t values is represented by a square. Uniform motion with velocity 1 (or light propagation) is shown by the diagonal array of blackened squares ; all other details of the diagram are self-explanatory. While in the usual graphs of world-lines x and t are given in cm. and sec., here they are marked in " points " and " instants."

Our limiting velocity, $v = 1$ (a direct consequence of assumption A), having been identified with light velocity in vacuo, it is interesting to note that in all familiar cases of terrestrial and celestial motion the constituent particles of the moving body must linger
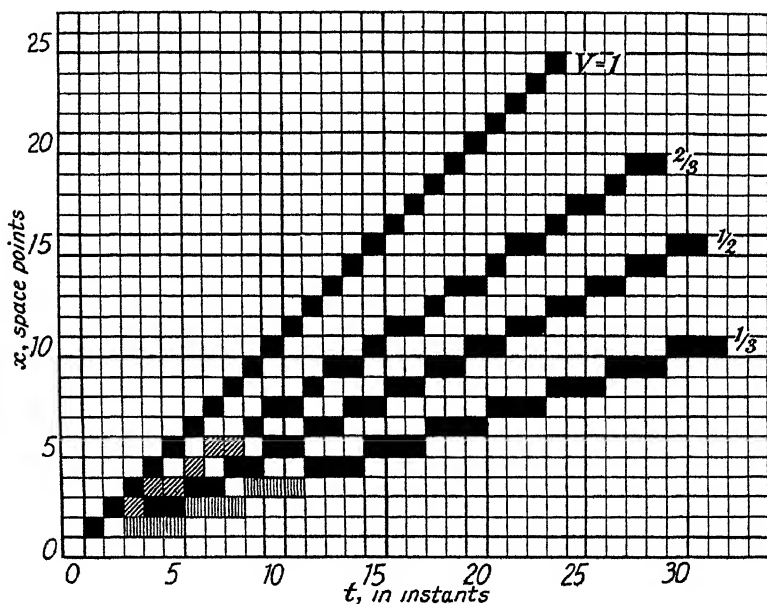
very many instants at each point of their path. The velocity $v = 300$ metres per second is nothing uncommon for a bullet or shell. Its ratio to the light velocity is $v/c = 10^{-6}$, so that each particle of such a projectile lingers a million instants at a point before it moves to, or rather reappears in, the next point. Some β-particles (electrons) are, no doubt, more nimble, *i.e.* do not lose many opportunities, instants, of changing their places. For the velocities of some such electrons are known to approach the velocity of light very closely.

How many instants are contained in our usual time unit, the second ? At this stage of our venture we certainly do not know,

not even approximately. But what can easily be done is to find a not-uninteresting *lower limit* to this number. Let a, b be the numbers of points and instants per centimetre and per second, respectively. Then, since the light velocity is, in our reckoning. unity, $\frac{1}{a} : \frac{1}{b} = 3 \cdot 10^{10}$ cm./sec., whence

$$b = 3 \cdot 10^{10} a.$$

Now, it is no exaggeration to say that we can almost lay hands on atoms or molecules, such as H_2, whose diameters are of the order of 10^{-8} cm. This forces us to take $a \geq 10^8$ cm.$^{-1}$, and therefore, $b \geq 3 \cdot 10^{18}$ sec^{-1}. Thus there are at least three trillion instants per second. The upper limit of b is, of course, for the present, entirely unknown. If in the future, however, some physical conclusions, implying either a or b alone and leading to experimental predictions, are derived, so that a or b can actually be estimated, the known value of b/a will enable us to find also b or a, respectively. In the meantime nothing more can be profitably said about this subject.

Consider once more the (macroscopically) *uniform* motion of a particle with any velocity $v < 1$. Its equation can be written $x = [vt]$, where the brackets indicate that the nearest integer below or above vt is to be taken, according as t is positive or negative. Thus, for example, for $v = \frac{2}{3}$, the equation $x = [\frac{2}{3}t]$ gives for the successive instants $t = 0, 1, 2$, etc.,

$$x = 0, \ 1, \ 1, \ 2, \ 3, \ 3, \ 4, \text{ etc.,}$$

as shown in the diagram.

Notice that any such equation as $x = [vt]$, where v is a rational fraction, is, unlike the equations of uniform motion in continuous Physics, by no means a linear equation. If k be any factor, $[vkt] \neq k[vt]$, in general. In fact, $[vt]$ can be developed into a power series of t which never breaks off with the first power of t, unless $v = 1$.

ARITHMETICAL EQUIVALENT OF TAYLOR'S THEOREM

In continuous physics the precise form of causality is embodied in the famous theorem of Taylor (or MacLaurin). In discrete spacetime this is replaced by a certain Arithmetical Theorem.

Let us first recall the former theorem, limiting ourselves always to a two-dimensional world x, t. If x_0 be the initial position ($t = 0$) of a particle, $x_0' = (dx/dt)_0$ its initial velocity, x_0'' its initial acceleration, and so on, then (under certain analytical conditions) the

position of the particle at any other time t, past or future, can be expressed by

$$x_t = x_0 + tx_0' + \frac{t^2}{2!}x_0'' + \frac{t^3}{3!}x_0''' + \dots$$

Apparently this gives us the power of deriving the whole future and past of a particle from its present (initial) state. But the term " state " means here the totality of data x_0, x_0', x_0'', $\dots$, infinite in number, and owing to this circumstance the almost magical power of Taylor's theorem, commonly viewed as the expression of strict causality, turns out to be purely academic, theoretical. When we come to facts, that mysterious power dwindles almost to nothing, since all that mass of data, x_0, x_0', etc., turns out to be but formally initial or instantaneous, while in reality it implies the knowledge of the particle's history stretching over quite a considerable time interval. To see this, one has only to recall the definition of the derivative, which is

$$x' \equiv dx/dt = Lt_{\Delta t \to 0} \{(x_{(t+\Delta t)} - x_{(t)})/\Delta t\},$$

and similarly for x'', etc. The requirement " $\Delta t \to 0$ " means squeezing a multitude of x_t-values (for $t \gtrless 0$) into the instant $t = 0$ itself. To determine x_0' experimentally with some accuracy, we must take a goodly interval Δt and we cannot reduce it indefinitely. To determine x_0'' one must know three positions x_0, x_1, x_2 corresponding to instants which again must be spaced rather amply. And the more so for x_0''', when four positions are needed, and so on. This takes, practically, all the virtue and mysterious power out of the theorem. None the less, an abstract philosopher, grown up in the tradition of continuous spacetime, would still proclaim the causal determinateness of the past and future by the complete present " state."

If we now turn to discrete spacetime, the rôle of Taylor's theorem is taken over by a certain arithmetical theorem, familiar in connection with arithmetical series of the second and higher orders.

Let x_0, x_1, x_2, etc., be the positions of a particle at the instants $t = 0, 1, 2$, etc. The so-called *first difference*, at x_0, is defined by $\Delta x_0 = x_1 - x_0$, and that at x_1 by $\Delta x_1 = x_2 - x_1$, and so on. The *second difference* Δ^2 is the difference of two successive first differences, $\Delta^2 x_0 = \Delta x_1 - \Delta x_0 = x_2 - 2x_1 + x_0$. Similarly, the third difference, $\Delta^3 x_0 = \Delta^2 x_1 - \Delta^2 x_0$, and generally the tth difference, for any positive (integer) t, taken at x_0, $\Delta^t x_0 = \Delta^{t-1} x_1 - \Delta^{t-1} x_0$.

Now, by the definition of Δ, $x_1 = x_0 + \Delta x_0$ or, more compactly,

$$x_1 = (1 + \Delta)x_0,$$

where $\varDelta$ is an operator, the symbol of the operation of "taking the difference" or the increment from the operand to the next x-term. Similarly,

$$x_2 = (1 + \varDelta)x_1 = (1 + \varDelta)(1 + \varDelta)x_0$$

or, briefly,

$$x_2 = (1 + \varDelta)^2 x_0,$$

where the exponent 2 stands for the iteration of the operation $(1 + \varDelta)$. So also $x^3 = (1 + \varDelta)^3 x_0$, etc., and for any positive [1] integer t,

$$x_t = (1 + \varDelta)^t x_0 \qquad . \qquad . \qquad . \qquad . \qquad (1)$$

Since $\varDelta$ and hence also $1 + \varDelta$ is a distributive operator, this can be developed as Newton's binomial. Thus, and denoting the familiar binomial coefficients by $\binom{t}{2}$, $\binom{t}{3}$, etc., $\left[\binom{t}{2} = \dfrac{t(t-1)}{1.2} \right]$, etc.,

$$x_t = x_0 + t\varDelta x_0 + \binom{t}{2}\varDelta^2 x_0 + \binom{t}{3}\varDelta^3 x_0 + \ldots + \varDelta^t x_0. \quad . \quad (1a)$$

This is the required arithmetical theorem. It enables us to express any future position x_t of the particle in terms of its present position x_0 and the differences of the first up to the tth order, taken at x_0 itself. Formally, therefore, one might say that the future of the particle is predetermined by its present "state," namely, by x_0, $\varDelta x_0$, $\varDelta^2 x_0$, etc., up to $\varDelta^t x_0$ inclusively. But the knowledge of $\varDelta x_0$ presupposes that of x_0 and x_1, the knowledge of $\varDelta^2 x_0$ that of x_0, x_1, x_2, and so on. Finally, in order to get $\varDelta^t x_0$ we must first know x_0, x_1, . . . , and x_t itself. The performance of the arithmetical theorem is thus very meagre : it yields nothing but one of the very data. In fine, it is an identity or tautology. And so it should be. For the arithmetical machine can yield only what we put into it. At the same time we see that the mysterious prophesying power of the Taylor theorem in continuous spacetime, where all the points, x_1, x_2, . . . x_t are squeezed into x_0 itself, is here, in the discrete manifold, explained away. Nothing is left of it but an identity, though stated in a form which sometimes may be useful. And the actual status of those apparent predictions is herewith cleared up, leaving nothing to be wondered at.

It may be well to illustrate the theorem (1) or (1a) at least by one example. Consider the uniform motion of a particle with the velocity $v = \frac{1}{2}$, as expressed by $x = x_t = [\frac{1}{2}t]$. By writing out

---

[1] For *negative* t use the operator Δ' defined by $\Delta' x_0 = x_0 - x_1$. Then $x_t = (1 - \Delta')^{-t} x_0$, where $-t$ is a positive integer.

the successive values of x, Δx, $\Delta^2 x$, etc., from the instant $t = 0$ onwards, the reader will see at once that

$$\Delta x_0 = 0, \quad \Delta^2 x_0 = 1, \quad \Delta^3 x_0 = -2, \quad \Delta^4 x_0 = 2^2, \ldots \Delta^t x_0 = (-2)^{t-2},$$

so that, by (1a),

$$x_t = \binom{t}{2} - 2\binom{t}{3} + 4\binom{t}{4} - \ldots + (-2)^{t-2}$$

which, by gathering together the equal powers of t, can be readily converted into a power series of the form $\dfrac{x}{t} = c_1 t + c_1 t^2 + \ldots + c_i t^i$.

THE PRINCIPLE OF SPECIAL RELATIVITY IN DISCRETE SPACETIME

Consider two platforms S, S' moving relatively to each other uniformly with a velocity v. Let x, t and x', t' be the co-ordinates employed by observers attached to these platforms respectively. Let the origin O' of x' coincide with that of x at the instant $t = 0$ which we may take also as the instant $t' = 0$. In fine, let $x' = t' = 0$ for $x = t = 0$. Then the motion of O' viewed from the S-standpoint will be expressed by $x = [vt]$, where $v < 1$. This, as we already know, is a non-linear equation.

The light velocity being unity, the propagation of light from the S-standpoint, along the two senses of the x-row, is expressed by

$$x = t \quad \text{and} \quad x = -t.$$

Let us now claim, as in Special Relativity, *the invariance* of light propagation, *i.e.* that the last two equations should be transformed into

$$x' = t' \quad \text{and} \quad x' = -t'$$

respectively. What is the transformation of x, t into x', t' which satisfies this requirement ? Since $x = [vt]$ is not a linear equation, the transformation aimed at must, obviously, be a *non-linear* one, while the Lorentz transformation familiar from continuous physics is linear. We may state beforehand that, if also the *reciprocity* (or perfect equivalence) of the two systems $S(x, t)$ and $S'(x', t')$ is required, the relativistic principle, *i.e.* the invariance of the equations $x = \pm t$, can be satisfied only approximately, for such cases, that is, where the lengths implied contain many points and the times, many instants.

Since $x' = 0$ whenever $x - [vt] = 0$, we may put

$$x' = [\gamma(x - [vt])] \quad . \qquad . \qquad . \qquad . \quad (2a)$$

where γ is independent of x, t, but will depend on v, and since γ

need not be an integer, the external brackets are necessary for making x' an integer. To secure the invariance of light propagation we may put

$$t' = [\gamma(t - [vx])] \quad . \qquad . \qquad . \qquad . \quad (2b)$$

In fact, if $x = t$, (2a) and (2b) give $x' = [\gamma(t - [vt])] = t'$, and similarly $x = -t$ leads to $x' = -t'$, as required. It remains to determine γ as function of the velocity v. Now, if one claims *reciprocity* for the two systems S, S', so that neither should be privileged, one is driven to assume, on the pattern of (2a), (2b),

$$x = [\gamma(x' + [vt'])], \; t = [\gamma(t' + [vx'])].$$

On the other hand, (2a), (2b) can be solved for x, t, and if these x, t values are identified with the last-written ones, we get two conditions containing γ, v alongside with x', t' and some fractional numbers. Now, a discussion of these two conditions, which may be omitted here, shows that they cannot in general (*i.e.* for all x', t') be satisfied rigorously, but only *approximately*, namely by putting $\gamma = 1/\sqrt{1 - v^2}$. This gives ultimately the transformation

$$x' = \left[\frac{x - [vt]}{\sqrt{1 - v^2}}\right], \; t' = \left[\frac{t - [vx]}{\sqrt{1 - v^2}}\right] \qquad . \qquad . \quad (3)$$

where the square brackets have the meaning explained above. This turns out to be simply the well-known Lorentz transformation *arithmeticised* (by those brackets) in accordance with the discrete character of our spacetime.

To show more clearly the nature of the *approximation* with which the Special Relativity principle is satisfied in this treatment of spacetime, we may consider the often-invoked case of to-and-fro light signalling in one platform, S', as viewed by an observer placed on S' and by another observer stationed on the platform S.

Having limited ourselves to one-dimensional space, we can consider only longitudinal (not transversal) signalling, and this cannot give us separately the familiar relativistic contraction of moving rods and slowing down of clocks, but only the ratio of these two effects.

Let the S'-observer send a light flash from any point of his platform towards a (point-)mirror placed at a distance l' and receive it back after the interval T' of his time. Since the light velocity is unity, $T' = 2l'$. Let l and T be this distance and time interval as estimated by the S-observer. What is the ratio of T/T' to l/l'?

On the usual, continuous spacetime theory we have $T = t_1 + t_2$,

where $t_1 = l + vt_1$ and $t_2 = l - vt_2$ (to-and-fro passage), whence $T = 2l/(1 - v^2)$. Thus the required ratio is

$$\frac{T}{T'} : \frac{l}{l'} = \frac{1}{1 - v^2} = \gamma^2 \qquad . \qquad \text{(continuous)}$$

This agrees with the well-known Einsteinian contraction and retardation formulæ $l/l' = 1/\gamma$, $T/T' = \gamma$.

In discrete spacetime we have again $T' = 2l'$, but the expression for T is somewhat different. We now have $T = t_1 + t_2$, where

$$t_1 = l + [vt_1], \; t_2 = l - [vt_2],$$

and since $v < 1$, these equations for the to-and-fro times of passage of light are *non-linear*. Further, they do not always admit whole solutions for t_1 and t_2. This depends on the values of l and v. If we write $[vt_1] = vt_1 - \varepsilon_1$, $[vt_2] = vt_2 - \varepsilon_2$, the fractions ε_1, ε_2 themselves depend on t_1, t_2. In an approximate treatment, however, we may disregard this fact and take $t_1 = (l - \varepsilon_1)/(1 - v)$, $t_2 = (l + \varepsilon_2)/(1 + v)$. Then

$$T = \frac{2l}{1 - v^2}\left\{1 - \frac{v(\varepsilon_1 + \varepsilon_2)}{2l}\right\},$$

and since $T' = 2l'$, we have ultimately

$$\frac{T}{T'} : \frac{l}{l'} = \gamma^2\left\{1 - \frac{v(\varepsilon_1 + \varepsilon_2)}{l}\right\} \qquad . \qquad \text{. (Discrete)}$$

Now, ε_1, ε_2 as well as v being fractions, $\frac{1}{2}v(\varepsilon_1 + \varepsilon_2) \lesssim 1$, so that the bracketed coefficient of γ^2 is contained between 1 and $1 - 1/l$. Thus, if l is even as small as a millimetre, and contains, therefore, at least ten million points, our formula is experimentally indistinguishable from that based on the assumption of continuity. The same thing holds of our transformation (3) and its inverse.

Special Relativity, then, is valid in our discrete spacetime provided that the time intervals implied in its crucial tests, as, for example, the famous Michelson-Morley experiment, contain a large number of instants, even a thousand only, and the lengths an equally considerable number of points. Now, in all actual experimental tests of that doctrine both conditions are amply satisfied. If, on the other hand, the dimensions become atomic or subatomic and the implied times shrink to 10^{-18} sec. or so, the principle of relativity may well break down according to our discrete theory. But we certainly cannot claim to know that it actually does hold in the atomic or ultra-atomic world. On the contrary, the modern physicists are rather inclined to believe that our usual macroscopic

physics and our space and time concepts themselves are not applicable in that domain. We may mention here Niels Bohr's recent " complementarity " principle according to which we can have, microscopically, either Causality or Spacetime localisation (continuous), but not both.

It seems, therefore, reasonable to hope that the discrete spacetime theory here proposed may render good service especially in the domain of atomic and subatomic phenomena.

We have, so far, considered only *special* relativity. Einstein's General Relativity is, in the present connection, on a different footing. Since our space and time co-ordinates are limited to integers, rigorously or microscopically, no room at all is left for infinitesimals and, therefore, for generally co-variant tensors. Macroscopically, however, the principle of general co-variance with all its implications can well be upheld provided that a " physically infinitesimal " world element $dxdydzdt$ is supposed to contain a good number of discrete spacetime points, in much the same way as when the methods of the infinitesimal calculus are being applied, in the kinetic theory of gases, to a swarm of molecules or, in astronomy, to a galaxy of stars.

One more peculiarity of the proposed scheme may be mentioned. Consider two particles, initially n points apart and moving against each other with the constant velocities v_1 and v_2, so that their co-ordinates at any time are $x_1 = [v_1 t]$ and $x_2 = n - [v_2 t]$. The instant of their meeting will be determined by $x_1 = x_2$ or by

$$[v_1 t] + [v_2 t] = n.$$

Now, v_1 and v_2 being any given rational fractions, this equation will admit one or more integer solutions or none, according to the value of n. In other words, the two particles may, as it were, shoot through each other without ever meeting, *i.e.* occupying simultaneously the same point. Whether such will be the case or not depends on their velocities and their initial distance. Thus, for example, if $v_1 = v_2 = v$, our equation becomes $2[vt] = n$, and since $[vt]$ is an integer, the particles will meet or pass each other freely according as n is even or odd. In the latter case they simply exchange their positions. If, for instance, $v = 1$ and $n = 3$, the successive positions of the two particles are 0, 3; 1, 2, and 2, 1. Nay, two rods (rows of particles) might thus shoot through each other, no particle of one rod ever coinciding with a particle of the other. This would abolish, theoretically, the concept of impenetrability of bodies. Practically one could scarcely expect two sets of very numerous particles to be arranged so artfully as to satisfy

the requisite conditions.   The free passage of two rigid rods through each other would thus not be impossible, but only very improbable, like the freezing of water in a kettle placed on a blazing kitchen range.   On the other hand, one might be tempted to make the assumption that two particles cannot occupy the same point at the same instant.   If then two particles moved in such a way as to lead to an encounter, $x_1 = x_2$, for a certain integer value of t, they would in the next instant rebound from each other, as it were, each inverting its motion.   But no such possibilities can be profitably discussed until the proposed doctrine is more fully developed.

Needless to say, the writer is far from claiming to have constructed a somewhat complete theory of Discrete Spacetime, not even of its two-dimensional section.   The ideas here expounded form but a first attempt at such a theory and the main purpose of this article is to stimulate interest in what seems by no means an unreasonable scheme and to invite the effort of more able workers to master some actual problems of atomic and subatomic physics by using a discrete set of spacetime labels.

THE LAYER-LATTICE IN RELATION TO MINERAL CHEMISTRY: A REVIEW

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SINCE the layer-lattice conception was put forward by Pauling [1] to interpret the structure and variable composition of micas, chlorites, and the clay-species, its intensive application to hitherto refractory problems in the chemistry of these minerals has been highly fruitful of results confirming the fundamental soundness of the conception. The position prior to and since 1930 was reviewed by C. E. Marshall in this Journal, January, 1936.

While this new principle has supplied many close approximations to the truth in general cases, it is but natural that its application should have encountered many subsidiary problems concerning purely academic details. These problems in turn have had their repercussions in many fields of applied science, since they bear on matters which are of primary importance to industry and technology.

Admittedly, the layer-lattice conception has its foundations in a highly specialised branch of physical chemistry—intimately co-ordinated with the application of X-ray methods to the study of crystal structure. The tardiness with which it has been assimilated to supersede the earlier empirical treatment of these platy mineral-silicates is due partly to unavoidable lag in the incorporation of its results in standard text-books on systematic mineralogy, and partly to a mistaken assumption that both the conception and its results are too technical, too detailed, for the student who does not propose to specialise in mineralogy. With a view to disposing of that assumption, the present review outlines the conception, and some of its outstanding applications, in simple terms, while directing attention to subsidiary problems of special interest to the analytical chemist.

The study of crystal structure by X-ray methods is gradually welding detached species and entire groups of silicate minerals into communities distinguished by conformity to a comparatively small

number of structural types or classes, which are fully described in a summary of results obtained (up to 1930) by Professor W. L. Bragg and his co-workers [2]. One such community comprises the "sheet"-class, to which the clay-species, the micas, and the chlorites belong.

The first conception of the sheet-lattice was that of a single two-dimensional layer of tetrahedral (SiO_4)-groups linked up to form a network of (Si_4O_{10})-hexagons repeated indefinitely. This was amplified in 1930 by Pauling [1], who suggested that the micas and
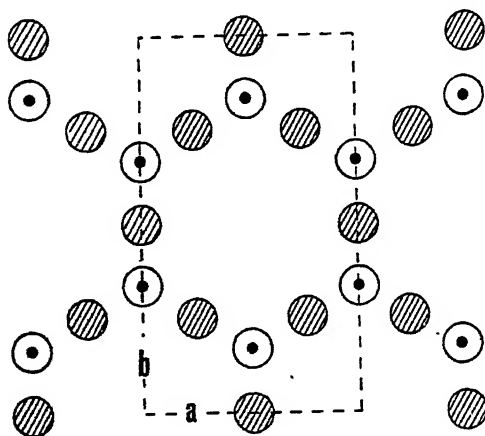
FIG. 1.—Silica layer $\{Si_4.O_6.(OH)_4\}n$

a = 5·0Å ; b = 8·7Å.
Silica-atoms in the plane of the paper.
Oxygen-atoms above this plane (shaded circles).
(OH)-groups below the plane (plain circles).

related minerals are built up by the stacking of layer-lattices belonging to three idealised types :

The Si-layer (Fig. 1) $Si_2.O_3.(OH)_2$ or some multiple.
The Al-layer (Fig. 2) $Al_2(OH)_6$,,   ,,   ,,
The Mg-layer (Fig. 3) $Mg_3(OH)_6$,,   ,,   ,,

Each compound as formulated above would be completely balanced as regards internal valencies, and for each lattice conventional pseudo-hexagonal symmetry may be assumed for the general case.

The individual layers may be stacked so that their planes of symmetry either fall into the same vertical plane or intersect at a definite angle. In talc and pyrophyllite for example [3], this inclination or relative "shift" is 120°.

The Si and Al layers agree so closely in unit-cell dimensions that

stacking is possible not only symmetrically thus . $\frac{Si}{Al}$, as in pyro-
                                                    $\overline{Si}$

phyllite and muscovite, but also asymmetrically thus $\frac{Si}{Al}$, as in

kaolinite and its isomers. Dimensional agreement between the
Si and Mg layers is less close ; hence for talc and phlogopite the
symmetrical arrangement $\frac{Si}{Mg}$ is deemed more probable than an
                        $\overline{Si}$

asymmetrical arrangement [4].

A brief survey of the kaolinite-halloysite group will serve to
illustrate the simpler conditions of the case. In members of this
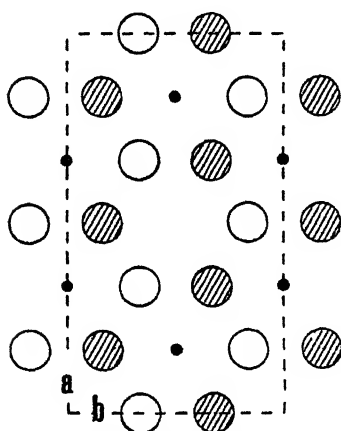
FIG 2.—Gibbsite layer $\{Al_4(OH)_{12}\}n$

$a = 8\,7Å, \ b = 5\,1Å$

Al and Mg atoms in the plane of the paper
(OH) groups above this plane (shaded circles)
(OH)-groups below this plane (plain circles)
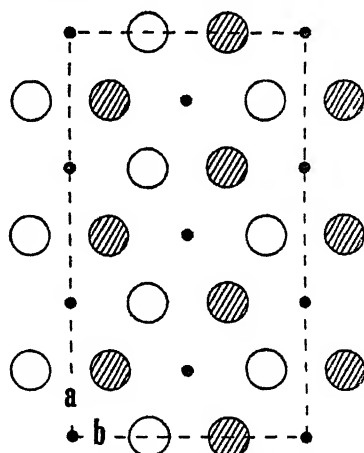
FIG 3.—Brucite layer $\{Mg_6(OH)_{12}\}n$

$a = 9\,4Å, \ b = 5\,4Å$

Al and Mg atoms in the plane of the paper
(OH)-groups above this plane (shaded circles).
(OH)-groups below this plane (plain circles)

group, Si-layers and Al-layers are *initially* stacked alternately thus :

$$
\begin{array}{c}
\cdots \cdots \cdots \\
O_6 \\
Si_4 \\
(OH)_4 \\
\cdots \cdots \cdots \\
(OH)_6 \\
Al_4 \\
(OH)_6 \\
\cdots \cdots \cdots
\end{array}
$$

We should hardly expect that adjacent layers
having the balanced constitutions shown could
assume, and retain, a definite relative orientation
without the aid of primary valencies effective across
the spaces between them. Yet the summation of the
two layers is a simple multiple of the empirical com-
position $4H_2O.Al_2O_3.2SiO_2$ which is that of halloy-
site. If the two layers have no ions in common, it
is difficult to understand why they should hold to-
gether at all.

When, however, half the (OH)-groups are condensed to molecular water, surplus O-atoms become available for linking up the two layers. The new condition may be represented thus:

. . . . The summation of the bonded stack is now a
O_6 simple multiple of $(OH)_4Al_2.(Si_2O_5)$, which is the
Si_4 constitutional formula for kaolinite and its isomers
$O_4(OH)_2$ dickite and nacrite. This formula should be
Al_4 contrasted with the older empirical formula
$(OH)_6$ $2H_2O.Al_2O_3.2SiO_2$, which confuses " molecular
. . . . water " with (OH)-groups. The need for distinguishing between the two will be discussed in a later section.

As the computed unit-cell for dickite (and probably also for its isomers) contains a total of 32 Al- and Si-atoms, the cell-content would be expressed by four times the summation of the column set out above and written thus: $O_6.Si_4.O_4(OH)_2.Al_4.(OH)_6$, or $(OH)_8.Al_4.Si_4O_{10}$.

Kaolinite and its isomers differ among themselves in respect of certain physical and optical properties [5]. These specific characters are reasonably attributed to differences in the space-distribution of the atoms in the unit-cell. Gruner [6, 7] explains them as being due to displacement of one layer relative to another.

Meta-halloysite is another isomer of kaolinite. For halloysite, a structure similar to that of kaolinite was deduced by Mehmel [8], who found the empirical molecular formulæ to be

$4H_2O.Al_2O_3.2SiO_2$—at room temperature (halloysite)

$2H_2O.Al_2O_3.2SiO_2$—at 50° (meta-halloysite).
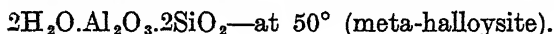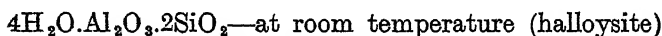
Gruner's dickite-structure and the structures of kaolinite and halloysite derived from it [6] have recently been re-examined by Ksanda and Barth [9], who confirm the unit-cell content and the manner of stacking the layers but dispute Gruner's conclusions concerning the actual arrangement of the atoms in the layers.

Montmorillonite and pyrophyllite conform to a symmetrical

$$Si$$

layering of the general type Al. The simplified constitutional

$$Si$$

formula is $(OH)_2Al_2(Si_4O_{10})$; the cell-content is expressed by four times this formula.

As mentioned above, the usual analytical figures for " water " do not discriminate between (OH)-groups, which are essential parts of the crystal-lattice, and " molecular water " which is mechanically held in the lattice corridors or adsorbed along the crystal surfaces.

By future refinements of technique, chemists may possibly reduce or even eliminate this uncertainty.

In the case of the micas and amphiboles, there is warrant for continuing to treat the analyst's figure for " water above 105°–110° C." as an approximate measure of the (OH)-content. But Mehmel's work [8] indicates the probability that a significant proportion of the (OH)-groups in clay substances generally would be accounted for in " water *below* 105°–110° C."

In particular, the dehydration of halloysite to meta-halloysite implies gradual transition from an initial term containing the group $(OH)_{16}.O_6$ to one with $(OH)_8.O_{10}$. This range could be scaled to define sub-species. Throughout the range, the total valencies of (OH) and O would be constant at 28 and balanced exactly by the total for Al and Si—a condition which is satisfied also by the completely anhydrous compound $Al_4Si_4O_{14}$ corresponding to completely dehydrated halloysite, kaolinite, etc. Hence, if a clay analysis, computed to this basis of O_{14}, be found consistent with a stack of Si-layers and Al-layers in equal proportions (as in halloysite, etc.) the clay could safely be grouped with the halloysite-kaolinite series. Similarly, either a beidellite-montmorillonite or a pyrophyllite term would be suggested by conformity in composition to the formula $Al_4Si_8O_{22}$, which represents a dehydrated stack of Al and Si layers in the proportion 1 : 2. The advantages of this basis of computation have been demonstrated by Marshall [4]. There is need, however, for the co-ordination of carefully controlled dehydration work with X-ray, analytical, and other investigations of clay species, to distinguish, if possible, between neutral water and hydroxyl groups.

The distinction would explain several properties peculiar to clay-species—particularly the lattice-shrinkage displayed by montmorillonite and its " bentonite " varieties when they are progressively dehydrated. The shrinkage is believed to be due to the gradual expulsion of mechanically held water while a definite (OH)-content remains " fixed ". If this distinction were admitted, the fundamental formula for the montmorillonite-beidellite group would be generalised as $mH_2O.(OH)_4.Al_4(Si_8O_{20})$, in which m alone is variable within the stability-range for the species.

This variation was first observed by Hofmann, Endell, and Wilm [10], who found that when m is zero the distance between the layers (crystallographically the d_{001} space) was only about 3·2 Å, but increased to about 13 Å for samples containing more than 30 per cent. by weight of water. They also found that the uptake of water, and the corresponding lattice-expansion, were reversible within the stability-range for the group.

Nagelschmidt [11] has amplified these findings : for montmorillonite samples dried at 250° C. and rehydrated under appropriate controls, the variations in m and d_{001} are co-ordinated as follows :

As m rises from zero to 4, d_{001} expands from 10·5 to 15 Å.

,, ,, ,, from 4 to 10, d_{001} expands from 15 to about 15·9 Å.

,, ,, ,, from 10 to 15, d_{001} shows no significant expansion.

,, ,, ,, from 15 to 20, d_{001} expands markedly—to 19 Å.

Nagelschmidt concludes that the first four water-molecules enter between the layer-lattices and force the layers apart. He suggests that the next ten molecules are mainly absorbed along the crystal-surfaces. With further increase in water-content, the material assumes the consistency of a rather liquid paste : forces attributable to the water-shells surrounding the fragmented crystals are probably strong enough to explain the further widening of the d_{001} space. We may perhaps be justified in concluding also that these forces are competent to disrupt the layer-lattices to fragments approaching colloidal dimensions.

Here, however, the " state of hydration " is again uncertain. The material used by Nagelschmidt was not specially purified [11] ; but even if its hydroxyl-content is measured solely by the analytical figure for " water above 110° C." the analysis, computed to Marshall's basis of O_{22}, gives the formula

$$(OH)_{9.08}Na_{.13}Ca_{.32}Mg_{.48}Fe'''_{.05}Al_{3.54}(Si_{7.49}Al_{.51})O_{17.46}$$
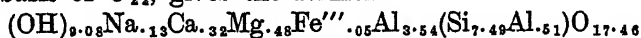
which is in good agreement with common montmorillonite but suggests a sub-species transitional between an ideally pure initial term containing $(OH)_4.O_{20}$ and a final term with $(OH)_{20}.O_{.12}$.

Bentonitic clays, which are composed largely of montmorillonite [12], have a high adsorption-capacity and disperse spontaneously to a highly colloidal material when treated with excess water. The clay fractions of many soils are now known to contain montmorillonite as one of their main constituents [13, 14, 15], and many of the properties of raw soils may ultimately be co-ordinated with lattice-shrinkage and lattice-expansion, as a differential property of their various constituents. As will be observed later, such migrant water is considered by some workers in soil-chemistry to be directly concerned in the mechanism of base-exchange which, in turn, bears directly on various aspects of agricultural practice, such as the control of p_H-content and the fertilisation of soils.

Exposition so far has dealt only with ideally pure clay-species engaging no kations other than (SiAl)-atoms. Before proceeding to the more complex micas and chlorites, which incorporate other kations in varying proportions, brief reference must be made to the

scope, and the consequences, of atomic substitution or proxy-rôles among the atoms [2]. These proxy-rôles conform to the following skeleton scheme :

In the Si-layer : $Si \leftrightharpoons Al \leftrightharpoons Fe'''$.
In the Al-layer : $Al \leftrightharpoons$ (Fe''', Fe'', Mg, Mn, Ti, Ca, etc.)
General : $(OH) \leftrightharpoons F, Cl$.

In the Si-layer, the substitution of Al-atoms for Si-atoms results in a surplus charge on the lattice. This charge must be balanced by an equivalent kation-charge per atom of proxy Al. The further substitution of Fe''' for Al would not of course entail any change in a pre-existing charge. In nontronites, this substitution may be complete, in both layers.

In the Al-layer, the substitution of Fe''' for Al leaves the lattice charge as before ; whereas the substitution of Mg for Al leaves the lattice negatively charged, and this charge must be balanced by its equivalent of another kation, which could be K, Ca, Na, Mg. . . .

These principles have been applied by Marshall [4] to a refractory problem presented by montmorillonite and beidellite, which show important differences in composition, though the same X-ray diffraction pattern was obtained for both species by Hendricks and Fry [13] and also by Hofmann, Endell, and Wilm [10]. This discrepancy between analytical and X-ray data has for some time been a stumbling-block [4].

At first sight, the empirical formulæ for beidellite,

$$3H_2O.Al_2O_3.3SiO_2,$$

and montmorillonite, $nH_2O.(MgCa)O.Al_2O_3.5SiO_2$, show no simple relationship to the ideal constitution $(OH)_4.Al_4.(Si_8O_{20})$ which seems to be established beyond doubt for both species.

Marshall [4] explains the differences between the two species, thus :

Beidellite . . $R'_x Al_4.(Si_{8-x} Al_x).O_{20}(OH)_4$
Montmorillonite . $R'_x (Al_{4-x} Mg_x).(Si_8 O_{20}).(OH)_4$
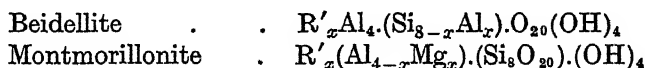
In beidellite, replacement mainly concerns the Si-layer, the predominant replacement being Al for Si. In montmorillonite, the replacement is mainly Mg for Al in the Al-layer, the Mg itself being liable to subsidiary replacement by Ca, Fe'', Mn, Ti. . . .

The principle may now be applied to the interpretation of

muscovite, in which the layers are stacked according to the plan $\begin{matrix} Si \\ Al \\ Si \end{matrix}$

In this case, however, the tetrahedral Si-layers are modified by the

substitution of Al-atoms for some of the Si-atoms, the generalised proportion being approximately (Si_6Al_2) for Si_8. This substitution entails two surplus oxygen valencies, which are balanced by two K-atoms or their equivalent. Some proportion of the (OH)-groups are also replaced by fluorine. The constitutional formula for muscovite is accordingly $(OH.F)_4.K_2.Al_4(Si_6Al_2)O_{20}$, which is a simplification of the spatial arrangement shown below:

. . . .
O_6
Si_3Al
$O_4(OH)_2$
Al_4
$O_4(OH)_2$
Si_3Al
O_6
K_2
. . . .

Varietalism among the muscovites is accounted for by subsidiary replacements, such as would be expressed in the formula by (K.Na.Li, Ca . . .) and (Al.Fe'''.Fe''.Ti . . .)

The unit-cell for muscovite contains a constant total of $(OH.F.O)_{24}$. It is obvious therefore that no analysis of muscovite can be reduced to an exact formula unless all three constituents are known. The analyst's figures for "water" present in a mineral distinguish between water eliminated at temperatures respectively above, and below, 105° C. While it would be arbitrary to assume that the figure for "water above 105° C." can be commuted to an exact equivalent in (OH)-groups, nevertheless it is the conventional practice to do so, and in some cases it is probably true that this figure is a close approximation to the facts. In the majority of cases, the figure affords only a rough measure of (OH)-content, and perplexing problems have been raised by observed discrepancies between "water-content" theoretically expected and the amount determinable by drastic methods of dehydration.

Reverting to the constant (OH.F.O) = 24, which is a characteristic of the mica constitution:

If analytical data alone be available, F may be correctly assessed, but the (OH)-content must be uncertain. Moreover, the actual number of O-atoms is not the constant O_{20} shown in the ideal mica formulæ; but it can be computed from analytical data supplemented by determinations of density and unit-cell dimensions.[1]

It follows also that, if the actual content of O-atoms is not known, the computation of a constitutional formula from analytical data alone can afford only a first approximation to the truth. Yet such an approximation provides a useful working basis for comparing mica species, and a method of computing it is given on p. 649:

| | Per cent. | Atomic Proportions. | | | To Basis (OH).O.F. = 24. |
		Oxygen.	Kations.		
SiO_2 . .	44·68	1·4878	0·7439	...Si...	6·15 } 8·00
Al_2O_3 .	31·80	0·9357	0·6238	...Al...	{ 1·85 } For Si_6Al_2 { 3·31 }
Fe_2O_3 .	4·49	0·0843	0·0562	...Fe‴...	0·46
FeO .	0·84	0·0116	0·0116	...Fe″...	0·10
MgO . .	1·30	0·0322	0·0322	...Mg...	0·27
TiO_2 . .	0·70	0·0174	0·0087	...Ti...	0·07 } 4·30
MnO .	0·04	0·0005	0·0005	...Mn...	trace } For Al_4
CaO . .	0·52	0·0092	0·0092	...Ca...	0·08
BaO .	0·23	0·0015	0·0015	...Ba...	0·01
SrO . .	0·04	0·0003	0·0003	...Sr...	trace
K_2O . .	10·48	0·1112	0·2224	...K...	1·84 } 2·05
Na_2O . .	0·79	0·0127	0·0254	...Na...	0·21 } For K_2
Li_2O . .	nil	—	—	—	—
P_2O_5 . .	trace	—	—	—	—
CO_2 . .	nil	—	—	—	—
	Total	2·7044			
$H_2O+(105°)$	3·50	0·1944	—	...(H)...	3·22 }
$H_2O-(105°)$	0·28	—	—	—	— } For $(OH)_4$
F . . .	0·11	0·0028	—	...F...	0·05 }
	99·81				$O_{20·73}$ For O_{20}
Total : (OH).O.F. :	2·9016				

This computation is checked on a valency basis : in the last column, the total valencies of the Si, R‴, R″, Ti, and R′ atoms balance the total valencies of (OH), F, and O. It is obvious that, as even an incomplete, or an inaccurate, analysis could be computed to a "balanced" formula, analyses used for comparative work on the micas should be both complete and accurate.

This particular muscovite is a well crystallised variety occurring in granite-pegmatite at the Tollgate quarry, Malvern. The extent to which it departs from the ideal muscovite composition serves to illustrate the variation-range spanned by true muscovites and hydromuscovites :

Ideal Muscovite.	A Malvern Species.	Variation, Generalised.
$(OH.F)_4$ K_2 Al_4 (Si_6Al_2) O_{20}	$(OH.F)_{3·27}$ $(K, Na)_{2·05}$ $(R‴, R″, Ti)_{4·30}$ $(Si_{6·15} Al_{1·85})_{8·00}$ $O_{20·73}$	$(OH.F)_{4 \pm m}$ $(K.Na. . .)_{2 \pm n}$ $(R‴, R″, Ti)_{4 \pm x}$ $\{Si_p(AlFe‴)_s\}_{p + s = s}$ $O_{24 - (4 \pm m)}$

The computed formula for this Malvern species shows a loading

of the Al-layer beyond the equivalent of Al_4. If this proves to be invalid, the Ca and Mg in excess would enter the (KNa)-group. The Malvern example would then contain a high content of (KNa . . .), and a low content of (OH.F.O.). On the other hand, hydromuscovites are characterised by a low (KNa . .)-content and a high content of (OH.F.O).

Mention has been made above (p. 646) to the occurrence of natural clay-species containing kations in addition to the usual Al and Si. In some measure, the kations of clay-substance (in soils for example) are mutually exchangeable. If a " mixed " (KNaCa)-clay be shaken up with, say, KCl-solution for a sufficiently long period, K-ions from this solution are exchanged against an equivalent of (NaCa)-ions in the clay.

This property of base-exchange was first observed in clay soils by Thompson in 1845 [16] and was studied by Way (1850–54) [17], who attributed it to the bases loosely combined in obscure " hydrated alumino-silicic acids " assumed to be present in clay soils. Since 1858, when Eichhorn [18] observed this property in certain zeolites, base-exchange capacity has been found to vary among clays and zeolites alike, while the list of base-exchange minerals has been considerably extended.

Until quite recent years, the exchange-capacity of clay soils in particular was explained solely by appeal to either the " zeolite " or the " alumino-silicic acid " hypothesis—with a proviso that the property might conceivably be vested in kations physically adsorbed in amorphous hydrates or gels. Its bearing on several geological problems has recently been discussed by Boswell [19], who cites relevant literature.

The new conception of layer-lattices has stimulated soil-chemists, physicists and geologists alike to re-examine this property of base-exchange, the mechanism of which is, however, still uncertain. Yet each attempt to correlate the property with some peculiarity of lattice-structure has revealed the latter in a new and interesting light.

Marshall, for example [4], reviews the case on the following lines :

(a) Finding that colloidal clay-particles orientated in the electric field behave as true crystals, and show a pronounced birefringence which varies with the kations concerned in the base-exchange, he concludes that these exchangeable kations must have definite places in the lattice—must, in fact, be chemically combined, not merely adsorbed ; whence, he argues, the peculiar behaviour of these bases must depend on some peculiarity in the lattice-structure itself.

(b) The association of base-exchange capacity with lattice-shrinkage and lattice-expansion in the case of the bentonitic clays,

and the essential failure of both these properties in the case of kaolinite and its isomers, leads him to suggest a close connection between the two properties. He points out that lattice-expansion would provide room at high water-content for kations to move as freely in and out of the lattices as they do through the lattice-corridors of zeolites. He therefore suggests that the exchangeable bases are those which can enter and leave the lattice via this molecular water, and that these kations assist not only in neutralising charges on the lattices but also in maintaining the layers in their correct relative positions.

Edelman, however [20], advances considerations which would appear obvious on first principles. He points out that the edges of the cleavage-planes of kaolinite (in common with the bentonitic species) can never be out-balanced in detail, so that unsaturated valencies favourable to the attachment of bases should be present. Hence, exchange-capacity must in some measure depend on free edges as well as upon some undetermined peculiarity of internal structure. He points out that drastic grinding, which increases the proportion of both unbalanced edges and potentially reactive surfaces, may strengthen the exchange-capacity of reactive species and even raise a normally inert mineral to the status of a base-exchange species.

Van der Meulen [21] would explain the property from yet another view-point :

He finds that the exchange capacity of silicates is closely related to their content of Al-atoms replacing Si-atoms, and Edelman [20] agrees it is remarkable that Al-free pyroxenes and amphiboles show little or no base-exchange capacity ; whereas aluminous species containing Al-atoms in proxy relationship to Si-atoms do exchange bases. It may be objected, however, that Van der Meulen's hypothesis does not explain the occasional exchange-capacity of halloysite, which contains no Al-atoms in proxy relationship to Si-atoms ; nor would it account for the relegation of the micas and chlorites to the group with only " moderate " base-exchange capacity.

Edelman finds the main explanation in peculiarities of cleavage-surfaces. By comparing the ideal lattices of kaolinite, muscovite, and halloysite, he shows that the cleavage-planes present two configurations :

1. A sheet of O-ions bound to Si.
2. A sheet of (OH)-groups bound to Al.

As neither of these ideal surfaces appears to favour base-exchange, he concludes that such sheet-patterns are essentially insensitive to kations in contact with them.

Turning next to halloysite, he shows that the cleavage-surfaces comprise configurations 1 and 2 above together with a third :

3. A sheet of (OH)-groups bound to Si.

This he regards as the active layer, and the mode of bonding is thus co-ordinated with the constitution of an alumino-silicic acid : both engage (OH)-groups in which hydrogen is replaceable by bases. In this sense, halloysite would rank as a true acid co-ordinate with a hydrogen-clay, which can be artificially prepared from a bentonitic clay by extracting the bases with weak (below $N/200$) hydrochloric acid, and subsequent dialysis.

To explain the exchange-capacity of muscovite (and the felspars) Edelman points out a peculiarity of the mica-lattice :

$$\text{In the ideal case, muscovite is built up of layers } \begin{matrix} \text{Si} \\ \text{Al} \\ \text{Si} \end{matrix} \text{ bound}$$

strongly together by common O-ions, and the distribution of the K-ions is assumed to be completely regular. On the other hand, Al-atoms which replace Si-atoms must (he argues) be irregularly distributed ; hence the K-ions cannot be everywhere equivalent as regards the forces binding them to the lattice.

But the mica-cleavage goes over the K-ions which, though not equivalent, must nevertheless be equally distributed over both the upper and lower cleavage-surfaces. Statistically, therefore, the surface to which any particular K-ion is attached claims only two-thirds of the valency-bond ; the cleavage-gap gets the remaining third. On this purely statistical basis, areas on the cleavage-surfaces must be differentiated from one another in such a way that some are more, others less, favourably situated for base-exchange. This condition is in strong contrast with the uniform balanced surfaces of kaolinite and pyrophyllite. In all these cases, allowance must be made for the consequences of incomplete outbalancing of valencies along the torn edges of cleavage-surfaces.

Complete base-exchange is proved only if particular atoms can be experimentally introduced and subsequently replaced. Both these operations are practicable with some clays, hydromuscovites, and many zeolites ; yet in the case of these species, to which a great amount of co-ordinated X-ray, analytical, and other work has been devoted, the actual mechanism of base-exchange is still uncertain. This uncertainty is more likely to be resolved by persistence in the field of the clays, etc., than by speculations extending to more refractory and less thoroughly studied groups such as the amphiboles and biotites.

The constitution of the basic micas, notably the biotites and

varieties rich in lithium, though broadly understood, presents uncertainties which are still matters of current investigation.

In phlogopite, the layers are stacked according to the plan $\begin{matrix} Si \\ Mg, \\ Si \end{matrix}$ and the constitution is generalised as $(OH.F)_4.K_2.Mg_6.(Si_6Al_2)O_{20}$.

The biotite-constitution is tentatively written as a derivative of the phlogopite formula thus: $(OH.F)_4.K_2.Fe_6.(Si_6Al_2)O_{20}$ or $(OH.F)_4.K_2.(MgFe)_5Al.(Si_5Al_3)O_{20}$.

As in the case of muscovite, the group $(OH.F)_4K_2$ commonly takes the form $(OH.F)_{4\pm x}K_{2\pm y}$, and this variation entails variation in the quantitative relationships between other components. In particular, it cancels any assumption that O_{20} is a fixed quantity, since $(OH.F.O) = 24$, which is a constant.

Similar considerations apply to chlorite, for which the simplified general formula is $(OH)_8.Mg_5Al.(Si_3Al)O_{10}$, with $(OH.O) = 18$ as a constant [3].

The possibilities of atomic replacement in both the micas and the chlorites endow these constitutional formulæ with considerable elasticity, which in large measure accounts for the wealth of " species " and " varieties " included in the two groups.

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THE APPLICATIONS OF FLOATING EQUILIBRIUM TO THE DETERMINATION OF DENSITY

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Archimedes was puzzled until one day, stepping into a bath he observed the water running over. "Eureka," he cried. . . .

The Legend of the Crown of Hiero.

WHILST doubtless the facility of obtaining water enriched with D_2O has greatly accelerated the remarkable spate of researches involving heavy water and hydrogen-deuterium exchange reactions, an important contributory factor has been the comparative ease with which it is possible to measure the deuterium content of even small samples of water by physical processes of analysis which are adaptable to routine testing, require no elaborate apparatus and, of course, involve no loss of material.

Of the many methods employed from time to time to this end [1], that most commonly adopted is the so-called float-temperature method. In an admirable recent account by Briscoe and his collaborators of the apparatus and technique involved in the measurement of the density of 150 c.c. samples to an accuracy of one part in 5 millions [2], the authors remark " a critical comparison . . . led us to adopt the method of flotation [1] which was used for accurate determinations of the density of ice 72 years ago by Dufour [3]."

In point of fact the method has a far older and predominantly English pedigree and, in view of its manifold applications (*q.v.*) it seems worthwhile to attempt a brief account of the flotation method, its origins and development.

Although there are two distinct versions of the familiar school-

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[1] In this context the German expression "Methode des Schwebens" with its implication of "poise" and "hovering" is more felicitous than the English "flotation" which inevitably suggests floating, *i.e.* partial immersion. The alternative nomenclature—"suspension method "—is also open to misinterpretation.

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book story of the Crown of Hiero [4], it is not disputed that Archimedes (212–287 B.C.) laid the foundations of the science of hydrostatics, and indeed in Book I, Proposition 7, of his " περὶ ὀχουμένων," he formulated the principle—" A solid heavier than a fluid will . . . when weighed in the fluid, be lighter than its true weight by the weight of the fluid displaced "—now known by his name [5].

From this principle it follows that the smaller the specific gravity of a liquid, the deeper a floating body will sink in it. This constitutes the theory of the areometer [1] or hydrometer stated by Synesius Cyreneus [6] to have been invented by the woman philosopher Hypatia of Alexandria (*circa* A.D. 350). The earliest history of this instrument is, however, most obscure and an exceedingly detailed account of one appears in the poem " De Ponderibus et Mensuris " [7, 6], which may well have been written some three centuries earlier. Its discovery may even be due to the legendary inventive genius of Archimedes himself.

In the *Book of the Balance of Wisdom*, a treatise on hydrostatics and the practical determination of specific gravities composed (probably) by 'Abû-Jafar'al-Khâzinî at the court of Saljûke Sultân Sanjar in Baghdâd between A.D. 1117 and 1157, appears a detailed description [8] of the construction, calibration and method of using an instrument described as " the areometer of Pappos the Greek." This instrument which comprised a hollow brass cylinder of stated dimensions (diameter two fingers and length half a hand cubit) was weighted internally at one end so that it floated upright and sank down to the marked " equator of equilibrium " when placed in water " of some known stream of city or valley . . . such as the Jaihûn of Khuwâragin . . . or the Euphrates." That the influence of temperature when determining specific gravities with the aid of the hydrostatic balance was fully realised is implicit in the directions that " the water-weight of metals must be determined in early autumn with water of middling coldness."

Although some form of areometer must surely have been used in the ensuing centuries, its early European history has not yet been traced. Indeed, at the beginning of the seventeenth century, the outworn hypothesis of Aristotle, that the floating and sinking of a body depend mainly upon its shape is again current and we may read [9] of Galilei's well-contrived experiments to disprove this in his *Discourse on Floating Bodies* (Florence, 1612).

The re-discovery, or rather the re-introduction of the areometer, is commonly ascribed to de Roberval (alias Gilles Personne), the celebrated French physicist who in 1670 invented the modern

[1] From the Greek ἀραιός, thin ; and metaphorically, fluid.

platform-scales. The first account of a hydrometer published in England in modern times dates back to 1675 when the ubiquitous Robert Boyle described his "New Esſay Instrument" [10] : this device, intended for the detection of counterfeit coins, differs from the later Nicholson's hydrometer in details only. At the beginning of the paper Boyle proposes to "give you *now* a more explicite and particular Account than I *then* had time to do, of the Inſtrument which you ſaw tried at the Royal Society. . . . You may remember, that many years ago [1] I ſhew'd you a little glaſſ-Inſtrument conſiſting of a bubble, furniſhed with a long and ſlender ſtem, which was to be put into ſeveral Liquors, to compare and eſtimate their ſpecifick Gravities." A sketch of this instrument appears in the concluding Plate (*ibid.*, p. 340) with the legend :—" Fig. 8. ABCD, the ſmall Glaſſ-Inſtrument for eſtimating the ſpecifique gravity of Liquors. (of which an account may be expected in our next.) EE, the Quickſilver or Water that is imployed as Ballaſt to ſink it in an erected poſture." The promised account cannot, however, be traced.

The original design of Boyle's instruments have since undergone many changes of detail in attempts to improve their accuracy or to adapt them to specific purposes [11, 12], yet all the lineal descendants—the hydrometers of Beaumé, Nicholson, Twaddell and Sikes, the Bates' Saccharometer and Dicas lactometer, etc., and even hydrostatic balances like those of Jolly and Westphal have all a common weakness which strictly limits their value as precision instruments. Since part only of the instrument is immersed, there is a pronounced liability to error in consequence of the surface tension at the liquid-air interface [11]. This error is naturally reduced as the length of the wetted surface is decreased and Kohlrausch [14] found that the large and irregular variations due to surface tension could be minimised by working with a float of 900 c.c. volume suspended from a balance by means of a very thin platinum wire coated with platinum black. Such measurements of density which Kohlrausch claimed to be accurate to 1 part in 5 million were confirmed by Ditjer [15] and by Lamb and Lee [16].

The Gordian knot of this problem was cut, as it were, by Reggiani using a method suggested by Professor Pisani which eliminates the use of a suspension wire altogether by employing a totally immersed sinker. After preliminary work by Cavazanna in 1886 [17] two methods were developed for studying the seasonal variation in the density of sea-water from the Mediterranean Sea. In one

[1] Probably May 1662 [13].

method distilled water was added to the sea water until a sinker of known specific gravity just attained floating equilibrium in the body of the liquid : in the other this equilibrium was reached by slipping small weights in the form of platinum rings over the stem of the sinker. Nansen [18] who developed the same idea independently in 1896 while studying the salinity of water in the North Polar basin used small variations of temperature to attain final equilibrium, and Dr. Warrington [19] who examined the method in 1899 claimed an accuracy of 1 part in several hundred thousand. The method was next adapted by Richards and Shipley [20] for the " Quantitative Analysis of Solutions by Precise Thermometry " and slightly later more detailed studies of its possibilities were published [21], density measurements accurate to 1 part in 5 million being claimed. Substantially the same methods are used in later high-precision work [2].

In order to extend the range of densities over which any one sinker may be employed at a fixed temperature, Lamb and Lee [16] enclosed a soft iron core inside a 250 c.c. glass sinker which terminated at its lower end in a fine platinum point. The water or solution under investigation occupied a 4-litre beaker placed in an 80 litre thermostat regulated to $\pm 0 \cdot 005°$. When thermal equilibrium had been attained, platinum weights were placed in a small depression in the upper part of the sinker until it barely floated. Then, by passing a current through a solenoid placed centrally underneath the inner vessel, the sinker was attracted downwards till its platinum point entered a tiny conical saucer 6 mm. deep and 25 mm. in diameter. The current through the solenoid was then gradually reduced and the current noted when the platinum point just left the contact saucer—an observation facilitated by a special microscope immersed in the liquid. Using this combination of electro-magnetic attraction for the finer variations and milligram weights for larger alterations in buoyancy, one sinker could be used for density determinations accurate to one part in 10 or 20 millions.

To attain the same end, K. Kuhlmann [22] has recently revived the principle of the Cartesian Diver, and using floats with somewhat flexible walls, attains approximate floating equilibrium by temperature regulation and exact equilibrium by adjusting the external pressure. Substantially the same idea had been embodied by Polanyi and Gilfillan [23] in their micro-pyknometer, some years previously.

Another, though cruder solution of the same problem was reached by Alexander Wilson (1714–86, the first Professor of Astron-

omy in the University of Glasgow), who proposed the use of series of areometrical glass beads or hollow balls. As subsequently improved and patented by Mrs. Lovi, sets of beads were constructed in which each bead differed from the next in density by 0·002 units. When cast into a liquid of unknown density all beads denser than the liquid sank, and all of lesser density floated : a bead of density exactly equal to that of the liquid would float totally immersed in any position. In any case the density of the liquid must lie between that of the lightest bead that sinks and that of the densest bead that floats : a finer adjustment could be made by slightly raising or lowering the temperature since, owing to the marked difference between the coefficients of expansion of liquids and solids, the density of the liquid changes much more rapidly than that of the solid submerged in it.

Wilson's Beads may, to some extent, be said to have had their precursors in the thermometric beads employed by the Academicians of Cimento (1657–67) for their fifth thermometer. In the contemporary translation of Richard Waller [24] we are told to place a 100-degree thermometer in a vessel filled with Spirits of Wine cooled by ice or snow and to add " many little glaſs *bubbles* blown, and *Hermetically* Sealed at a Lamp ; theſe by reaſon of the *Air* in them, will keep themſelves floating upon the surface of the Water, and if by chance any one being a little heavier in *Specie* than water, ſhall ſink to the bottom, take it out, and upon a plate of Lead, with fine *Emeril* grind off ſo much of the end as will make it light enough to ſwim." The temperature is now allowed to rise when " the Balls (which at a more intenſe degree of Cold kept juſt upon the Surface) ſhall begin to dive toward the bottom. . . . That Bubble or Ball thereof which ſinks when the *Thermometer* is at 20 *deg.* ſhall be reckon'd the *firſt*, that is, the heavieſt, becauſe it deſcended when the Water was yet very cold." Proceeding in this way with gradually increasing temperatures, 10 balls were prepared, one for every 10 degrees between 0° and 100°, and it " do well, if they were of *coloured Glaſs*, to be more diſcernable."

A submerged float may thus be used both to determine the exact temperature of a liquid of known density [21] and conversely to determine, at constant temperature, the density of a liquid—and hence its composition [20]. The related problem, that of determining the density of a solid,[1] can be solved by making a mixture

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[1] The use of the hydrostatic balance for this purpose probably dates back to Archimedes' era. The " Balance of Wisdom " [8] is typical of those in use in the twelfth century and designed primarily for dealers in precious metals and gem-stones " to make a Trial of their Wares."

of two liquids of greater and less density respectively than the solid, in such proportions that a homogeneous fragment will just remain suspended therein, neither rising nor falling. The density of the liquid mixture is then determined at the same temperature by means of a pyknometer or hydrostatic balance.

This " Methode des Schwebens " was used so extensively by Jan Willem Retgers in his crystallographic researches that this classical method is often called by his name.

At first some limitations were imposed as a result of the necessity of having a suitable inert liquid of density greater than that of the solid investigated. This difficulty was gradually surmounted by the introduction of saturated aqueous solutions of potassium mercuric iodide (D., 3·196—Thoulet, and Sonstadt), cadmium borotungstate (D., 3·28—Klein) and barium mercuric iodide (D., 3·58—Rohrbach, 1883),[1] which could be diluted with water, or such organic compounds as bromoform (D., 2·88), acetylene tetrabromide (D., 3·0) or methylene iodide (D., 3·23—Brauns, 1886) suitably diluted with benzene. By adding iodine and iodoform alternately to methylene iodide, Retgers [25] obtained a solution with a density as high as 3·65.

Before the introduction of such liquids Thoulet [26] recommended embedding a known weight of the mineral in a sufficient known weight of wax of known density to give a compound float of density between 1 and 2 : this density could be determined by flotation in the usual way whence that of the mineral could be calculated.

In the method elaborated by (the late) Professor Sollas, a diffusion column in which the density increases regularly with increasing depth is obtained by leaving methylene iodide overlaid with benzene in a closed tube for some hours. On introducing a fragment of the given solid M it sinks until it reaches a layer of the same density : areometric beads, or crystals or mineral fragments of known density are now added (v.s. Wilson's method) and the positions of the two nearest fragments A and B lying respectively above and below M are noted by means of a vertical scale. Knowing the distances AM, MB and the densities of A and B, that of M can be calculated with some accuracy.

Now the rate (v) at which a (spherical) particle of radius r and density d_s rises or falls in a liquid of density d_1 and viscosity η is given by Stoke's Law : $v = 2r^2g(d_1 - d_s)/9\eta$, where g is the acceleration due to gravity. As the difference between the density of

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[1] Suschin had already described this heavy liquid in 1879, but his paper was in Russian and overlooked. *Cf. Zeit. dtsch. geol. Ges.*, 1885, **37**, 869.

solid and liquid is reduced, the rate of sedimentation decreases so that near the point of floating equilibrium the relative motion will be minimal—especially in a medium of high viscosity—and the disturbing effects of convection currents will be most pronounced. However, by increasing the magnitude of the gravitational field the rate of rise or fall of the suspended solid will be correspondingly enhanced and using a centrifuge to this end, C. B. Hendricks [27] has incorporated this idea into the accurate determination of the densities of single crystals. Miss D. Crowfoot and J. D. Bernal [28] have ingeniously developed the method for microdensity determinations for X-ray work : with 0·05 mg. or less of powdered crystal centrifuged at 2000–4000 r.p.m., equilibrium could be reached in 1–2 minutes and the mixture of equal density, in which no rise or fall of the suspended crystals occurred, could be prepared with some ease—even with the highly viscous sugar solutions used in conjunction with certain hydrocarbons of high molecular weight. The same principle has been employed by biologists for measuring the density of living cells.

So far from claiming any priority for the discovery of the " flotation method," Retgers [29] credited Louis Dufour (1832–92, sometime Professor of Natural Science at Lausanne) with this achievement. Thus he writes : " So viel ich ersehen konnte, ist das Prinzip des Schwebens fester Körper in gleich dichten Flüssigkeiten zum ersten Mal angewendet bei der spezifischen Gewichtsbestimmung des Eises von Dufour [3] welcher Stücke luftfreien Eises in einer Mischung von Petroleum (spez. Gew., 0·82) und Chloroform (spez. Gew., 1·50) zum Schweben brachte. . . . Auf Flüssigkeiten hat man die Suspensionsmethode schon längst angewendet. Dufour benutzte dieses Verfahren sehr geistreich zur Dichtebestimmung von Tropfen überschmolzener Körper, so dass er wohl mit Recht als der Vater der Suspensionsmethode zu betrachten ist, wenigstens wenn man nicht Plateau mit seinen in wässrigem Alkohol schwebenden Oeltropfen als solchen ansehen will." [1]

Tacitly accepting the accuracy of Retger's statement, W.

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[1] " So far as I can discover, the principle of floating solid bodies in liquids of the same density was employed for the first time by Dufour who, when determining the Specific Gravity of ice, brought pieces of air-free ice into floating equilibrium in a mixture of petroleum (Sp. G., 0·82] and chloroform (Sp. G., 1·50). . . . The suspension method has been used for liquids for some time past. Dufour very cleverly applied this procedure to determining the density of drops of superfused substances, so that he ought certainly to be regarded as the Father of the suspension method—provided, at least, that Plateau with his drops of oil suspended in aqueous alcohol is not to be considered as such."

Ostwald [30] wrote in the first edition of his *Hand- und Hilfsbuch zur Ausführung Physikochemischen Messungen*; " . . . Für unsere Zwecke ist keine Methode der Dichtebestimmung bei festen Körpern geeigneter, als die zuerst von Dufour angegebene " Methode des Schwebens ".[1] In all subsequent editions [31], however, the name of Dufour is replaced without further comment by that of Davy, whilst Richards and Harris [21] also expressed the opinion that " the use of floating equilibrium in determining specific gravity was perhaps first suggested by Davy in his work on the density of the alkali metals."

Evidently, Richards and Harris refer to the Bakerian Lecture to the Royal Society (1807) in which Davy refers to the density of the element potassium, which he had just isolated, in the following words [32] : " Resembling the metals in all these sensible properties, it (potassium) is however remarkably different from any of them in specific gravity ; I found that it rose to the surface of naphtha distilled from petroleum, and of which the specific gravity was ·861 and it did not sink in double distilled naphtha, the specific gravity of which was about ·770, that of water being considered as 1."

It is clear that this extract in no way supports the contention that Davy was the first to employ the flotation method—with, for example, potassium suspended in a mixture of these two solvents —and Professor Ernst Cohen [2] in his monograph [33] *Wien danken wij de " Zweefmethode " ter bepaling der dichtheid van vaste stoffen ?* concludes that Richards and Shipley were in error. This impression was doubtless confirmed by the succeeding sentence, " I endeavoured to gain approximate information on the subject by comparing the weights of perfectly equal globules of the basis of potash and mercury."

However, on page 21 of the same lecture Davy refers to the newly isolated basis of soda (sodium) as follows : " It swims in oil of sassafras of 1·096, water being 1, and sinks in naphtha of specific gravity ·861. This circumstance enabled me to ascertain the point with precision. I mixed together oil of sassafras and naphtha, which combine very perfectly, observing the proportions

[1] " For our purpose, no method of determining the density of solid bodies is more suitable than the ' flotation method ' which Dufour was the first to describe."

[2] The writer wishes to express his indebtedness to Professor Ernst Cohen for his kind permission to quote from this short Dutch monograph. Though in error in this one respect, Professor Cohen has none the less established beyond cavil that Davy was anticipated in this discovery by Thomas Thomson (*q.v.*).

till I had composed a fluid, in which it remained at rest above or below ; and this fluid consisted of nearly twelve parts of naphtha, and five parts of oil of sassafras, which gives a specific gravity to that of water, nearly as nine to ten, or more accurately ·9348 to 1."

Here then is definite proof that Davy used the principle of flotation in 1807 although admittedly he appears to have determined the density of the flotation liquid by calculation rather than by direct measurement.

Now as mentioned above, Dufour certainly employed the suspension method in his comprehensive studies on the density of ice [3]. In one paper [34] he remarks : " Le principe de cette méthode, *qui se présente d'ailleurs assez naturellement à l'esprit de chacun*, à déjà été appliquée par Thomson suivant H. Kopp."

The words italicised inevitably recall the oft-quoted lines from Milton's *Paradise Lost* :

> Th'invention all admir'd, and each, how he
> To be th'inventor miss'd ; so easy it seem'd
> Once found, which yet unfound, most would have thought
> Impossible.

For the first account of the application of the suspension method Kopp refers rather indefinitely to Thomas Thomson's text-book, *A System of Chemistry*. This book, first published in 1802, was widely read on the continent for many decades, its popularity and importance being due in no small measure to the fact that in the third edition (1807) appeared for the first time a comprehensive account of Dalton's Atomic Hypothesis.

The question now arises in which of the various English editions Thomson first refers to his use of the suspension method. Should this occur in any edition printed prior to November 19, 1807 (the date on which Davy read his paper to the Royal Society), then it is clear that the discoverer of the alkali metals cannot be credited with priority in having also discovered the flotation method.

The first edition of *A System of Chemistry* (Edinburgh, 1802) contains nothing pertinent to the point at issue. In the second edition (Edinburgh, 1804), however, in which the article " Of Caloric " is treated more fully than in the previous edition, Thomson discusses the phenomenon exhibited by some liquids of expanding on freezing, and investigates in particular the transformation of water into ice. On page 353 he writes : " I tried various methods to ascertain the specific gravity of ice at 32° ; the one which succeeded best was, to dilute spirits of wine with water, till a mass of solid ice put into it remained in any part of the liquid without

either sinking or rising I found the specific gravity of such a liquid to be 0·92 ; which of course is the specific gravity of ice, supposing the specific gravity of water at 60° to be 1."

Here then is the clearest possible proof that Thomson must be recognised as the discoverer of the flotation method. In this connection how singularly apt are the following lines quoted from the Foreword to the first edition of his own *System of Chemistry* : " The labours of our philosophers have been frequently overlooked and their discoveries claimed by others to whom they did not belong."

Among various applications of the principle of floating equilibrium, mention must be made of Briscoe, Robinson and Stephenson's work [35] on the isotopes of boron. Boric oxide from various sources was fused into clear glass beads the density of which could be determined with an accuracy of 1 part in 10,000 by flotation in a mixture of methylene iodide and pentachloroethane at temperatures between 17° and 19·5° C. : variations of atomic weight from 10·847 to 10·806 were detected in this way. In another field, Riesenfeld [36] was able to establish the identity of three samples of (alleged) triamminoperchromate CrO_4, $3NH_3$ despite variations in crystal habit, for in a mixture of ether and acetylene tetrabromide D., 1·964 at 15·8° C., all the crystals sank at 17° and all of them floated at 14°, proving that to within 0·1 per cent. at least, the densities of needles and plates were identical.

The fact that the density of the aqueous sulphuric acid in the lead accumulator falls gradually from about 1·20 to about 1·17 during discharge forms the basis of a number of familiar commercial devices for indicating the state of charge. Some makers employ one or more differently coloured balls of graded density, while others prefer an ingenious compound float made from two coloured balls of different density : these may float horizontally at the surface, or vertically at the surface, or sink vertically to the bottom, or lie flat on the bottom—positions corresponding to progressively diminishing density and indicating " Full Charge," " Three-quarter Charge," " Quarter Charged," and " Discharged." In yet another device the float is arranged to move a submerged lever over a scale bearing the appropriate legends.

Many attempts—some of comparatively recent date [37]—have been made to improve the accuracy of the flotation method as practised by Retgers. Since these improvements are mainly directed towards some thermostatic device for avoiding the disturbing effects of convection currents and arrangements for withdrawing the liquid of equilibrium density into a pyknometer without change of composition, it seems imperative to conclude by

referring to the admirably simple and accurate modification of Retger's method developed by Andreae [38], especially as this technique and that of the alternative pyknometric method (*loc. cit.*) has not received the attention it deserves [39]. Andreae first prepares a suitable mixture of liquids in which the given solid, a fragment of about 1 mm. diameter, attains approximate floating equilibrium. The mixture is then introduced into a dilatometer with a graduated stem of some 2 mm. internal diameter which is placed in a thermostat. The temperature is now slightly raised or lowered until the cessation of the vertical motion of the suspended solid in the stem indicates that exact floating equilibrium has been reached : convection currents are hereby eliminated. Since at this temperature the density of solid and liquid are identical, the volume occupied by the contents of the dilatometer would be unchanged were part or all of the solid present to be replaced by the liquid, and vice versa. This volume is read off directly from the calibration marks on the graduated stem and the dilatometer and its contents are removed from the thermostat, dried and weighed : knowing the weight of the empty dilatometer, the density of the solid follows immediately. The vessel in which floating equilibrium was attained serves also as the dilatometer : there is no transference of liquid with its attendant disadvantages and the density of the liquid of equilibrium concentration is determined at the same temperature —and even the same time—as floating equilibrium is attained.

The principle of Archimedes is consummated in the practice of Andreae of which it may surely be said *artis est celare artem.*

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BITUMEN : ITS SOURCES, DEVELOPMENT AND USE ON ROADS

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THE most recent and officially accepted meaning of the term " bitumen " includes both bitumen from oil, produced quickly in the refinery or slowly in natural conditions, and tar obtained by distillation of coal, because both materials contain bitumen, according to chemical definition. Both bitumen and tar are excellent road materials when properly used, and are of more interest than ever on account of the Government's schemes of road development.

The object of making a road is to enable traffic of all kinds—automotive and horse-drawn, pedestrians and animals—to proceed easily on their way without damage to Mother Earth. The care of roads and bridges was carried out in medieval times under the direction of the monasteries, as a " good work." When these were dissolved conditions deteriorated till the days of Telford and Macadam, whose splendid planning and design was seriously interfered with by the development of the railways. Later, with the advent of the motor-car, a complete reconsideration of the whole problem had rapidly to be made, as power-wheels began the rapid destruction of water-bound surfaces. This led to the experimental use of tar (originally for dust-laying) and of bitumen as a binder for the road metal, and the chemist and physicist made their first appearance in road technology. It took some time for the proper use to be made of the scientists, but their position is becoming established, though their work could, even to-day, be more widely extended.

Present-day developments have led to a clear differentiation between the two fundamental ideas on the nature of the ideal road. There are those who advocate the use of asphalt and tar-macadam on the grounds that they " give " sufficiently to absorb the stresses of traffic ; there are those who advocate concrete roads which, by their rigidity, resist the stresses of traffic. The latter cannot see why there should be any use for asphalt and macadam ;

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the former look on concrete as being really useful only as a foundation for their mixtures. In addition to the clash of these basic principles, the choice of the best road surface is rendered extremely complicated by considerations of prime cost, length of life, cost of maintenance, ease of reaching buried pipes and cables, the nature of climate to be resisted, nature of the traffic to be carried , as well as the vigour of propaganda, the personal representations of manufacturers and other firms, and the personal experience and predilections of the road engineer.

The view taken in this article is from the position of preference for the resilient road, of asphalt and tarmacadam. Asphalt is bitumen mixed with mineral matter, usually broken stone, sand, and a very fine material (filler) ; tarmacadam is tar mixed with roughly the same materials. One form of this mixture may be made in a plant, spread and rolled ; or it may result by the hot binding material being poured on to a suitable macadam road surface and allowed to penetrate into it. This general statement is sufficient for the present purpose, but it must be remembered that there are a very large number of variations within wide limits of properties and proportions and manipulation of the ingredients.

The astonishing thing is that bitumen and tar should have any use in road-making. A petroleum from only a few places, of which Mexico, Texas, and Venezuela are typical, is distilled and a continuous series of substances of continuously graded properties (of which the most important is a gradually rising boiling point) are removed until most of the " heavy oils " have been taken off, and bitumen remains. If a soft or hard bitumen is required, more or less of the heavy oils is allowed to remain : if the distillation is pushed too far, petroleum coke results. Bitumen is apparently only the end member of graduated groups of substances. And very closely analogous changes occur in the production of coal tar and pitch. Yet, following the changing nature of the lightest to the heaviest oils, there is an abrupt change in properties in the end material. This material is found to have developed a power of adhesion to stone (and other substances) that is absent in the next neighbours, the heavy oils, as well as a high viscosity which changes with the temperature in a manner that is of the highest importance in its employment ; and other remarkable properties as well. What has happened to cause this abrupt discontinuity ?

During the course of production, whether in the refinery or in natural conditions, certain highly complex chemical changes have taken place, which have resulted in the distribution through-

out the material of innumerable and extremely small particles of carbonaceous matter, so small as to have quite peculiar properties. These are the cause of the conversion of heavy oils into bitumen and tar. Looked at broadly, this section of physical chemistry, known as "colloid chemistry" ("the chemistry of minute particles") has a very wide range of technical and literally "vital" importance. This mixture of a liquid and colloidally fine solid particles is paralleled by a mixture of extremely fine liquid particles in another liquid. These are "emulsions," and emulsions of bitumen in water (and less of tar in water) are widely used in road work, mainly in penetration processes. Here water is used to carry the bitumen to the position required and to leave it there, which is the equivalent of heating the bitumen to a temperature and ease of flow that enables it to take its own position and to remain there by cooling.

The study of tarmacadam has shown that the choice of tar suitable for use, presents little difficulty; so that, with recognition that much excellent work is being done for its continual improvement it can, with courteous salute, be left for consideration of the more varied, and therefore more interesting, nature of bitumen and asphalt.

Refinery bitumen, as such, need not be considered further, except to note that in certain parts of the world, such as Mesopotamia, a naturally occurring heavy material seeps to the surface of the earth, that is somewhat like the hot material that flows from a petroleum still. Other forms of natural bitumen are found, associated with mineral matter. The most famous (such a statement is almost invidious) is the Rock Asphalt, such as occurs in the Val de Travers, by Neuchâtel, in Switzerland; as well as Seyssel in France, Ragusa in Sicily, Vorwohle in Germany, and Lobsann in Alsace. In all these asphaltic rocks, the bitumen is associated with beds of limestone to an average amount of 6–7 per cent. A suitable type of this material has the property of being ground to a powder and reconsolidating when heated and compressed; and also of forming mastic when mixed with a further quantity of bitumen, so that when hot it can be spread like porridge by hand by means of "floats."

Another naturally occurring material consists of bitumen associated with a porous sandstone or even loose sand. These are to be found in America (Kyrock), and in Canada in a form known as the Alberta Bituminous Sands (the older term of "Tar Sands" is now obsolete). Their use in road making is limited.

A further type of natural asphalt is that of a mixture of bitumen

with finely divided mineral matter from the size of fine sand to colloidally minute clay-like particles. A useful deposit is that at Selenizza, in Albania; but the world-famous material is the Trinidad Lake Asphalt. More will be given about this later, but for the moment it may be said that there is little in the world of technology that is so astonishing in its characteristics, its occurrence, and its history and development. It is admirable for its purpose, its formation is almost incredible (if one of the two rival theories is correct), and its development dawns with Christopher Columbus, restarts with Sir Walter Raleigh, continues with a family of sea captains and admirals, and blossoms with the magic touch of science.

Another interesting material, one that is striving for recognition and partially strangled by latter-day economics, is the natural asphalt found in the island of Boeton in the Dutch East Indies. There are several varieties, but that offered for roadmaking consists of bitumen through which is disseminated large quantities of limestone shells of minute sea organisms known collectively as "globigerina." One remarkable feature is its position, which allows it to be quarried from the side of a hill—as compared with the often complicated mining of asphaltic rock, and the surface digging of the Trinidad Lake.

It should be again stated that special reference to these materials —and such reference will be even more selective later—does not imply that they are the only good materials available for roadmaking.

The technique of the use of these materials is a difficult and tricky one, and the production of a good and lasting road surface is one requiring considerable experience. It is not a matter that can be learned out of a book, because there are an uncomfortable number of "variables" to be watched for and understood, and controlled. And, conversely, it redounds very greatly to the credit of the asphalt and tarmacadam manufacturers who, with the aid of the scientists, have disentangled these numerous simultaneous influences, so as to rely on attaining so high a proportion of first-class results.

Such work will only be followed here in outline, as purely technical detail would be boring in such an article as this : the following is sufficient. "Hot-process Asphalt" consists of sand and stone mixed together in such proportions and sizes as to form a skeleton of stone, wedged together and their interstices occupied with the sand : each particle is surrounded by, and the vacant spaces are filled with, bitumen that has been stiffened by an admix-

ture of a filler, a finely divided powder of one of many materials such as Portland cement, limestone, and others. These ingredients are well mixed, and spread and rolled hot. There is a great art in carrying out these various stages so as to obtain the best advantages from the mixture.

One aspect might be mentioned here, only for the sake of its queerness. It has been noticed that the best mixture for an American sand carpet (a type that is scarcely ever used here to-day) was : bitumen 12 per cent., filler 18 per cent., and sand 72 per cent. This was most elaborately worked out by the " numerology " mystics amongst the Rudolph Steiner followers, and correlated with many vital precesses of human life : because they were the same as the " vital " numbers was the reason for the excellence of the mixture. Apparently, experimental laboratories are no longer necessary ; all technologists retire, and only multiplication tables and analogies are necessary for progress in road surfacing.

" Compressed asphalt " and " Mastic " have already been referred to ; in these there is frequently an admixture of stone chippings to give stability to the mass and improve its non-skid qualities.

Let us now digress, for a little, and examine something of the origin and history of bituminous materials, and return afterwards to consider certain characteristics of the surfacings.

In my little book, *The Genesis of Petroleum* (1922), I discussed the various existing theories, and showed that the most acceptable was that according to which petroleum originated from the decomposition of animal and vegetable matter in perculiar conditions during geological periods of time. Work since that time has overwhelmingly confirmed this, with the exception of one and significant suggestion by J. E. Hackford, who worked on material which still contained intermediate products of the change. His theory, which he supported as far as was possible by laboratory experiments, was that bitumen resulted from the acid decomposition of algæ, and that oil was formed at the same time. Probably further work will reduce the divergence of ideas.

The formation of asphaltic rock is almost certainly a matter of simultaneous deposition of the limestone and formation of bitumen ; but in some cases subsequent infiltration is strongly indicated by the concentration of the bitumen being greater near the tectonic fissures in the limestone beds.

The formation of the Trinidad Asphalt Lake is so remarkable that there must be a few words about the lake itself to serve as a background for subsequent description. Imagine over 100

black irregular acres, of which " the edge shelves down rather like the Round Pond in Kensington Gardens " The creases and shallow channels across the surface preserve enough water permanently to give a habitation to small fish. Clumps of spare vegetation grow in isolated patches on the lake. The whole lies about 130 feet above sea level, and is reached by a winding road much of which runs through forest. Most astonishingly, statements are contradictory as to whether there is a sulphurous smell or not : variation of opinion may vary with variations of wind direction. The lake, in spite of its surface hardness, which is enough to support a man with safety (except in the middle), is in constant movement of circulation ; and objects such as ancient trees and bones come slowly to the surface and slowly disappear.

Such is the lake and its position ; and this position coincides so badly with the description given by Sir Walter Ralegh of his tour of the island to seek water and contact with the natives as to raise great doubts as to whether he visited the lake at all. He undoubtedly saw and examined the shore asphalt by the village of La Brea ; but if he had seen so remarkable thing as the lake itself he surely would have recorded having done so.

There are two theories of the formation of this extraordinary lake. The first is the older which has recently received a recrudescence of support in America (W. J. Millard). It depends on a rather elaborate condition of earth movements, associated with the bursting of a mud volcano through an oil-bearing stratum, whereby the mud became intimately mixed with the heavy oil. The second (Prof. V. C. Illing) is based on the simultaneous deposition of silt and sand with the upward percolation of the bitumen—another example of simultaneous formation—with associated earth movements. The latter theory has the attractiveness of simplicity and close affinity with the mode of other asphalt deposits. The former has the advantage of correlation with the Bermudez Asphalt Lake in Venezuela, across the water ; and also with the deposit of Maracaibo. All these lie almost exactly on the same degree of latitude, a little over 10° N. Although Trinidad asphalt bears a quantity of fine mineral matter in considerable excess of that in the other two, the chemical constitution of the Trinidad and Bermudez (pure) bitumen is identical.

In contrast to these theories is the Trinidad Indian's tradition : Formerly, the region about La Brea was ordinary land, and was selected by the Cheime tribe of Indians for their village, because of the numerous pine-apples, large flocks of birds, and abundant fish. But when the Indians wantonly destroyed the beautiful

humming birds, which were associated with the souls of their departed relatives, " the Good Spirit took awful vengeance upon them for their impiety and, in one night, made the whole encampment with its sacrilegious inhabitants sink beneath the earth. Next day there was nothing visible but the asphalt lagoon."

The commercial development of Trinidad asphalt is no less remarkable in its way for the alternation of keen observation and far-sighted anticipation with home neglect and lethargy.

The island was discovered by Christopher Columbus in 1498. He had vowed that the first land he found would be dedicated to the Trinity ; and one can imagine the holy thrill he must have felt when he saw, as the first sight of land, three hills appearing over the horizon. Nothing except trading seems to have taken place until the visit of Sir Walter Ralegh in 1596, when he was on his tragic journey to El Dorado. He found the asphalt at La Brea to be excellent for caulking ships. Again little attention was paid to the material till the early nineteenth century, when the Dundonald family, through its various naval members of Cochrane fame, being stationed in those waters, made acquaintance with the asphalt. But even their energy, together with an official geological survey published in 1858, failed to arouse British interest. At last, but only in 1864, did the American oil interests wake up to the possibilities of its usefulness and, with the continued connection of the Dundonalds, a series of activities occurred, which have permanently established Trinidad asphalt as a most important industrial material.

The asphaltic limestone rocks are older in use, but less spectacular in development. The Swiss deposits, by Neuchâtel, were known to the Romans, and attention was vividly drawn to them by that queer Greek adventurer, Dr. Eirinis of Eyrinys, in the early eighteenth century. Through his activities it was made known in Paris. Buffon, the naturalist, used it to line a basin " in the king's garden " in 1747. The main history of the exploitation of these deposits is relatively dull and sedate. The French deposits were worked at Seyssel by a concession of 1797, and at Gard by one of 1844. Here again the history of both was not very eventful, except for moments of official interference at Seyssel. The German rock at Vorwohle was discovered in 1601 by a woodsman who used a lump to protect his fire ; but only in 1868 was the first mine opened. Since the war it has taken the place in commerce of the Limmer deposit, a rock that was discovered about 1730. The only Italian deposit used for road work in this country is at Ragusa in Sicily, discovered in 1838 but not

worked seriously till 1880. Other deposits, useful to Italy occur at Abruzzi ; and she controls the Selenizza works in Albania.

These various examples of road asphalt have been chosen because, except for Seyssel, they are all used to a greater or less degree for roadmaking in this country ; but there are dozens of other deposits, all over the world, of only local or no importance, or used for other purposes.

We have now shortly considered the necessity for roads, the various types of bituminous surfacings, and the origin of bitumen and natural asphalts and their nature. Let us now see the relation of the finished road to the public.

A road must bear the weight of passing traffic, and resist the backward scoop of its power-wheels when proceeding, and the forward shove when braking to come to rest. It must last a long time—this for the sake of the Public's purse and for the avoidance of the Public's inconvenience through the necessity of frequent repairs and replacement. Such excellence is not always made use of, because the first cost of a road frequently appears to be greater in importance than the total cost of the road during its life.

The most important condition after—or perhaps before—that of remaining in use for a proper period is that it shall remain non-skid. Slipperiness is seriously detrimental to the safety of the motorist and the horseman, and it is very difficult to please both within the limits of the purse of the local council. The contractor wants a road that will " stay put " and remain waterproof, and this usually means an upper limit of bitumen to be present. Such a surfacing rapidly becomes slippery, so that the users complain with justifiable bitterness. A leaner mixture may be safeguarded by surface-dressing, that is, by treating with bitumen or tar of a suitable nature to hold fast small chippings so as to fill surface voids and to form a rough surface. This usually lasts a year but sometimes as much as three ; if it is badly done, it is quickly swept into the gutter by passing traffic of which the varnish may suffer badly. The latest non-skid surfacing consists of mastic or compressed asphalt, into which are rolled, at the right temperature (*i.e.* consistency), rather large stones, thereby producing a rough and firmly held surface, necessarily rather " open " but with a waterproof base.

This " micro-alpine " type of non-skin surfacing is being rivalled by the " sandpaper " type. In this, small and sharp particles of stone are allowed to jam themselves together, with bitumen or tar in so small proportion (about 5 per cent.) as to serve more as a lubricant than a binder. This was first developed in Germany in

the economical days after the war. The margin of safety of manufacture is rather narrow, and particular watchfulness is required to make the system useful in a wide range of conditions.

Straight tarmacadam is also widely used for its non-skid properties, when the heaviest traffic is not to be carried. Concrete is also liked, but on account of its better conductivity for heat, it is more liable to be very slippery when the temperature is low enough to give rise to ice on the surface or in the surface pores. It is sometimes surface dressed.

The measurement of skidding is being carried out in several countries on a principal worked out by this country. A wheel is attached to a moving vehicle, which can be turned about a vertical axis so as to give a varying and controlled amount of skid, of which the forces can be graphically recorded. The matter appears to be simple, but the extraction of the full advantage of the results obtained is not easy.

It remains only to consider the lines of progress along which bituminous roads have travelled. The original dust-layer was found to strengthen the road surface to some degree. Asphalt surfacing, in the form of modern manufactured mixtures, was devised in America by Clifford Richardson; it failed on its first trial in London in 1894, but was followed in 1896, after more research was done by him, by a success in King's Road, Chelsea, and a much finer one on the Victoria Embankment. Wider application to carry increasing traffic and combat climatic attack led to the use of what is now considered to be an excess of bitumen of the wrong hardness. This resulted in much corrugation and pushing in hot weather. After some time less bitumen of a softer grade was used; and more recently there has been a tendency to replace the classic combination of a lower coarse layer covered by a finer wearing course, by a single-coat construction of more average composition.

We have already met the later development of the fine-grained low-bitumen mixtures, and the coarse stone rolled into mastic or compressed asphalt. The main trouble of all non-skid surfaces in which the stone or chippings are " proud " of the main body of the mixture is that they diminish in efficacy with use, due to the mineral particles sinking into the true road surface. Too frequent renewal produces a thick superficial layer which is deficient in mechanical strength.

It is hard to foresee the direction of progress of the near future; it will probably be in the improvement of details and the recombination of principles and types rather than the evolution of new

principles. Prophecy is always dangerous, particularly in face of such queer events as the introduction of cotton fabric into bituminous roads : probably the most remarkable advance will arise from the experiments on the incorporation of rubber in bitumen and in tar, with the production of mixtures of unusual properties. The whole matter of road mixtures is undergoing continual investigation and improvement, and it is thought by some that less keen competition would lead to better prices being obtained and thus to more money being available for laboratory work, for it is only in laboratories that progress is hatched.

The situation is being regularised by a series of Standard Specifications of the British Standards Institution for various types of asphalt surfacings, and those for tarmacadam are being discussed. The matter is one of more than usual complexity and difficulty. On the whole the effect is good, in that all concerned are shepherded towards the best results, directly and indirectly ; but continual watch has to be kept for modification and improvement, as the technique develops, so as to avoid any influence towards stagnation and oppression.

RECENT ADVANCES IN SCIENCE

ASTRONOMY. By R. W. Wrigley, M.A., F.R.S.E., Royal Observatory, Edinburgh.

Star Catalogues.—In consequence of the recent spectacular advances in astrophysics, and the publicity given to various theories of the size and structure of the universe, there has been a tendency to ignore, if not actually to depreciate, the astronomical work of the last century, devoted mainly to the routine and humdrum meridian observations necessary for the compilation of stellar catalogues. Yet the fundamental importance of this work, uninteresting as it may sound to the readers of the numerous popular treatises on astronomy in which it is barely if ever mentioned, cannot be questioned. The knowledge of stellar proper motions, on which as a foundation the study of stellar dynamics must necessarily be built, can only be acquired from a comparison of reliable catalogues of widely different dates. The secular parallaxes which can be derived from them are of immense value for statistical purposes, and a star's absolute velocity can be deduced from its observed radial velocity only when its proper motion in addition is accurately known. The choice by Dr. A. Kopff of " Star Catalogues, especially those of Fundamental Character " as the subject of his George Darwin Lecture (*Monthly Notices, R.A.S.*, 1936, June) was therefore opportune, especially in view of the imminent publication by Benjamin Boss of the Albany General Catalogue. This monumental work, commenced over forty years ago, will contain about 32,000 stars, and will give their positions and proper motions referred to a revised fundamental system. The positions are estimated to have probable errors not normally exceeding 0·15″ in each co-ordinate, though for stars of high southern declination, where observations are fewer in number, the accuracy will fall short of this standard. The determination of a reliable system of reference has always been the great difficulty in the compilation of a stellar catalogue, for all the bodies visible in the sky are in motion. It is therefore necessary to attempt to fix the system of reference from observations of the very stars whose individual motions it

is desired to determine. The co-ordinates of any star depend on the positions of the equator and the equinox, and to obtain its place at any given date a knowledge of the constant of precession is necessary. The determination of this constant involves some hypothesis for the dynamics of the stars, and an ideal system of reference is therefore impossible. As Dr. Kopff pointed out, stars are observed at different epochs relative to a moving equator and a moving equinox, and by a comparison the total motion of any star in the interval is obtained. This motion is a combination of the motion of the system of reference and the individual motion of the star, and, if the constant of precession is changed, the derived proper motion is changed also. The precessional constant has hitherto always been determined on the assumption that the individual movements of the stars are completely at random, and, while at present the numerical value of the constant seems quite satisfactory, future knowledge of the laws governing stellar motions may necessitate some alteration. Dr. Kopff referred to a recent proposal to utilise the spiral nebulæ as points of reference on the ground that their immense distances and regular distribution in space would make their mean angular motion approximate to zero. He pointed out the impossibility of building a fundamental system on the spirals alone, for they cannot be observed over the whole sky with meridian instruments, and do not provide a homogeneous network of points of reference. But although the stars must be used to fix the desired fundamental system, the spirals should prove very useful as controls of the system, and accurate determinations of their positions are desirable for this purpose.

Dr. Kopff was mainly responsible for the revision of Auwer's fundamental system, and the result, known as the " Dritter Fundamental–Katalog des Berliner Astronomischen Jahrbuchs," has been published in the *Astronomische Nachrichten*, Nos. 5540 and 5802, and in the Appendix to the *Berliner Jahrbuch*, 1936. It was found that the systematic errors present in Auwers' and other modern fundamental catalogues were the result of the adoption of erroneous proper motions caused by the presence of systematic errors in earlier catalogues. In declination serious errors have been caused by instrumental flexure and by a faulty constant of refraction, and there has often been a seasonal influence owing to the stars being necessarily observed at different times of year. As regards Right Ascension, improvements in instruments and clocks have practically eliminated errors in present-day observations, but treatment of catalogues older than 75 years, unless they comprise a series observed over a long period with the same unaltered instru-

ment, is fraught with many difficulties, and Dr. Kopff considers that it is not at present possible to supply exact corrections to the proper motions which should have been made available from them. The value of his work was suitably acknowledged at the Paris meeting of the International Astronomical Union in 1935 when it was decided that his revision of Auwers' Catalogue should be adopted in future for the preparation of all the national Almanacs.

In future it is probable that the fundamental stars will be observed with meridian instruments, faint stars observed photographically, and spiral nebulæ given a regular place in the system. To connect the meridian and photographic observations Kopff wishes to have a standard catalogue of 30,000 stars, regularly distributed throughout both hemispheres, as many as possible being taken from the new Albany General Catalogue, but, as proper motions of sufficient accuracy will not be immediately available for all this number, he advocates the selection of about 3000 stars between the sixth and ninth magnitudes to form a preliminary fundamental catalogue. These would not only provide a system of reference for the whole 30,000 stars, but could also be used to determine the systematic errors of stars of similar magnitude in earlier catalogues, compiled when observers made no attempt to eliminate magnitude errors by the use of screens or other devices.

Considerable progress is being made with the re-observation of the Astronomische Gesellschaft Zones, averaging about six stars in a square degree, and originally observed in 1880. Photographic plates covering 25 square degrees are now being employed. They contain, on an average, 17 reference stars apiece, observed with meridian circles. The work is being done by the Astronomische Gesellschaft in co-operation with the Yale and Cape Observatories. The comparison stars, numbering many thousands, are to be observed at least six times each. At Yale Observatory an experiment is being tried with plates covering 110 square degrees, a diffraction grating being placed in front of the telescope objective, and first-order spectra being measured. This method permits the use of brighter stars than would otherwise be advisable, and the available number of suitable comparison stars is therefore greatly increased.

VISUAL BINARY STARS.—Various theories have been put forward from time to time regarding the formation and evolution of binary stars, and most of them make use of an observed relation between the periods and the eccentricities of the orbits, which appear to increase together. This correlation is regarded by Dr. R. G. Aitken

(*Monthly Notices R.A.S.*, **92,** 7) as "not only to be established beyond reasonable question, but also probably to have a physical basis, related in some way to the origin and evolution of the systems." From the consideration of 226 orbits of spectroscopic binaries and 103 orbits of visual ones he found that, while the members of the former group with the shortest periods had nearly circular orbits, the visual orbits which accompanied long periods were greatly elongated ellipses. A more extended investigation by Russell, in which wider pairs, for which complete orbits were not available, were included, found the average eccentricity of 500 pairs with average period of 2000 years to be 0·61. For nearly 800 more pairs whose average period was estimated to be roughly 5000 years the average eccentricity was as great as 0·76.

But no satisfactory explanation is yet forthcoming as to the way in which a rotating fluid mass, even if it splits into two bodies revolving nearly in contact round their centre of gravity, can ultimately result in a system with the huge orbit and high eccentricity of a normal visual long-period binary. The rival theory of evolution in the opposite direction, expounded by W. Markowitz (*Astrophysical Journal,* **75,** 2), which supposes the spectroscopic systems of short period to have been produced from the long-period visual systems, is equally in difficulties. The almost unlimited time necessary for such an evolution is only one of the hurdles to be surmounted, but it is sufficient to rule it out of court according to modern ideas of the maximum age of the Universe.

In *Harvard Observatory Bulletin* 896, Carl K. Seyfert calls attention to a possible difference between the observed and the true distribution of the eccentricities of double star orbits. The unavoidable incompleteness of the data in any investigation necessitates certain selection effects appearing in the results ; for example, if two double stars have equal semi-major axes and their orbital planes are supposed perpendicular to the line of sight, then the one with the greater eccentricity will have the better chance of discovery, for the separation of its components will at times be considerably the greater. According to Öpik (*Tartu Publications,* **25,** 6) the "measure of difficulty" of a double star is given by the expression $\Delta m - 4 \log d$, where Δm is the difference of magnitude of the two components and d is the angular separation. Öpik also gives a method of converting this "measure of difficulty" of a system into a "percentage chance of discovery," based upon an examination of double star discoveries made with telescopes of varying resolving power. Seyfert has made use of these formulæ and has calculated a true relative frequency distribution

of the eccentricities. From this he deduces that the difference between the observed and corrected distributions is not sufficient to influence the assumption that increase of eccentricity with length of period is a real effect.

This view has not, however, found general acceptance. In *Comptes Rendus*, **199**, 930, Barbier suggests that computational selection is more important than discovery selection. A well-defined ellipse is more easily obtained for a long-period binary if the eccentricity is large and the date of periastron happens to be included within the available series of observations; this must cause a tendency for an apparent correlation between period and eccentricity. A recent paper by W. S. Finsen (*Monthly Notices R.A.S.*, 1936, Oct.) supports this view. His investigation is based on a catalogue of double star orbits compiled at the Union Observatory, Johannesburg, in 1933 (*U.O. Circular* 91), the total number of orbits taken into account being 135. From a table showing the distribution of the periods and eccentricities, he finds that the smallness of the correlation coefficient and the size of its standard error, combined with the possibility of selection effects, cast serious doubt on its reality. He then proceeds to consider possible correlations between the other elements of double star orbits, with rather surprising results. While a connection between the period and the semi-major axis might be expected, it is difficult to imagine any real association between the position angle of the nodal point and any other orbital element. Nevertheless, the relation between it and the eccentricity appears quite as pronounced as that between the latter and the period. Finsen considers that, as the former correlation is obviously illusory and due to sampling errors, the observed period-eccentricity relation is probably of similar origin and equally unreal; if it actually exists it is too small to be yet discoverable.

Regarding the similar correlation which appears in the orbits of spectroscopic binaries, he maintains that this in no way necessitates its existence in the case of visual systems, for it has not been proved that they represent different stages in an evolutionary sequence. His inclination, however, is to doubt the reality of the correlation in both cases. For spectroscopic binaries of short period, radial velocity determinations made at random are more likely to lead to the discovery of discordances when the eccentricity is small. When the period is long the chance of discovery favours cases of high eccentricity, and in this way it is quite possible for a spurious period-eccentricity relation to make its appearance. In any case, it seems wise to await further observational evidence of

its reality before attaching to it an elaborate theory of double star evolution.

A Catalogue of spectroscopic binary stars, compiled by J. H. Moore, has lately been issued as *Lick Observatory Bulletin* 483. This is named the " Fourth Catalogue " and follows the " Third " (*Lick Bulletin* 355) after an interval of twelve years. The new catalogue is a revision and enlargement of its predecessor. There is an addition of 346 stars to the list of those with variable radial velocities, orbital elements for 154 additional binaries have been calculated, and a number of stars which were at one time thought to have variable velocities have now been omitted. The catalogue comprises three Tables. The first contains the orbital elements of 375 stars, which, with the possible exception of about ten, Moore regards as real spectroscopic binaries. Fifty-five of these are eclipsing variables, four are ellipsoidal variables, and, of the remainder, twelve are visual doubles, and eighty-seven have both spectra visible. For 29 eclipsing binaries, which have both spectra visible and for which good photometric elements are available, the absolute dimensions, masses, and densities have been calculated. They are divided among the Algol, β Lyræ, and W Ursæ Majoris types. Table II consists of 44 stars whose variable radial velocities are probably due to causes other than orbital motion. They may be described as pulsating stars, and are either Cepheids or members of the classes represented by ζ Geminorum, β Canis Majoris, RR Lyræ, and Mira Ceti. Table III is a list of 447 stars with variable radial velocity comprising those whose discovery was announced after July 1, 1934, and for which no orbital elements are yet available, together with certain stars contained in the Third Catalogue concerning which later additional information has been received. The observed velocity range and the revised spectral class is given for each star.

PHYSICS. By W. N. BOND, M.A., D.Sc., F.Inst.P., The University, Reading.

AUTOMATIC REGULATION.—In physical laboratories and in engineering works it is frequently necessary to keep apparatus at a nearly constant temperature. Sometimes by insulating the apparatus thermally from its surroundings its temperature may be kept almost constant. But the insulation can never be made perfect, and it may happen that heat is being generated inside the apparatus. Thus, in general, it is necessary to measure the temperature of the apparatus and to add or remove heat (according to whether the apparatus is below or above the desired temperature). It may be

possible for the measurement and adjustment to be carried out personally by the one using the apparatus. But there are several advantages in using an automatic thermostat.

This is only one instance of a more general problem. It may be desired to maintain the humidity of the air in a room constant ; or to regulate the fuel and air supplied to a furnace, by making measurements on the steam pressure or output, or on the composition of the flue gases. These problems of automatic regulation raise a number of physical and mathematical questions of considerable interest and difficulty, some of which we will consider.

The equipment consists, in general, of two portions. The one part measures the temperature, humidity, or whatever is concerned ; the other part performs the controlling. The measuring apparatus will not respond instantaneously to changes in temperature (etc.) ; and there may also be a delay before the motion of the controlling mechanism affects the conditions near the measuring apparatus. Thus there is a time-lag and there may also be inertia. It was shown by A. Ivanoff (" Theoretical Foundations of the Automatic Regulation of Temperature," The Institute of Fuel, 1933) that the lag was, in several instances, like the lag that occurs when heat flows into a semi-infinite solid. If the " potential temperature " of the control mechanism be made to vary periodically, so that $\theta = A \sin mt$, then the recorded temperature is

$$Ae - c\sqrt{m} \sin (mt - c\sqrt{m}),$$

where c^2 is a time that indicates the amount of lag that is characteristic of the system.

If a change in the surrounding conditions arises, so that the measuring apparatus indicates a change, it is desirable that the control part of the apparatus shall rapidly restore the temperature to its original value. It is tempting to try to attain rapid control by making a small change in the measured temperature cause a large motion of the control mechanism. However, it must be remembered that there is a " time-lag," so that the thermometer will not inform us immediately, when enough adjustment has been made. Too large an actuation of the controls may necessitate a subsequent reversal of the action of the controls and so forth ; this will delay the attainment of the desired temperature, and may even cause permanent instability (termed " hunting "). It is necessary, therefore, to obtain rapidity of action without instability. But, at best, it is impossible to prevent fluctuations completely.

In the case of a thermostat, there are three things that influence its rate of change of temperature ; namely, the temperature of its

surroundings, its own temperature and the " setting " of the control mechanism. The action of the control mechanism might be arranged to depend on the amount by which the temperature departs from the desired value ; but the action might be made to depend on the *rate of change* of the temperature indicated by the thermometer. Even the *second* differential of the temperature with respect to time might be used ; or an integral ; or some function of these. Again, these might be used to decide the " setting " of the control mechanism, or the rate of change of the setting, and so forth.

The general problem of finding which of all these possible methods will be most satisfactory is very complex. Two necessary conditions can, however, be discovered immediately. In the first place, some account must be taken of the actual difference between the observed and desired temperatures. If only rates of change of the difference were used, the thermostat might asymptote to a constant temperature but one that differed from the desired value. In the second place, we note that the rate of heat supply required to keep the apparatus at the desired temperature, so that (observed temperature) — (desired temperature) = 0, is not zero or constant, but depends on the temperature of the surroundings. Hence the rate of supply of heat due to the control must not depend entirely on the difference between the observed and desired temperatures.

Methods of regulation, rapidity of action and conditions for stability were discussed by A. Ivanoff in the paper already mentioned. Another recent paper on these topics is by A. Callender, D. R. Hartree and A. Porter ("Time-lag in a Control System," *Phil. Trans. R. Soc.*, A, **235**, 415–44, July 1936). Mechanical details of control systems are discussed more fully in a paper by J. L. Hodgson and L. L. Robinson (" The Development of Automatic Combustion Control Systems for Industrial and Power Station Boilers," Inst. Mech. E., 1934).

CONVECTION OF HEAT.—It is not usually possible to deduce by theoretical methods the rate of loss of heat by convection, from a body of given shape. The problem is even more complicated than a typical problem in hydrodynamics ; but, nevertheless, it is one that is of considerable practical importance. Two types of convection may occur. In the case of " forced " convection, the hot body is placed in a moving stream of fluid. On the other hand, the heat emission causes local differences in density of the fluid ; as a result, the action of gravity produces a circulation of the fluid, and we have " free " or " natural " convection.

Considering only solids of one type (such as spheres, vertical

planes or horizontal cylinders) we find that the problem involves about nine variables. If forced convection be absent, the number is reduced to eight. A further simplification may be made by assuming that the rate of loss of heat per unit area per unit temperature excess, E, depends on the product (coefficient of expansion of the fluid)(temperature excess)(acceleration due to gravity) $= \alpha \theta g$ treated as one variable. It is then possible to deduce, by the method of dimensions, that E is given by

$$\mathrm{E}/lk = f\left\{\frac{(ag\theta).l^3s\rho^2}{\mu k}, \frac{\mu s}{k}\right\}$$

where l represents the linear dimensions of the solid, and k, s, ρ and μ are the conductivity, specific heat at constant pressure, density and viscosity of the fluid (respectively). If the fluid be a gas, the ratio μ/k is nearly independent of pressure and temperature, and thus $\mu s/k$ is nearly constant. In that case, the problem is reduced to that of finding a relationship between two non-dimensional products, and we need only plot a single curve relating

$$(\mathrm{E}/lk) \quad \text{and} \quad (ag\theta l^3s\rho^2/\mu k).$$

A recent paper by O. A. Saunders ("The Effect of Pressure Upon Natural Convection in Air," *Proc. Roy. Soc.*, A, **157**, 278–91, 1936) gives an account of experimental determinations of such curves, for (a) wide vertical planes, and (b) horizontal wires. It is pointed out that, for a given gas, $(ag\theta l^3s\rho^2/\mu k)$ is proportional to θl^3, and is nearly proportional to the square of the pressure. Many experimenters have determined the effect of l and θ upon the heat loss from surfaces ; but the effect of pressure had only once been investigated systematically (by Petavel in 1901). In order to determine E/lk for various values of $(ag\theta l^3s\rho^2/\mu k)$, it is most satisfactory to vary the pressure. Changes in l would necessitate making many lots of apparatus, of different sizes ; and large changes in θ would make the theoretical assumptions inadmissible.

O. A. Saunders therefore measured the loss of heat by free convection from vertical plane surfaces of four different heights, in air at pressures from 0·001 to 65 atmospheres. He thus investigated the heat losses for values of $(ag\theta l^3s\rho^2/\mu k)$ that ranged from about 0·4 to 4×10^{11}. The value of E/lk increased from about 1·2 to 800 in this range.

These experiments illustrate the fact that the rate of heat loss per unit area per unit temperature excess (which I have denoted by E) is not independent of the temperature excess, θ. In fact, it was found that the rate of loss of heat is approximately pro-

portional to θ^n, n increasing from 1·15 to 1·33 when $(ag\theta l^3 s\rho^2/\mu k)$ is increased from 10 to 10^9.

The application of dimensional analysis to problems in convective cooling was treated in an interesting paper by L. F. Richardson some years ago (*Proc. Phys. Soc.*, **32**, 405–9, 1920).

CONTINUOUSLY SENSITIVE CLOUD CHAMBERS.—We are most of us familiar with the wonderful way in which Professor C. T. R. Wilson's Cloud Chamber has enabled the tracks of α-particles and of electrons to be seen and photographed. Dust-free air in the chamber is saturated with water vapour. Then the air is suddenly expanded, causing supersaturation and condensation of water on any ions that are present. It is not always possible to tell at what instant the track of ions will be formed. Consequently, many expansions and photographic exposures are made (automatically), in order that some of them may reveal the phenomenon that is being studied. In the case of cosmic ray tracks, two Geiger-Müller counters may be used to detect when a ray traverses the cloud chamber ; and the expansion and photographing are then carried out immediately, by a mechanism controlled by the counters.

Attempts have recently been made to devise cloud chambers that are continuously sensitive. A. Langsdorf, Jr. (*Phys. Rev.*, **49**, 422, 1936) gave a preliminary account of a chamber containing carbon dioxide, methyl alcohol vapour at 75° C. and atmospheric pressure being supplied at the top of the chamber. The bottom of the chamber was kept at − 45° C. ; and there is an intermediate region sufficiently supersaturated to show tracks due to cosmic-ray primaries and secondaries. Many tracks were observed, some being nearly two feet in length ; but a continuous rain of condensed droplets prevented the tracks being satisfactorily photographed.

R. E. Vollrath (*Rev. Sci. Inst.*, **7**, 409–10, November 1936) carried out similar experiments, and obtained similar results. He also developed a continuously sensitive cloud chamber, in which the supersaturation is produced by the inter-diffusion of water-vapour (moving downwards) and hydrochloric acid vapour (moving upwards). The chamber is claimed to be of a very simple type ; but this chamber, also, has so far only been used successfully for *visual* observation.

THE VALUE OF e/h.—During the last few years, many precise measurements of the energies of X-ray electrons have been made by Professor H. R. Robinson and others, using the magnetic spectrometer. In a paper entitled " X-ray Levels and Atomic Constants " (*Phil. Mag.*, **22**, 1129–36, December 1936) Professor Robinson

discusses some deductions from these results. He finds that

$$(e/m_0)(e/h) = \frac{1 \cdot 277_4 \times 10^{24}}{1 \cdot 0020},$$

expressed in the usual mixed units, e/m_0 in e.m.u. gm.$^{-1}$, and e in e.s.u. Assuming that $e/m_0 = 1 \cdot 7576 \times 10^7$, this gives $h/e = 1 \cdot 378_7 \times 10^{-17}$. For comparison, Professor Robinson uses the value of e/m_0 and the value $e = 4 \cdot 803_5 \times 10^{-10}$ abs. e.s.u., and deduces from Rydberg's equation that $h/e = 1 \cdot 3799 \times 10^{-17}$. This result (which corresponds to $\dfrac{hc}{2\pi e^2} = 137 \cdot 06$) agrees with the first result within experimental error. Professor Robinson also estimates h/e from work by Kirkpatrick and Ross and by Schaitberger on the limit of the continuous X-ray spectrum, and obtains the values $1 \cdot 3754$ and $1 \cdot 3757 \times 10^{-17}$ (respectively). These results do not agree so well with Professor Robinson's first estimate (that deduced from the magnetic spectrometer measurements).

METEOROLOGY. By E. V. NEWNHAM, B.Sc., Meteorological Office, London.

THE STRUCTURE OF SNOWFLAKES REVEALED BY THE MICROSCOPE.—Very few people are aware of the diversity of forms taken by snowflakes. In the lower-lying parts of the British Isles snow seldom falls when the temperature is far below the freezing point and in consequence the commonest form of snowflake is a more or less shapeless collection of crystals. Even if these crystals can be distinguished as separate entities they are generally damaged or partly melted, and no idea can be formed of the intricate beauty that they nearly always have before leaving the cold upper levels of the atmosphere. Even those who live in colder climates are likely to be equally ignorant of the finer structural details of snow crystals for the reason that to a casual glance little more is evident in a newly-fallen crystal beyond the fact that it is generally of the form of a hexagonal plate or of a six-rayed star. To see the true structure a powerful pocket lens or a microscope must be used.

The literature of this subject is not very extensive, but the work of one investigator—the late W. A. Bentley—was carried on with such enthusiasm that reproductions of more than 2000 microphotographs are available for the student, in his book *Snow Crystals*.[1] Several writers have tried to explain how the different types of crystal are produced. The subject is reviewed in the first part of

[1] *Snow Crystals*, by W. A. Bentley and W. J. Humphreys, 1931.

a paper by G. Seligman,[1] of which an account will be given here. Among the simpler kinds of snowflake may be seen the very common form consisting simply of a flat hexagonal plate, usually not more than 3 mm. in diameter and about ten times greater in diameter than in thickness. The type is roughly sketched in Fig. 1. Other simple forms include the long thin needle, the hexagonal prism (Fig. 2) and the six-rayed star (Fig. 5). The prism often has end plates which may be equal in size (Fig. 3) or unequal (Fig. 4). The type shown in Fig. 5 is often elaborated into designs of astonishing complexity, of which Bentley photographed about 5000. The design is not always symmetrical and is sometimes very far from being so. There is nearly always a nucleus, and often flower-like developments are built regularly around this; one variety of nucleus is an almost perfect replica of the Tudor rose of heraldry except that the "petals" of each ring number six instead of five.

The structure of a snowflake is of scientific as well as æsthetic interest, for there are grounds for believing that this structure is so closely dependent upon the atmospheric conditions experienced by the flake, between its first beginnings and its descent to earth, that these conditions can be to some extent deduced by an examination
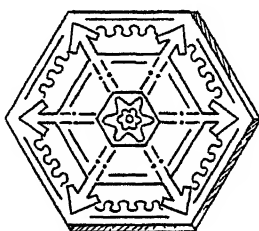
Fig. 1.
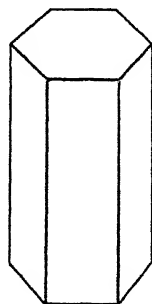
Fig. 2.
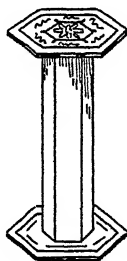
Fig 3
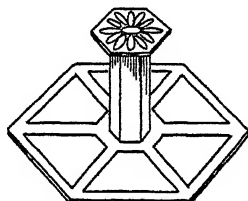
Fig. 4.
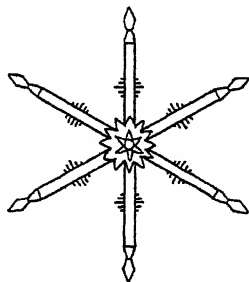
Fig. 5.

[1] "Snow Structure: Some Practical Applications," by G. Seligman, Q.J. Met. Soc., **63**, No. 268, January 1937, pp. 93–103.

of the structure. The beginning of the flake is believed to be as follows. The cooling of damp air below the freezing point results in condensation of water vapour upon minute solid nuclei, to form very small drops, which may then freeze. Water vapour will then condense as ice on the drops without passing through the water stage. This so-called "sublimation," once it has started, will cause the ice particles to grow at the expense of the water drops, which drops are supercooled. In general the colder is the region of formation the smaller are the ice crystals. In the Alps a flake is nearly always a single crystal, with a diameter of not more than 3 mm. or less than 1 mm., but in extremely cold regions the size is smaller and fallen snow is then very powdery. Observations made by Lehmann many years ago of the crystallisation of iodoform out of solution, which results likewise in hexagonal crystals, suggested to Wegener the interpretation of snowflake structure to which reference has already been made. It was noted that after a crystal had grown for a time the solution bordering it became lighter in colour, the darker solution corresponding with supersaturation being found only at a distance. Growth then took place mainly along six "rays" extending outwards from the six points of the initial nucleus somewhat as in Fig. 5. This stage continued until the length of the "rays" became such that concentrated solution readily diffused into the spaces between them and side growth between the rays began. Corresponding with the different strengths in the solution of iodoform, according to Wegener, are the different vapour pressures in the atmosphere, the growth of a snow crystal being controlled by the gradient of vapour pressure. A steep gradient on this analogy should favour growth along the six "rays," and lack of such gradient would favour simple plate development. It is a plausible theory that adds interest to the study of the different types of snow crystal; it could conceivably be verified or disproved by experiment although the difficulties to be overcome would clearly be very great. Very complex structure should according to this theory be produced in snowflakes that fall from great heights, where the air is very cold, through one or more inversions of the ordinary "lapse rate" (vertical gradient of temperature) in the layers of air nearer to the ground, for sudden changes of vapour pressure are likely to be found near these inversions. Small simple flakes according to this theory are flakes that have not fallen very far or that have soon passed into regions where there is little water-vapour.

The distribution of the different types of snowflake in relation to cyclonic depressions bears out Wegener's ideas to some extent.

Bentley and Humphreys found that the simple forms reach the earth in the largest numbers at high altitudes, or in the western and north-western sectors of large depressions where, it may be noted, in general air drawn from polar regions is most likely to be found. It would be expected on the other hand that the more complex forms, which are believed to require abundant water-vapour and not such low temperatures, would be found in the front of a depression, *i.e.* in the northern hemisphere generally in the eastern and south-eastern parts of the storm. That expectation seems to be borne out by observation. Other observed facts bearing on the same subject include the fact that local storms and low detached clouds generally give fragile tabular crystals or granular crystals, while it is in large depressions that the more solid tabular crystals and the prism forms are to be found. The different cloud layers, if present singly, in only moderately cold weather, have their characteristic forms of snowflake. Thus, detached nimbus cloud at a low level tends to yield large fragile branching tabular crystals. Clouds of medium height (alto-stratus, alto-cumulus) produce a smaller tabular crystal with solid hexagonal plate at the centre. High cirro-stratus, as would be expected according to Wegener's ideas, tends to produce very small columnar or tabular varieties. It is not proposed to go at all deeply into the second part of Seligman's paper, which deals with the properties of snow as a covering to the ground and its reactions to different meteorological factors, for these are matters that are more interesting to mountaineers and travellers in polar regions than to meteorologists, but one feature of this is very important through its influence on the surface temperature, and deserves notice. Freshly fallen snow is a very loose assemblage of ice crystals. As their facets face in all directions they scatter light in all directions. There is very little absorption of solar radiation and in consequence the sun is very ineffective in raising the temperature of the air. Moreover, a great deal of air is trapped between the crystals and the loose snow-covering in consequence forms an eiderdown-like layer that prevents the heat stored deeper down in the summer (in those regions where the ground is bare at that season) from passing up to the top of the snow-covering and ameliorating the cold of winter. A spell of calm weather in initially cold air in winter will therefore tend to develop into a much more severe spell of frost if it begins with a fall of light powdery snow several inches thick. It is only in this way that cold comparable with that of the Continent can develop in the British Isles in the absence of an exceptionally strong wind current originally drawn from polar regions but arriving from

the east. Air temperatures below zero (Fahrenheit) probably never occur in England without the help of a snow-covering.

BIOCHEMISTRY. By W. O. KERMACK, M.A., D.Sc., F.R.S.E., Research Laboratory, Royal College of Physicians, Edinburgh.

CHEMICAL CONTROL OF REPRODUCTION AND LACTATION.—In recent years, a very large amount of work has been carried out with the object of discovering how the various complex processes which occur in the female organism before, during, and after pregnancy are controlled and co-ordinated. It has become increasingly evident that an exceedingly important part is played by the so-called sex hormones, and that, though present usually in excessively small quantities, to be measured in gamma rather than in milligrams, they are of the utmost importance for the proper functioning and control of the reproductive mechanism. These hormones have two main sources in the body, the anterior lobe of the pituitary gland and the ovaries. The former, the anterior lobe of the pituitary,
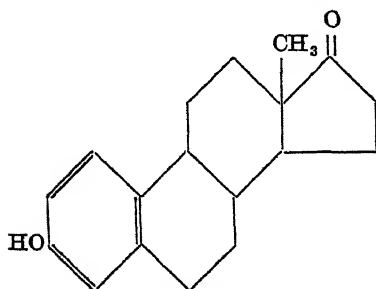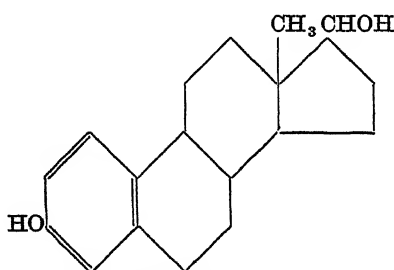
I. ŒSTRONE.   II. ŒSTRADIOL.

we may regard as the central authority, sending out its orders, partly to the tissues affected, and partly to sub-stations scattered throughout the body. The ovary acts as one of these sub-stations and, when stimulated by the appropriate pituitary hormone, liberates the œstrus or heat-producing hormone into the blood stream. This compound in its various forms, œstrone, œstradiol, and œstriol is now well known, but it may be pointed out here that it has recently been shown by Macquorquodale, Thayer and Doisy (*J. Biol. Chem.*, 1936, **115**, 435) that the main hormone actually present in follicular fluid from sows' ovaries (as opposed, for example, to the urine of stallions or pregnant women, the common source of œstrus hormone commercially) is the alcohol œstradiol, from which the urine hormone, œstrone, is presumably derived by oxidation. (See I and II.)

Although pregnancy urine is one of the main sources of these œstrogenic compounds, it now appears that they are excreted in it, not in the free, active condition, but mainly in a combined form, namely as the glycuronate. Cohen, Marrian and Odel (*Biochem. J.*, 1936, **30**, 2250) have recently described a method for isolating the glycuronate of œstriol, in relatively good yield, as a crystalline sodium salt ; the pure compound proves to have only about 1/17 of the activity of the free œstriol. This result has an obvious practical bearing on the assay, by physiological methods, of the total œstrin content of the urine, and it is also of considerable interest from the physiological point of view. According to Cohen, Marrian and Watson (*Lancet*, 1935, i, 674) the percentage of free œstrogenic compounds increases rapidly at the time of parturition, and it may be that this corresponds to an increase, in the body, of the total œstrogenic activity, an increase which, they suggest, may play an important part in the initiation and control of labour.

It is an interesting fact that these œstrogenic compounds are found, not only in animal organisms ; they also occur in plants. Thus extracts of the female catkins of the willow plant contain œstriol (Skarżyński, *Bull. Acad. Polonaise*, 1933, **B**, 347), whilst œstrone is found in palm kernel extract. Perhaps these potent substances first occurred accidentally, in the course of the evolutionary development of living organisms, as by-products formed from sterols and related compounds, and were subsequently utilised by the higher animal organisms for the control of the complicated and important reproductive cycle. They are keys which release an involved cell mechanism ; but perhaps the key was there first and the lock was made to fit it.

The elucidation of the details of the hormonal control of the reproductive functions in the female is likely to prove a very difficult task, not only because of the essential complexity of the problem in the case of any one species, but also because each species appears to have developed on more or less individual lines, so that what is true of one often fails to hold for another. The hormones themselves are, however, by no means specific. The same œstrone, for example, is derived from man, pregnant women, and from stallions, and it seems to produce the same type of action on any mammal. Similarly, the other hormone of ovarian origin, progesterone (III), which is produced by the surviving follicle, the corpus luteum, from which the ripe ovum has erupted, appears to be common to all mammals investigated. However, its importance would seem to differ according to the species. As its name indicates its general effect is to prepare the female organs, and especially

the uterus, for the developing fertilised ovum. If a normal pregnancy ensues, the corpus luteum, in most species, persists, and does not disappear, as happens in the absence of fertilisation. In many animals, removal of the corpus luteum during pregnancy results in abortion, as if progesterone were necessary to keep the uterus in proper condition for the development of the embryo. In the guinea-pig, however, it seems the corpus luteum does not normally persist during pregnancy, whilst in the human species, its surgical removal during pregnancy is not necessarily followed by abortion. In this field therefore, it is obviously very dangerous, as far as details are concerned, to apply results obtained with one animal species to another.

The process of lactation is obviously closely connected with those of pregnancy and parturition, and it is natural to expect
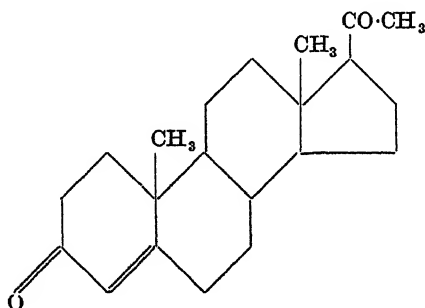
III. PROGESTERONE.

that it, too, is controlled by hormones produced by the ovaries and the pituitary. Indeed, this expectation should apply not only to the actual process of milk production, but also to the development of the mammary glands in which the elaborate chemical reactions involved in the synthesis of the peculiar constituents of milk is carried out. Numerous experiments, on many different species of mammals, all point to the important rôle which the œstrogenic hormones play in the development of this gland, especially during puberty and the period before pregnancy (for a very useful and interesting review, cf. Nelson, *Physiol. Rev.*, 1936, **16**, 488). However, it would seem that in many species at least, œstradiol alone does not suffice to bring the glands to the stage at which they are ready for actual secretion, though exceptions may exist, such as the guinea-pig. In many species complete development does not seem to take place until the action of œstradiol is supplemented by that of progesterone. The treatment of immature or

ovariectomised female animals by courses of administration of these two hormones has been shown in many instances to to the extensive development of the mammary system, and even to the production of lactation itself on cessation of the injections. (*Cf.* Turner and Schulze, *Mo. Agric. Exper. Stat. Res. Bull.*, 1931, 157 ; Turner and Frank, *Mo. Agric. Exper. Stat. Res. Bull.*, 1932, 174.)

It was at first concluded from this and other observations, that the actual process of milk secretion was not directly stimulated by the action of a hormone, but that it occurred rather as the natural activity of the ripe mammary gland, when the stimulus to growth was removed as the result of the decrease in the amount of ovarian hormone available. There is much evidence that the termination of pregnancy is accompanied by such a fall in the output of ovarian hormone by the female organism. However, it was shown by Stricker and Greuter (*Compt. Rend. Soc. de Biol.*, 1928, **99**, 1978 ; *Klin. Woch.*, 1929, **8**, 2322) that suitably prepared extracts of the anterior lobe of the pituitary are able to stimulate the non-lactating mammary gland to produce milk, a result also found by Corner (*Am. J. Physiol.*, 1930, **95**, 43) in America, and since then amply confirmed by many workers using various species of animals. This direct action of pituitary hormone, it would seem, can only be exerted on the gland when it has already been prepared, presumably by the previous action of œstrone and progesterone. However, it would seem that as long as there is an ample supply of the ovarian hormones, that is to say, during pregnancy, the direct lactogenic action of the pituitary is somehow inhibited, and normally no milk secretion occurs. There is some evidence, too, that the placenta plays some part in antagonising the pituitary lactogenic hormone. When parturition occurs, these inhibiting influences are removed, and, under the action of prolactin, as the principle has been named, the mammary glands begin to secrete milk. This view receives some support from experiments recently reported by Folley (*Biochem. J.*, 1936, **30**, 2262) who found that the administration of œstrogenic compounds to lactating cows reduced their yield of milk considerably.

Prolactin, like the other hormones of the anterior lobe, has not so far been isolated as a pure compound. As it is destroyed by trypsin digestion it is probably a protein or polypeptide. There is considerable evidence that it is a substance distinct from the other numerous anterior lobe hormones. The variety of these principles becomes more and more confusing to those who are not immersed in the subject. Besides the gonadotropic substances,

prolan A and B, which act on the ovary, there is the growth-pro-
moting principle, as well as one which stimulates the thyroid, and
another which acts on the adrenals. These, of course, do not
include the group of hormones found in the posterior lobe—here
we encounter the pressor principle, which raises the blood pressure,
the oxitocic principle, which acts on the uterus, and also the melano-
phore-expanding substance which causes toads to turn dark, and
the anti-diuretic factor, which is concerned with the elimination
of water through the kidneys. The number and variety of these
factors emphasises the extreme importance of the pituitary gland
(which in the human being weighs only about 0·5 g.) in the regula-
tion and co-ordination of the various body functions. It seems to
act like a central office, sending out its chemical messengers all
over the body and so controlling and keeping in step the various
activities of the organism.

As has already been remarked, prolactin, like the other pituitary
hormones, is not species-specific. This lack of specificity is well
brought out by the fact that one of the favourite methods of assay
depends on its action in stimulating the growth of the crop gland
of the pigeon, which it may ultimately cause to produce crop milk.
Thus its activity is not limited even to mammals. Riddle, Bates
and Lahr (*Amer. J. Physiol.*, 1935, **111**, 352) report the very in-
teresting observation that prolactin preparations, administered to
hens, cause them to brood and to stop laying eggs. It would seem,
then, that this hormone is not only lactogenic, but that it plays
an important rôle in determining maternal behaviour. That this is
also true in the case of mammals is indicated by the observations
of Riddle, Lahr and Bates (*Proc. Soc. Exp. Biol. and Med.*, 1935,
32, 730), who found that the administration of the hormone to
rats resulted in the type of behaviour associated with maternity
in that animal. In the absence of experiments with pure com-
pounds, it is, of course, difficult to decide whether this effect on the
behaviour of the animal is really due to the same substance which
stimulates milk secretion, but it is in any case very significant that
such characteristic psychological effects can be produced by traces
of chemical compounds, and particularly interesting that these
compounds should be found in the pituitary extracts (*cf.* Wiesner
and Sheard, *Maternal Behaviour in the Rat*, Oliver & Boyd, London,
1933). The instinct which exhibits itself in broodiness, and other
forms of maternal behaviour, is found in certain animals more
primitive than mammals and birds, and it is possible that pro-
lactin may control this important instinct in these more primitive
types. It is suggested by Riddle, Bates and Lahr (*loc. cit.*) that

the control of lactation by the same hormone may be a secondary development, an adaptation of an already existing instrument for a new purpose.

Milk is a highly differentiated product, containing several characteristic substances not to be found elsewhere. Comparatively little is known about the manufacture of these, but the line of work described by Grant in a recent paper (*Biochem. J.*, 1936, **30,** 2027) seems likely to prove valuable. If thin slices of mammary gland (guinea-pig tissue was used) are incubated in a suitable medium, small quantities of lactose are formed. The yield of lactose is increased by the addition of glucose, but not by the addition of any of the other sugars tried. Further, the addition of galactose along with glucose was found to be no better than the glucose alone. The above result seems to establish that glucose is the compound out of which the mammary cells synthesise lactose. It might have been expected that, in the course of the synthesis, part of the glucose is first turned into galactose, and that the second stage consists of the combination of one molecule of this galactose with one molecule of glucose. It is therefore surprising that the galactose in the glucose-galactose mixtures was not utilised, at least as readily as the glucose alone. This may indicate that the two-stage theory of lactose formation is wrong, but on the other hand it may only show that galactose is unable to penetrate the cell membranes, or at least, to reach those localities in the cell where combination with glucose takes place.

In addition to developing the tissue slice technique, Grant has also studied the composition of milk, secreted under the influence of prolactin injections. Generally, in previous work, only the quantity and not the quality of the milk secreted had been observed. Guinea-pigs which had recently borne young, but of which the mammary glands were undergoing regression in consequence of the offspring having been removed, were used. The prolactin injections led to milk secretion, but the lactose content of this milk was less than 0·25 per cent. instead of the normal 4–5 per cent. Thus it would seem that, though prolactin undoubtedly stimulates the mammary cells to act, it does not suffice to induce complete normal activity. The suggestion is made that prolactin is probably not the only hormone involved in the control of mammary gland secretion—a suggestion extremely plausible in view of the known complexity of the problem.

VITAMIN B_1.—Before concluding this review of recent advances in biochemistry, one outstanding achievement of the last six months must be referred to, namely the synthesis of vitamin B_1 by Williams

and Cline (*J. Amer. Chem. Soc.*, 1936, **58**, 1504). It was only in 1932 that the correct empirical formula for this vitamin was first found by Windaus, Tschesche and Ruhkopf. Since that time intense efforts have been made to discover its structure and to confirm this by synthesis. The synthesis now reported by Williams and Cline shows that the structure of the vitamin is definitely IV and this is confirmed by the synthesis of its fluorescing oxidation product, thiochrome, by Todd, Bergel, Fraenkel-Conrat and Jacob (*J.C.S.*, 1936. 1601). And so one more chapter in the chemical history of the vitamins seems to have been completed. But immediately on the heels of this work comes a very interesting announcement by Lohmann (*Naturwiss.*, 1937, **25**, 26). He has separated

$$\text{Me}\underset{N}{\overset{N}{\bigcirc}}\text{NH}_2 \quad \underset{\underset{\underset{OH}{\overset{|}{\smile}}}{\overset{S}{\wedge}}}{\overset{}{}} \text{CH} \quad \text{C CH}_2\cdot\text{CH}_2\text{ OH}$$

IV
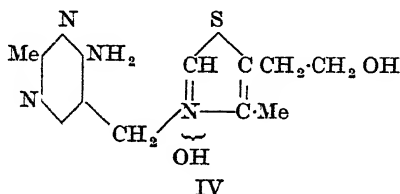
the co-enzyme of carboxylase in a crystalline form, and finds that it is a phosphorus-containing compound from which two molecules of phosphoric acid can be split by hydrolysis, the one easily, and the other with much greater difficulty. The residue contains sulphur, and is probably none other than vitamin B_1. Just as the phosphoric acid derivative of vitamin B_2 is the co-enzyme of an oxidation system (*cf.* SCIENCE PROGRESS 1935, XXIX, 694), so this phosphoric acid derivative of the antineuritic vitamin is the co-enzyme of the decarboxylating enzyme. It may be added that the co-enzyme itself exhibits anti-neuritic and anti-torulin activity, the characteristic function of vitamin B_1, but the unphosphorylated vitamin cannot act as co-enzyme. The exact relationship between the two will doubtless be cleared up by further work, but in any case the new discovery appears to be a fundamental one in the domains both of vitamin and of enzyme chemistry.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., D.Sc., The University, Glasgow.

SEDIMENTATION AND SEDIMENTARY ROCKS.—In the opinion of W. E. Powers (*Journ. Geol.*, XLIV, 1936, 214–19) einkanter and dreikanter, although distinctive, are less valuable as indicators of wind abrasion than certain types of rock surface which have been subjected to the action of natural sand blast.

L. C. King discusses certain abnormal types of wind-faceted stones from New Zealand (*Journ. Geol.*, XLIV, 1936, 201–13), which are due to the action of winds blowing alternately from opposite directions, and carving sand-blast facets on opposite sides of flat stone fragments.

O. R. Grawe deals with the action of ice as an agent of rock weathering (*Journ. Geol.*, XLIV, 1936, 173–82). When water freezes in rock it can exert a theoretical maximum pressure of about 30,000 lb. per square inch, but it is highly improbable that the maximum pressure is ever attained under natural conditions.

Observations by J. A. Steers on the rate of sedimentation in a salt marsh on Scolt Head Island, Norfolk, by actual measurement (*Geol. Mag.*, LXXII, 1935, 443–5) have shown that the average accretion of silt from July 1934 to July 1935 was about $5\frac{1}{2}$ mm. More detailed measurements are promised.

Study of the "Continental Shelf Sediments off the Mid-Atlantic States" by F. P. Shepard and G. V. Cohee (*Bull. Geol. Soc. Amer.*, **47**, 1935, 441–58) shows that there is no decrease of grain-size in the seaward direction. This, and other lines of evidence, suggest that the sediments are out of harmony with present-day conditions, and were probably deposited as material mostly derived from glacial outwash during the Pleistocene stages of lowered sea level.

In his paper "Observations on Chance Experiments in Consolidation of Sediments" E. M. Kindle (*Amer. Journ. Sci.*, XXX, 1935, 537–40) records two examples of encrusting conglomerate formed in one case under sea floor conditions on an old cannon, and in the other on a stove lid resting on the bottom of a lake. Oolitic limestone found on and within a steam pipe is evidence that solid oolitic rock is now forming on the Florida sea floor.

With the aid of experimental work on the relation between pressure and volume in the purer argillaceous sediments H. D. Hedberg (*Amer. Journ. Sci.*, XXXI, 1936, 241–87) has been able to throw much light on the processes of gravitational compaction in clays and shales. The process is believed to take place in four stages under the influence of superincumbent pressure, loss of free water, loss of adsorbed water, mechanical deformation of particles and recrystallisation.

B. Sander has carried out extensive researches on the rhythmic stratification of the limestones and dolomites of the Alpine Trias (*Min. u. Petr. Mitt.*, **48**, 1936, 27–139; 141–209), partly with the aid of the petrofabrics method developed by him. His conclusions are much too detailed to be summarised here; but the student of sedimentary rocks will find in this memoir by far the most thorough-going

investigation of the origin of dolomite and of rhythmic sedimentation in limestone formations that has been made for many years.

W. D. Keller has studied the flocculation of colloidal clays under the petrographical microscope (*Journ. Geol.*, XLIV, 1936, 52–9). The floccules formed relatively coarse aggregates of platy forms. The author suggests that bedding in shales is partly due to the deposition of these aggregates, and also that clay partings in limestones may originate from flocculated clay colloids.

A very detailed investigation on geometrical lines of the " Systematic Packing of Spheres—with Particular Relation to Porosity and Permeability " has been made by L. C. Graton and H. J. Fraser (*Journ. Geol.*, XLIII, 1935, 785–909). Besides packings that are described as " chaotic," " haphazard " and " chance," six cases of simple systematic packing are recognised. The " unit void " in each case has been thoroughly explored and illustrated, since in it lies the keys to the problems of porosity and permeability in sediments. In Part II of this work permeability is considered in all its relations to porosity; but it is concluded that experiment must still be relied on for the determination of permeability, since there are no existing means for its exact computation.

Following on the theoretical discussion dealt with in the foregoing paragraph H. J. Fraser has made an " Experimental Study of the Porosity and Permeability of Clastic Sediments " (*Journ. Geol.*, XLIII, 1935, 910–1010). The following summary by the author illustrates the scope of the work : " The effect on porosity of absolute grain-size, variable grain-size, shape of grain, method of deposition and compaction are experimentally determined for ideal materials, and compared with those of river and beach sands and other clastic sediments. Various equations for permeability are discussed ; and the controlling factors . . . are experimentally evaluated for ideal materials, and the modifications caused by natural materials are delimited and discussed. Field observations on the effects of wave and river depositional processes on porosity and permeability are presented. Comparisons are made of the relative permeability of gravel, sand, clay, loess, till and unsorted alluvium. Changes produced in the original porosity and permeability of clastic sediments by the metamorphic processes of compaction, cementation and recrystallisation are outlined. Finally, the principles governing the flow of fluids through unconsolidated clastic materials are applied to the flow of hydrothermal solutions through rocks of clastic origin in an attempt to use the principles of permeability to explain and predict the distribution and localisation therein of hydrothermal mineral deposits."

The above two memoirs would appear to be of fundamental importance for the study of many of the problems of the sedimentary rocks

Th. Zingg's " Beitrag zur Schotteranalyse " (*Schweiz Min. u. Petr. Mitt.*, XV, 1935, 39–140) is a very full treatment of gravels in general, with new methods of mechanical analysis and graphic representation of results. The shapes of pebbles are discussed, and a special application of the methods developed is made to the gravels of the Glattal in Switzerland.

A very complete petrographic study of the Ordovician St Peter Sandstone in the Mississippi Valley region has been made by G. A. Thiel (*Bull. Geol. Soc. Amer.*, 46, 1935, 559–614). The sandstone is mainly composed of white, frosted and well-sorted quartz grains. The pre-Croixian sandstones on the southern margin of the Laurentian Shield have contributed much to the St. Peter Sandstone, which was deposited in a shallow sea. Wind-rounding and frosting of the grains was accomplished during periods of marine retreat.

A useful account of " Die Petrographie der Grauwacken " has been given by G. Fischer (*Jahrb. Preuss. Geol. L.-A.* 54, 1933, 320–43. Summary by the author in, *Geol. Centralbl. A -Geol.* 54, 1935, 420). In Fischer's view greywackes are clastic sediments in which there are approximately equal amounts of several grades of grain-size.

A considerable amount of research on clay rocks and clay minerals has recently been published in France by J. de Lapparent, and in America, Japan and Switzerland, by several workers. The following are the most important papers : J. de Lapparent, " Constitution et origine de la leverriérite " (*C. R. Acad. Sci., Paris*, 198, 1934, 669–71) ; " Sur un constituant essentiel des terres à foulon (*ibid.*, 201, 1935, 481–3. This new constituent of fuller's earth is found to be sepiolite) ; " La place de la montmorillonite dans la catégorie des silicates phylliteux " (*ibid.*, 527–9) ; " Les hydroxydes d'aluminium des argiles bauxitiques de l'Ayrshire (Écosse) " (*Bull. Soc. Franc. Min* , LVIII, 1935, 246–67) ; P. G. Nutting, " The Bleaching Clays " (*U.S. Geol. Surv., Circular* 3, 1933, 51 pp.) ; C. S. Ross and P. F. Kerr, " Halloysite and Allophane " (*U.S. Geol. Surv., Prof. Paper* 185–G, 1934, 135–48) ; C. J. Ksanda and T. F. W. Barth, " Note on the Structure of Dickite and other Clay Minerals " (*Amer. Mineralogist*, 20, 1935, 631–7) ; B. Yoshiki, " Dickite in the Rôseki Deposits in Shôkôzan " (*Proc. Imper. Acad. (Japan)*, X, 1934, 417–20) ; R. H. Bray, R. E. Grim and P. F. Kerr, " Applications of Clay Mineral Technique to Illinois

Clay " (*Bull. Geol. Soc. Amer.*, 46, 1935, 1909–26) ; W. Noll, " Über die Bildungsbedingungen von Kaolin, Montmorillonit, Sericit, Pyrophyllit und Analcim " (*Min. u. Petr. Mitt.*, 48, 1936, 210–47. This investigation is based on actual syntheses of the above minerals.).

The paper by E. Minami on " Selen-Gehalte von europäischen und japanischen Tonschiefern " (*Nach. v. Ges. Wiss. Göttingen. Math.-Phys. Kl. Fachgr. IV, Geol. u. Min.*, N.F. 1, Nr. 12, 143–5) gives three valuable analyses of composite samples of 36 European Palæozoic shales, 14 Japanese Palæozoic shales and 10 Japanese Mesozoic shales respectively. The selenium content works out at the order of 0·0001 per cent. for the European, and 0·00003 per cent. for the Japanese shales. The same author has also determined the content of rare earth elements in European and Japanese shales (*ibid.*, No. 14, 155–70), but the results are too voluminous to be given here.

R. D. Russell describes " The Mineral Composition of Atmospheric Dust collected at Baton Rouge, Louisiana " (*Amer. Journ. Sci.*, XXXI, 1936, 50–66) from the material deposited by duststorms in April 1934 and April 1935. Both samples contained a high percentage of volcanic glass particles. The suggestion is made that the abundance of this light material is due to a natural concentration resulting from long suspension in the atmosphere.

An outstanding discussion of the problem of the origin of coral reefs has been given by P. H. Kuenen (" Geology of Coral Reefs," *The Snellius Expedition in the Eastern Part of the Netherlands East Indies*, Vol. V, *Geological Results*, Part 2, 1933, 125 pp.), based on intensive study of reefs in and near the Dutch East Indies. All the available evidence, according to Kuenen, tends to confirm Darwin's original subsidence theory ; and while he thinks that the glacial lowering of sea level probably influenced the upper parts of already existing atolls, he opposes the view that glacial control, according to Daly's theory, has been the predominant agent in the formation of the sub-structure of atolls.

In a discussion on " The Foundations of Atolls," J. E. Hoffmeister and H. S. Ladd (*Journ. Geol.*, XLIII, 1935, 653–65) show that undeserved support has been given to the theory of subsidence by failure to determine the organic composition and the ages of elevated limestones. Many elevated coral reefs have foundations composed of non-coralliferous limestones which, in many cases, are geologically considerably older than the superstructures.

In a criticism of the Glacial Control theory of the origin of coral reefs the same two authors (*Journ. Geol.*, XLIV, 1936, 74–92) state their belief that the theory offers a satisfactory explanation

of the occurrence of drowned valleys, and that Pleistocene conditions stimulated reef growth, but they question the attribution of barrier reefs and atolls to this mode of formation. The authors support the " antecedent-platform " theory, according to which, ecological conditions permitting, a reef can grow to the surface without any change in ocean level.

The importance of calcareous algæ as rock-builders in Russian geological formations is emphasised by V. Maslov (*Problems of Soviet Geology*, V, No. 5, 1935, 475–90). These organisms have produced notable limestones in the Pre-Cambrian, Cambrian, Devonian, Carboniferous, Permian, Jurassic and Tertiary formations of Russia and Siberia.

E. M. Kindle describes hard calcareous aggregations derived from the bottom of a small lake near 64° N. Lat. on Southampton Island, Hudson Bay (*Geol. Mag.*, LXXII, 1935, 519–21). These turned out to be concretions formed by blue-green algæ. The most northerly of previous records of algal concretions in Canada was from 47° N. Lat. in Ontario.

In a paper on " Travertine-depositing Waters near Lexington, Va., E. Steidtmann (*Journ. Geol.*, XLIV, 1936, 193–200) shows that the water of Wilson Falls Creek is supersaturated with calcium carbonate throughout the year, but that the creek loses lime when the temperature rises above 8° C. The loss increases with rising temperature, but at all times adjustment of the conditions to ·produce precipitation is very slow.

An exhaustive study of the ·" Boone Chert " (Mississippian) by A. W. Giles (*Bull. Geol. Soc. Amer.*, 46, 1935, 1815–78) shows that two types of chert are present, one insoluble, granular and of vitreous appearance, the other a calcareous chert or " cotton-rock," which is limestone in various stages of silicification. The intimate relationships of the two types suggest contemporaneous origin. The silicification and recrystallisation of the Boone Limestone are believed to have been accomplished by hydrothermal solutions of magmatic origin.

Concretions consisting of well-sorted sand-grains cemented with crystalline calcite are described by K. B. Garner from a Tertiary formation in the Lower Colorado Desert region of California (*Amer. Journ. Sci.*, XXXI, 1936, 301–11). The concretions have a club- or mace-like form. Their mode of origin is uncertain.

Ironstones characterised by the occurrence of oolitic clay minerals along with siderite are described by T. Deans from the Coal Measures of Yorkshire (*Trans. Leeds Geol. Assoc.*, V, Pt. 3, 1936, 161–87). These have been formed by the precipitation of

siderite and hydrous aluminium silicates, with subsequent replacement and crystallisation.

A valuable summary of the theories of origin of the peculiar " Iron formation " of the Lake Superior region is contained in a work by C. K. Leith, R. J. Lund and A. Leith on " Pre-Cambrian Rocks of the Lake Superior Region " (*U.S. Geol. Surv., Prof. Paper* 184, 1935, 34 pp.). The conclusions reached on this subject in Monograph 52 (1911) of the U.S. Geological Survey—namely, that both weathering and igneous processes have contributed to the accumulation of the iron salts, and that both chemical and organic processes have caused their precipitation—have not been appreciably modified by more recent work.

In his Presidential Address to Section C (Geology) of the British Association at Norwich (*Sectional Addresses, British Association, Norwich* 1935, pp. 47–62) Professor H. G. Hickling discusses recent work on the petrology of coal, in which the most interest centres on the nature of the " uniform brown substance " that probably forms quite 75 per cent. of common coal. Other topics discussed are the sapropelic coals, types of coal aggregate, the rank of coals, cannels and anthracites, and Hilt's Law that the deeper seams in a given vertical section are of higher rank than the upper seams.

Through the study of his original manuscript, and of many of the actual micro-sections employed, H. G. Hickling (*Trans. Inst. Min. Eng.*, XC, Pt. 4, 1936, 243–54) has thrown much light on William Hutton's pioneer paper, " Observations on Coals," of which an abstract was published in the *Proc. Geol. Soc. London* in 1833. This was the first attempt at the micro-study of coals by means of transparent thin sections, and is further noteworthy that it was based on examination of a large variety of coals and attempted a correlation between types of coal and their microscopic features.

W. J. Skilling has published a useful account of the characters, nomenclature, chemical composition and economic significance of the banded constituents of coal (*Trans. Geol. Soc. Glasgow*, XIX, Pt. 2, 1935, 270–84) ; and has described the methods used in the chemical and microscopical examination of coal.

V. M. Goldschmidt discusses the occurrence of rare elements in the ash of coals (*Industrial and Engineering Chemistry*, 27, 1935, 1100–2). A large number of rare elements have been found, notably beryllium, boron and germanium. These elements occur in much larger amounts in coal ashes (and therefore in the coal forests and soils) than their average amounts in the earth's crust.

BOTANY. By Professor E. J. SALISBURY, D.Sc., F.R.S., University College, London.

ANATOMY.—The micellar structure of the tracheid walls of certain woods is the subject of a paper by J. C. Maby (*New Phytologist*, XXXV, 432, 1936). Assuming the structure suggested by Brown and Forsaith of a framework of longitudinal silica rods and spirally arranged cellulose and lignin units, it is suggested that the adsorbed water will tend to be located between the longitudinal molecular series, and the more or less parallel striæ on tracheid walls, which exhibit a relatively constant slope, are held to be the outcome of such organisation. A general correlation was found to obtain between the slope of these striæ and the percentage of longitudinal shrinkage and also with the widths of the lumen of the tracheids.

Experiments by Wallace showed that potassium deficiency is accompanied by a lowered water-content in the leaves of fruit plants. L. G. C. Warne, utilising Spinach Beet, has obtained similar results and finds that with increased potassium supply there is an increase in the size of the leaf cells and a corresponding decrease in stomatal frequency.

The structure of fibre-bundles present in the calyx and corolla of two Gentianaceous plants, *Coutoubea spicata* and *Lisianthus brevidentatus* has been described by R. E. Woodson (*Ann. Bot.*, L, 759, 1936). They are of especial interest as presenting the rare condition amongst angiosperms of a mesarch structure; furthermore at maturity the phloem becomes completely obliterated. A comparison of the anatomical structure of the stems of a number of species in both the flowering and vegetative conditions has been made by O. C. Wilton and R. H. Roberts (*Bot. Gaz.*, 98, 45, 1936). They find that in most examples investigated the vessels of the stems of vegetative plants are appreciably larger than those of flowering specimens of the same age. Further, the cambium is less active and the products develop thicker walls in the flowering plants than in the non-flowering.

The anatomy of two of the cushion-forming Caryophyllaceous plants from Thibet is described by P. C. Joshi (*Proc. Indian Acad. Sci.*, IV, 92, 1936). In *Thylacospermum rupifragum* it appears that a liane-like abnormal structure of the roots and shoots is attained as a consequence of the meristematic activity of the wood-parenchyma, which brings about a division of the stele into numerous irregular vascular strands.

The origin of root hairs developed from the exodermis subsequent to the formation of those from the piliferous layer is recorded by M. E. Pinkerton (*Bot. Gaz.*, 98, 147, 1936) for members of the

Commelinaceae. The species in which this development of " secondary root hairs " was noted were *Rhoeo discolor, Tinantia fugax, Commelina coelestis* and *Tradescantia geniculata*. In the first-named species the secondary root hairs arise some centimetres behind the root apex and are thick-walled from the outset.

BIOLOGICAL.—The germination of the seeds of twenty-one species of Potamogeton has been studied by W. C. Muenscher (*Ann. Bot.*, L, 806, 1936). Seeds which have been air-dried for two months or more usually fail to germinate, but they will remain viable for prolonged periods if stored in water. Successful germination was obtained with the seeds of eighteen species which had been stored in water at a temperature of from 1° to 3° C. Amongst species which yielded above 80 per cent. germination may be cited *Potamogeton angustifolius, P. foliosus* and *P. obtusifolius*. Only low percentage germinations were obtained with *P. panormitanus* (4 per cent. to 12 per cent.), *P. spirillus* and *P. zosterifolius* (5 per cent. to 16 per cent.). Seeds stored in water at room temperature mostly failed to germinate. It is noteworthy that the species showing high viability of the seeds include several in which vegetative propagation is pronounced.

FLORAL MORPHOLOGY.—Several papers dealing with the development of reproductive organs in various angiosperms have appeared in the *Proceedings of the Indian Academy of Sciences*, Vol. IV. B. Singh (pp. 75–91) describes the development in *Ranunculus sceleratus*, which in most respects is confirmatory of that already known in other members of the genus. In megasporogenesis one to three archisporial cells are recorded and T-shaped tetrads are found to be the rule. In view of the many resemblances between Ranunculaceæ and the Helobiales, it is interesting to note that Ward recorded a similar condition in *Butomus umbellatus*. B. M. Johsi, in the same Journal (pp. 139–162), describes the development of *Butomopsis lanceolata*. Here sometimes two archisporial cells are present in the ovule, more usually one, and this functions directly as the megaspore mother cell, dividing into two, of which one-half degenerates and the other becomes the embryo-sac. In the microspore development the tetrads were bilateral, tetrahedral, linear or T-shaped. The most striking feature was the occurrence in several instances of pollen grains within the stylar canal itself, and in one flower a pollen grain had begun to germinate upon an ovule inside the ovary.

INDIAN ALGÆ.—The occurrence and distribution of freshwater algæ in Northern India is the subject of a paper by M. S. Randhawa in the *Proceedings of the Indian Academy of Sciences*, IV, 36–44, 1936. The soil algæ form three communities of which one is char-

acterised by *Vaucheria sessilis* and V. *geminata* playing the same rôle as *Zygogonium* in European soils amongst grass. On the drying mud of ponds *Protosiphon botryoides* and *Botrydium* occur together, whilst the former occurs in pure stands on fallow fields. Amongst wheat *Cylindrospermum muscicola* occurs with *Riccia*. *Campylonema Lahorense* with Myxophyceæ occurs on lawns.

The bark epiphytes are Myxophyceæ such as *Phormidium*, *Lyngbya* and *Tolypothrix*.

The communities of flowing water include Rhodophyceæ such as *Chantransia*, *Batrachospermum moniliforme* and *Stigeoclonium*, also *Cladophora glomerata* as features of the swifter streams, whilst the slowly flowing waters have a richer flora with *Oedogonium* spp., *Mougeotia*, *Chaetomorpha*, *Draparnaldia*, etc. In stagnant waters the more interesting of the characteristic benthic forms are *Sphaeroplea annulina*, *Hydrodictyon reticulatum*, which are accompanied by Zygnemales. The planktonic forms are Desmids with *Pediastrum*, *Scenedesmus*, *Volvox*, *Arthrospira*, *Microcystis*, etc. The commonest attached algæ are *Oedogonium* spp., *Spirogyra* spp., *Schizomeris irregularis*, *Pithophora Kewensis*, *Ulothrix oscillaria*, *Cladophora glomerata*, and *Chaetomorpha aerea*. *Stigeoclonium* spp. occur on twigs with species of *Ulothrix* and the epiphytic flora includes diatoms and myxophyceæ.

This author in the same journal (IV, p. 408) describes a new species, *Cylindrocapsa œdogonioides*, which is the first record of this genus from India. Also (pp. 239–48) three new species of Zygnema of which one *Z. giganteum* has cells from 38μ to 48μ, and exhibits all four types of reproduction known in the genus, namely by parthenospores, by isosgamously produced zygospores, by anisogamously produced zygospores and by azygospores. The sizes of diatoms is the subject of a paper by R. S. Wimpenny in the *Jour. Marine Biol. Ass.*, XXI, 29, 1936. The study concerned chiefly two species of *Rhizosolenia*. These were collected at various times during three successive seasons and comprise 21 sets of samples which show a considerable degree of positive correlation between temperature of the sea water at a depth of 20 m. and mean diameter of the samples, *viz.* + 0·59, whilst there was a negative correlation of the same magnitude with the salinity. It is suggested that, quite apart from any effect of temperature on size by inducing auxospore formation, increase of temperature tends to bring about selection of the larger individuals. Data are furnished which indicate that, with respect to pelagic marine diatoms generally, those with the largest diameters come from warmer areas and the smallest from cold seas.

PLANT PHYSIOLOGY. By Professor Walter Stiles, Sc.D., F.R.S., The University, Birmingham.

Permeability and Related Phenomena.—During recent years the chief interest in the problems involved in the passage of dissolved substances into plant cells has centred in the phenomenon of what is now generally called " accumulation," that is, the absorption of a solute to such an extent that its concentration inside the cell is greater than its concentration outside. So many cases of this have now been recorded that there is every reason to believe that with electrolytes it is a general phenomenon. Recently, interesting data have been furnished by R. Collander for Characeæ growing in brackish and fresh water in Finland and elsewhere (" Der Zellsaft der Characeen," *Protoplasma*, **25**, 201–10, 1936). The cell sap of eight species was analysed spectrographically, as was also that of the water in which the plants were growing. In every case the concentration of each metal and of chloride was greater, generally many times greater, in the cell sap than in the surrounding medium. But the interesting point is made that when the composition of the sap of individuals growing in brackish water is compared with that of individuals of the same species growing in fresh water, it is found that the composition of the sap is more uniform than that of the surrounding medium. Hence the ratio of the concentration of any element in the sap to its concentration in the surrounding water is much higher in the case of plants growing in fresh water with a low concentration of salts, than in the stronger brackish water. Thus, in the case of *Chara ceratophylla*, this ratio for chloride with plants growing in a freshwater habitat was found to be 1350 as compared with 2·9 for plants growing in a brackish water habitat. But it is of interest to note that the difference in the concentrations of the elements in sap and medium was found to be about the same. Another general conclusion drawn from this investigation was that potassium is the most readily absorbed kation, then sodium, while calcium and magnesium are absorbed to a less extent than sodium.

Much information concerning accumulation in the large cells of *Valonia* is due to W. J. V. Osterhout and his co-workers. A few years ago there appeared an important summary of this work, together with a statement of Osterhout's views regarding the mechanism of accumulation (" Permeability in Large Cells and in Models," *Ergebnisse der Physiol. u. exper. Pharmakol.*, **35**, 967–1021, 1933). His main conclusions may be summarised as follows. The protoplasmic surface is held to consist of a non-aqueous liquid film composed of a mixture of substances which is affected by water,

acid and alkalies in such a way that the electrical properties of the film, and hence sometimes its permeability, are changed. The antagonistic action of calcium in preventing injury to the cell by, or the entrance into the cell of, other metallic ions, is held to result from the property of calcium in preventing the leaching out from the protoplasmic surface film of some of the substances of which the latter is composed.

Contrary to the more general opinion, Osterhout regards electrolytes as entering the cell, that is, as passing through the surface layer of the protoplasm, in the molecular condition and not as ions. Thus, when a cell absorbs potassium from a solution containing a potassium salt, the potassium is supposed to enter as potassium hydroxide, the hydroxyl being, of course, provided from water. Having passed through the membrane in this way the potassium inside the cell forms potassium bicarbonate. The bicarbonate ions in the cell result from the production of carbon dioxide in respiration, and so the entrance of electrolytes is supposed to depend on the production of carbon dioxide. The production of organic acids other than carbonic acid would also be effective. Since, if a cell of Valonia is in a medium containing potassium chloride, the potassium accumulates in the sap as potassium chloride, it must be assumed there is an exchange of bicarbonate ions and chloride ions between the cell and the surrounding medium. Some evidence that this is so is derived from an examination of the changes in the cell when placed in a solution of ammonium chloride. At first the pH of the sap rises, presumably on account of the presence of ammonium hydroxide and the formation of ammonium bicarbonate. The pH of the sap then falls and the concentration of chloride rises, which is explained as due to the exchange of bicarbonate and chloride ions between cell and medium.

Osterhout points out that the energy needed for accumulation is not necessarily due to respiration, and in models accumulation can be brought about without the derivation of any energy from the formation of carbon dioxide. The carbon dioxide is merely used after it has been produced. A different view is, however, taken by F. C. Steward (" Salt Absorption and Respiration of Valonia," *Carnegie Inst. Year Book*, **32**, 91–3, 1933) who states that the rôle of respiration in accumulation " seems to be rather that of a source of energy than to provide hydrion and bicarbonate ion." Steward found that when *Valonia* cells are exposed in diffuse light to relatively small volumes of sea water or modifications of it with different K/Na ratios, obtained by addition of alkali chloride, all the cultures gain very considerably in potassium and this gain

is greatly in excess of the gain in chloride. The potassium content of the cells thus appears to be determined neither by the concentration of the potassium ion in the external solution, nor by the ratio of the potassium to the sodium ion, but by some other factor.

Steward also queries the validity of Osterhout's conclusions derived from experiments with ammonium chloride, and suggests that the changes observed are attributable to the toxic action of the ammonium salt (" Mechanism of Salt Absorption by Plant Cells," *Nature*, **135**, 553, 1935). The point is also made that Osterhout's theory does not account for the fact that sodium enters the cell more rapidly in presence of ammonium chloride than in its absence.

The relation between accumulation of potassium bromide in storage tissue and respiration has been further considered by F. C. Steward and W. E. Berry (" The Absorption and Accumulation of Solutes by Living Plant Cells. VII : The Time Factor in the Respiration and Salt Absorption of Jerusalem Artichoke Tissue, *Helianthus tuberosus*, with Observations on Ionic Interchange," *Journ. Exper. Biol.*, **11**, 103–19, 1934). Disks of Jerusalem artichoke tuber were maintained in water and dilute solutions of potassium bromide, the liquids being continuously aerated. Records were obtained of the respiration, salt absorption and excretion of electrolytes from the tissue over a period of six days. Under these conditions the rate of respiration falls continuously throughout the whole experimental period, and this is the case whether the tissue is immersed in water, in potassium bromide solution throughout the whole period, or for an initial 24-hour period only. The initial rate of respiration in the potassium bromide solution was, however, slightly higher than in distilled water, and it appears that potassium bromide does bring about a slight increase in respiratory activity of artichoke tuber tissue, for such a slight rise can be obtained by the addition of potassium bromide at any time in the course of an experiment. The increase is, however, only temporary. By measurements of the electrical conductivity of the external solution the exosmosis of electrolytes from the tissue followed by their subsequent re-absorption, was confirmed. The re-absorption takes place both from the (originally) purely aqueous medium and from the solution of potassium bromide. The re-absorption is complete after 48 hours. During the whole course of the experiment the ability of the tissue to absorb potassium bromide falls so that decreasing respiration goes along with decreasing capacity to absorb potassium bromide ; the relative fall in respiration rate after the lapse of any period of time is not necessarily equal to the relative fall in rate

of salt absorption. Since the absorption of potassium bromide is accompanied by a decrease in the electrolyte concentration of the external solution and an increase in that of the cell sap, it follows that the absorption of the ions of potassium bromide is not merely part of an ionic interchange between the cell sap and the surroundings.

In a later paper F. C. Steward, W. E. Berry and T. C. Broyer ("The Absorption and Accumulation of Solutes by Living Plant Cells, VIII : The Effect of Oxygen upon Respiration and Salt Accumulation," *Ann. of Bot.*, 50, 345–66, 1936) there are recorded the results of experiments on the concentration of oxygen on the respiration of, and salt accumulation by, disks of carrot and artichoke tuber tissue and roots of potato. The tissues were kept in aerated dilute solutions of potassium bromide. It was found that in the case of both tissues the concentration of oxygen in the aerating gas stream only affected the rate of respiration when the oxygen concentration fell below that of this gas in normal air. The diminution of respiration resulting with lower concentrations of oxygen was observed in higher concentrations of this gas with storage tissues than with potato roots. Along with the diminished rate of respiration in lower oxygen concentrations goes diminished salt absorption. The absorption of both potassium and bromide are equally affected. It would appear, therefore, that only aerobic, and not anaerobic respiration processes are related to accumulation. The authors emphasize again the view that the importance of respiration for salt accumulation depends on the energy production in the former process and not merely on the production of carbon dioxide.

The last paper on the subject of accumulation to which reference will here be made is a recent one by D. R. Hoagland and T. C. Broyer ("General Nature of the Process of Salt Accumulation by Roots with Description of Experimental Methods," *Plant Physiol.*, 11, 471–507, 1936). The experimental material in the experiments here described were excised roots of barley, and it was found that potassium salts are absorbed by these against a concentration gradient, and accumulate, as in other material. The necessity for an adequate supply of oxygen for salt accumulation was also confirmed for this material, while the effect of oxygen concentration was found to be similar to the cases mentioned above. Over a range of temperature from 6° to 24° C. it was found that the temperature coefficient (Q_{10}) for salt accumulation by barley roots is from 2·5 to 5·0, a result in keeping with the view that salt accumulation is dependent upon the chemical processes involved in the active metabolism of cells.

ZOOLOGY.　By Emeritus Professor W. Garstang, M.A., D.Sc.
E. B. Ford, M.A., B.Sc., J. A. Moy-Thomas, M.A., and B. W.
Tucker, M.A., The University, Oxford.

J. Z. Young (*Q.J.M.S.*, LXXVIII, 1936, 367, and *Proc. Roy. Soc.*
(B) CXXI, 1936, p. 319) makes a notable addition to those neuro-
muscular mechanisms for instantaneous action in which giant
nerve-fibres play a part, as when earthworms on alarm retract
within their burrows, or prawns and shrimps skip from the plane
of pursuit by violent flaps of their tails. To these must now be
added the familiar cuttle-fish tactics, when by sudden contraction
of mantle-muscles and ink-sac these animals shoot off behind a
cloud of ink. The full details of the complex system are still to
come, but the general features and some remarkable differences
between Decapods and Octopods have been revealed. In Decapods
the giant-fibres are in relays connected by synapses in the stellate
ganglia. The præganglionic fibres are single gigantic neurons
arising from a pair of giant cells behind the pedal ganglia ; the
post-ganglionic fibres are syncytial, arising by complete fusion of
axons from numerous nerve-cells in the stellate ganglia. These
cells are scattered over the ganglia in *Sepia*, but concentrated into
a special post-stellar lobe of each ganglion in *Loligo*. Then comes
a remarkable change. In Octopods the giant-fibres, as such, have
all disappeared ; the poststellar lobe has been transformed into a
closed " epistellar sac " ; and the nerve-cells around it no longer
combine their axons into giant-fibres traversing the stellar nerves
to the muscles, but are produced into short stumpy processes, the
free ends of which are immersed in the semi-fluid content of the
sac, which seems indeed to be derived by secretion from, or lique-
faction of, their free ends.

From experimental and histological data Young concludes
provisionally that in Octopods the nerve-cells have become " neuro-
secretory cells," which stimulate the muscles no longer by direct
contact, but through the intermediation of a substance which they
secrete, and which is presumably carried to the muscles by the blood-
stream. This first-class discovery must have greatly delighted the
heart of Professor G. H. Parker, whose book on *Humoral Agents in
Nervous Activity* (Cambridge, 1932) should be read in connection
with Young's account. It will obviously have many " reper-
cussions," morphological and physiological.

H. G. Smith (*Carnegie Publ. No.* 475, 1936), working with
Professor C. M. Yonge at Tortugas laboratory, has investigated
the feeding and digestive processes in a Rhizostome Medusa. Since
Hamann's work in 1881 the closure of the mouth and formation

of secondary stomata along the oral arms of these medusæ have been commonly associated with an idea of the prevalence among them of suctorial habits and extra-intestinal digestion. Smith's work on *Cassiopeia frondosa* shows that these ideas, even if applicable to the more advanced Rhizostomes, are incorrect as regards the more primitive types of which *Cassiopeia* is an example. These medusæ are well known in tropical waters for their habit of lying on their backs in shallow lagoons, lazily pulsating. Their oral arms, densely fringed with digitellæ (not "tentacles"), just cover the disk in this attitude, giving them superficially the appearance of sea anemones, especially as they are studded with tentacle-like appendages, the "oral vesicles," of which there is usually one guarding the entrance to each of the stomata. Smith says that these vesicles lengthen and wave about in the presence of food, and even shoot out bags of nematocysts in strings of mucus at passing Crustaceans. Round each mouthlet is a ciliated vestibule that only opens under the same conditions. It sets up a system of parallel currents in opposite directions, continuous with a couple of opposing streams on the roof and floor of the enclosed brachial canal. Food-particles, even large Copepods, after entering the stomata, are borne centripetally in the "roof" current to the cœlenteron. In this chamber, strangely enough, the whole ciliation is exhalant, but some of the digestive filaments, the cilia of which beat downwards from tip to base, solve the problem of hauling in the food-rope against the current by thrusting their tips into the connecting ("axial") passages, thus completing the inward stream.

In contrast to Hamann's results, Smith shows experimentally that in *Cassiopeia* the food taken in is not digested until it has entered the cœlenteron. Here proteolytic enzymes are set free in the fluid, the half-digested particles are absorbed by the filaments, and amœbocytes carry the products to the body generally by way of the mesoglœa. The digestive filaments, and the "plaited membranes" at their base, are also the seat of excretory processes, so that these organs play the same part as do the mesenterial filaments of Anthozoa. Other resemblances, doubtless due to the striking convergence of habit, are to be seen in details of the external ciliation, by which the body is kept clear of silt, and in the presence of Zooxanthellæ. These, however, are mainly lodged in the mesoglœa, and are definitely intrusive, entering with the food. They appear to be tolerated, as in Madreporaria, as accessory excretory organs, but do not serve as nutriment.

We have a major and two minor criticisms to offer on Smith's otherwise excellent paper. It is surely begging an important

question to assume an endodermal lining of the brachial canals and oral vesicles (pp. 24, 44). References to "subumbrella (subumbral) endoderm" in figures and text of the gonad development need clarification (pp. 25–28). "Oral mouths" for the brachial pores and their vestibules is a tautological misnomer.

For determining the existence of extracellular digestion Smith used Yonge's method, *viz.* ascertaining the pH changes in the coelenteron after a meal, and then comparing with the pH optimum for digestion by extracts of the tissues. A more direct method may here be noted.

Sven Hörstadius (*Biol. Zentr.*, **53**, 1933, 645) fed *Helix* and *Aplysia* with pulverised "Goldfibrin," *i.e.* fine particles of colloidal gold artificially coated with fibrin. None of the metallic particles were found in the cells of the digestive gland, but quantities were found, divested of their fibrin coats, in the gut-lumen. On the other hand the liver-cells of *Pleurobranchœa*, a related Mollusc, after similar feeding, were found full of free gold-particles—clear proof of intracellular digestion in this case, and of extracellular digestion in the others.

Dakin and Fordham (*P.Z.S.*, 1936) have now fully described the anatomy of their remarkable new Nemertine, *Gorgonorhynchus repens*, and a coloured plate shows well the contrast between its bright reddish-orange body and the pale semi-transparent Gorgon-headed tuft formed by its strange, dichotomously branched proboscis. Since the announcement of its first discovery on the coast of New South Wales, the worm has been found within the Great Barrier Reef off Queensland, on the coast of Madras, and even in the North Atlantic at Bermuda. The mode of eversion and introversion of the proboscis, with its curious axillary valves and absence of retractors, is carefully described. Apart from this organ, however, the new worm turns out in every respect to be a typical Lineid of the Heteronemertine order. Nothing is said as to its habits, except that it is found under stones. So marked a deviation from the normal form of the proboscis should help to determine the function of this characteristic but enigmatic Nemertine organ.

In a Presidential Address to the Linnean Society of New South Wales (*Proc.*, LX, Parts 1, 2, 1935) Professor Dakin also gives an extensive summary of his own and others' work on the relations of the internal and external media of aquatic animals, and on the various steps which they have taken towards the attainment of a "steady state," especially crabs and fishes. The emancipation of animals from their saline environment was clearly the first step towards environmental freedom as realised by birds and mammals.

Dakin was one of the first critics of Macallum's famous dictum that the fixed salinity of the body-fluids in terrestrial animals reflects the composition of the primæval seas which their ancestors deserted. This criticism he repeats with additional examples from Australian waters of homoiosmotic crabs, and the varying tonicities of their blood, full details of which are given in a separate paper by his co-worker Enid Edmonds, which is full of interest, in a later number of the same journal (Parts 3, 4, 1935). Macallum's static conception embodied an important fact of correspondence, but as an explanation has been completely replaced by dynamic considerations. As Pantin said in a recent review of the subject, and in complete harmony with Dakin : " The composition of the body-fluid cannot undergo evolution in the sense applied to morphological structures. It is the surface membranes which have undergone evolution " (*Biol. Revs.*, No. 4, 1931).

Following on Keck's demonstration (*J. Exp. Zool.*, **67,** 1934, 315) that the sexual differences in the plumage of the House Sparrow (*Passer domesticus*) is independent of hormonal control, Nowikow (*Biol. Zentr.*, **56,** 1936, 415–27) has now shown that the same is true of the Bullfinch (*Pyrrhula pyrrhula*). In a total material of 59 birds it was found that neither gonadectomy in either sex nor injection of ovarian hormone in normal or castrated males produced any effect on the plumage. Injection of ovarian hormone into females induced a strong hypertrophy of the oviduct.

Hitherto studies on the control of secondary sex characters in birds have tended to concentrate on the fowl, where the dependence of female plumage on the presence of the ovary is now well known. It was a natural assumption that conditions were similar for all birds, but this now proves not to be the case. Keck's and Nowikow's results are theoretically important because they appear to remove any obstacle to interpreting bird gynandromorphs as due to irregularities of chromosome distribution of substantially the same kind as have been actually demonstrated in insects. What is true of the Bullfinch and Sparrow may fairly be presumed to hold good for the whole order Passeriformes, and all the typical gynandromorph birds that have been described are of Passerine or allied types. The fowl described by Macklin (*J. Exp. Zool.*, **38,** 1923, 355–75) was gynandromorphic in its organs but not in its plumage, which was hen-like, as would be expected from the experimental data. Bond's pheasant (*Jour. Genet.*, **3,** 1913, 205–17) with ovotestes had each tail feather male on one side and female on the other, and was therefore not a normal bilateral gynandromorph. It appears to stand in a class apart, and will almost certainly be

found to require a special explanation, possibly in terms of differential susceptibilities correlated with local differences in growth-rate, as suggested by Lillie. Its real significance is still obscure.

PALÆONTOLOGY.—Rayner (*Ann. Mag. Nat. Hist.*, **19**, 1937) has given a very complete account of the anatomy of the Liassic fish *Leptolepis bronni*, and has discussed its affinities. The course of the supraorbital lateral line canal is of particular interest, since it runs back into the parietals as in Palæoniscids. She concludes that the Leptolepids should be placed very near the ancestry of the Teleosts, but their relationship with Holostei is not certain and they may have been independently derived from Palæoniscids.

Säve-Söderbergh (*Kungl. Svensk. Vetensk. Handl.*, **16**, 1936) has described the Triassic Stegocephalians from Spitsbergen. The neurocranium is treated in great detail and compared with that of the Crossopterygian *Rhizodopsis*.

Moy-Thomas (*P.Z.S.*, 1936, and *Geol. Mag.*, **73**, 1936) has described the anatomy of several Carboniferous Elasmobranchs, which have thrown much light on the evolution of this group. *Ctenacanthus costellatus* has been shown to have a fin structure which leaves no doubt that the Ctenacanth sharks occupy a position between the Cladoselachii and the Hybodonts. *Tristychius arcuatus* is shown to belong to a family distinct from the Hybodonts. The Cochliodonts are shown to be almost certainly ancestral to the living Holocephali. The Cochliodont *Helodus simplex* had a holo-stylic jaw suspension, a skull similar to that of the Holocephali but less specialised, and paired and unpaired fins very similar to those of the modern *Chimœra*. There were apparently, however, no cephalic or anterior pelvic claspers.

GENETICS.—Beadle and Ephrussi, *Genetics*, **21**, 1936, 225–47 ; Plagge, *Z. f. ind. Abst. u. Vererb.*, **72**, 1936, 127–37 ; Medvedev, *C.R. Acad. Sci.*, *U.S.S.R.*, **14**, 1936, 45–7. These three papers develop with much success a technique which has been used but little in genetic studies of the Insecta, that of tissue transplantation. This is an exceedingly difficult procedure in the Arthropoda, owing to the cuticle and open blood-vascular system. It has, however, been employed on a few occasions in the past, as in the demonstration by Meisenheimer that sex-determination in the Lepidoptera is zygotic.

Beadle and Ephrussi have now worked out a new and very successful method of transplantation, which should provide valuable opportunities for the study of physiological genetics in the future. It has already led to results of importance. Working on *Drosophila*, these authors have transferred optic disks from one larva to another. These grow well in the host, and allow a study of any interaction

between different eye-colour genes in host and graft. Of the 26 eye-colour mutants used, 24 proved autonomous, while genetically *vermilion* (*vv*) or *cinnabar* (*cncn*) optic disks produced wild-type pigment in wild-type hosts.

In the reciprocal experiment of grafting wild-type optic disks into mutant hosts, all remained wild-type except in a larva homozygous for the *claret* gene, in which the disk developed as a claret-eye.

Of great interest is the fact that grafts carrying the vermilion gene interacted with any one of seventeen different mutant eye-colour genes in the host to produce wild-type eye-colour. Also a wild-type optic disk from a young larva produced an eye of intermediate colour when grafted into an older host.

It has long been known that genes interact with one another to produce their effects. But here we have a demonstration that, in some instances, the genetic constitution of the rest of the body can override that of a particular tissue.

Interesting extensions of this technique are possible. Beadle and Ephrussi have found no effect on the eyes of *Drosophila* by transferring gonads from individuals of another constitution. On the other hand, this experiment was successful in *Ephestia*, for Plagge shows that changes in eye-colour could be obtained in this moth by transplanting genetically different gonads. Medvedev has extended these studies to the wing-rudiments of *Drosophila*, using as a test wild-type and four mutant body-colours. His results show that, in each instance, the imaginal disk differentiates autonomously.

PHYSICAL ANTHROPOLOGY. By L. H. Dudley Buxton, M.A., D.Sc., Exeter College, Oxford.

For a good many years Professor Fleure and his assistants have been studying the physical anthropology of western Britain. His method has always been to take samples of the present population excluding those whose parents and grandparents are immigrants of the district examined. In his last paper, which is likely to become the classic authority on the population of the Isle of Man (Elwyn Davies and H. J. Fleure, " A Report on an Anthropometric Survey of the Isle of Man," *Journ. Roy. Anthropological Institute*, LXVI, 1936), observations are discussed which were made by Mr. Davies on 1200 men, all of whom had four grandparents belonging to the island and, as a further safeguard, whose surname was associated with the island previous to 1800. As an introduction the natural regions of the island were investigated and it was later found, though the authors are not able to give any reason for it, that

divergencies in the population are in fact associated with differences in geographical surroundings. Generally speaking they found that a "bundle of characters," tall stature, fair colouring, and long noses, were introduced into certain areas and have persisted ever since. They associate this with the known Norse invasion of the northern part of the island in the early Middle Ages. In other areas there are darker longheads, with shorter noses, and generally smaller measurements are more numerous. In these parishes the authors conclude there is a population possessing characters which are analogous to many of the people found in Welsh moorland valleys, the "little dark Welshmen." The authors suggest that this element is found in oceanic Europe southwards from Britain and believe that it owes its origin to early migrations from the western basin of the Mediterranean. They conclude by showing that there are some definite distinctions between the population of the Isle of Man and of Wales. It should be noted that in discussing the ethnology of the Isle of Man Fleure and Davies have relied entirely on living measurements. There has been a tendency among many anthropologists of recent years to lay greater stress on the measurement of skulls, mainly on the ground that they can be measured with greater accuracy. This tendency is deprecated by Professor R. A. Fisher (" The Coefficient of Racial Likeness and the Future of Craniometry ") in the same number of the *Journal of the Royal Anthropological Institute*. He suggests that in order to clear up the fundamental problems of ethnographic research the study of modern living problems is absolutely essential. In the course of this paper Professor Fisher discusses at some length the fundamental problems concerned with the " coefficient of racial likeness." This coefficient, which was originally suggested by the late Professor Karl Pearson and developed and elaborated by Dr. Morant, has been used extensively by the Biometric school of anthropologists, especially in the pages of *Biometrika*, and recently in the first volume of the *Census of India*, 1931, Vol. I (Part III—Ethnographical, A : " Racial Affinities of the People of India," by B. S. Guha). Speaking generally it gives a weighted average of the differences between the measurements of two populations which it is desired to compare. Professor Fisher is of opinion that the name is an unfortunate one, as it really tests differences rather than likeness, and secondly that it is not a very reliable test of significance. He lays stress on the way in which correlation between the various measurements may affect the value, and that in fact we get very high or very low values far more frequently than we ought to get according to mere chance. The paper is likely to raise some con-

siderable criticism among the advocates of the use of the coefficient. It provides, however, an extremely clear statement of what exactly the coefficient means—a statement all the more necessary since the formulæ used are somewhat difficult for the physical anthropologist without mathematical training to grasp. At the same time such a reasoned criticism is apt to be of great advantage in clearing the air especially if it leads, as it is likely to do, to a frank discussion of both sides of the question. At present it need only be said that, as has already been shown on previous occasions in this column, most valuable results have been gained by its use ; it is, however, of importance to note, as has been pointed out many times in the pages of *Biometrika*, that high values of the coefficient are of little value.

The failure to recognise this point clearly has made Guha's account of the people of India, already alluded to, open to criticism. His conclusions, however, are of interest and may be summarised here, with the reservation that later research may invalidate some of his conclusions. He suggests that there are in India to-day five main types, together with two others in the sub-Himalayan regions. The first four are, first, a short, brown-skinned, long-headed race, which is the predominant element in the greater part of the lower stratum of the population of northern India, including to a certain extent the Punjab ; secondly, along the west coast and Bengal a round-headed race of medium stature, and thirdly, among such peoples as the Kaffirs and Pathans, another long-headed, tall, long-faced strain. In addition to the above Guha believes that there is a submerged pygmy strain among some tribes, and, more important, a short, dark, long-headed race, with prominent eyebrows, who make up the aboriginal tribes of central and southern India and are allied to the Veddas of Ceylon. Risley believed that Mongoloid peoples occurred in considerable numbers in the north-eastern part of peninsular India, a point of view which is attacked by Guha, who believes that, though two different strains of Mongoloid peoples do occur in India, they are limited in their extension to the sub-Himalayan region of the Brahmaputra valley and the regions lying to the east of Hindustan. He admits that at present there is not sufficient evidence to link up these modern races with the ancient peoples, but makes the following suggestions. First, that in the fourth millennium B.C. there were two long-headed races, differing from one another in the height of the cranial vault. At a later date, but still at an early period, an Armenoid type entered the country and penetrated to southern India and later into Bengal. In the Early Iron Age skulls like the Combe Capelle skull from

Europe are found. He also draws attention to the presence in northern India of skulls which may be outliers of the Nordic Race.

Population problems are beginning to interest physical anthropologists, although in the Pacific region the masterly work of Rivers has made the population of that region at least familiar to most anthropologists. Clark Wissler (*American Museum of Natural History*, XXXVI, Pt. 1) discusses the changes in the population profiles among the northern Plains Indians. He believes that there are relations between social changes and population shifts. The birth rate apparently does not change appreciably, but death rates, adult sex ratios and so on are, it would seem, correlated with economic and social changes. For example, the Wood and Plains Crees are morphologically similar, but follow different modes of life and have correspondingly different death rates and sex ratios. In general Wissler concludes that population studies suggest that, where the population characteristics approximate to those of the corresponding classes in the surrounding White population, they will be following the same manner of life as the latter.

Although it hardly comes within the scope of this article, attention may be drawn to the extremely interesting account of the history of the Anthropological Society of Tokyo by A. Matsumura, who was formerly secretary of the Society, published in the *Zeitschrift für Rassenkunde*, Band 4, 1936.

The literature concerning " Peking Man," *Sinanthropus pekinensis*, continues to grow. The most important recent contribution is that by Franz Weidenreich (*Palæontologica Sinica Series D*, Vol. VII). Peking Man differs from most other examples of early man in being represented by a number of specimens. The author of the present paper, on the basis of a study of the lower jaws, concludes that there is not the least doubt that this fossil must be considered as the direct forerunner of recent man. He goes on further to suggest that it forms a definite means of tracing the line which human evolution has followed in the reduction of the masticatory apparatus and represents not a " specialised " but a " generalised " type. He believes that his further researches have confirmed his previous hypothesis that the ancestors of the modern human race were anthropoids, with strong well-developed canines, which were less projecting than those of the modern anthropoids. He concludes that, although the question of speech cannot be decided from the examination of the morphology of bony structures, yet it is probable that this remote ancestor of the human race already had the power of articulate speech.

NOTES

In future the *Proceedings of the Prehistoric Society*—perhaps it is unnecessary to add "formerly of East Anglia"—will appear in two semi-annual parts, instead of one annual volume as heretofore. The shortened interval between issues may be taken as an indication of a wider appreciation of the fact that the activities of the Society and the communications it publishes, if frequently rooted in East Anglia, in virtue of the problems with which they deal, are always of significance for prehistoric studies at large, and delay in publication is undesirable.

The communications in the present issue are nicely balanced as between the local and the broader issues. Of the five major communications three geographically stand outside, though one of these deals with matters in which East Anglia is intimately concerned. The contents open, appropriately enough, with a report by Dr. Clark on his excavations of the Arminghall Circle, near Norwich, in which, after describing the excavation and its results, he discusses the affinities of the wooden circle, of which the Wiltshire Woodhenge is the type. Although Mr. Clark is unable to arrive at any completely decisive conclusion as to origin, his comparative study is a valuable piece of work. His argument for a " beaker " age appears conclusive.

Passing over, though not on account of lack of interest, communications by Messrs. W. H. B. King and H. P. Oakley discussing various pleistocene and holocene deposits in the lower and middle Thames Valley, and by Messrs. C. H. Drew and Stuart Piggott on the excavation of a Dorsetshire long barrow, attention may be directed more especially to a paper by Mr. O. G. S. Crawford on the field archæology of the Royston district, which once again demonstrates the value of surveys from the air for field archæology. By means of air photography Mr. Crawford has identified a number of

previously unknown round barrows, increasing the number now recorded from seventeen to sixty with a possibility of further additions, as well as added a large circle, four camps and other evidence of a once thickly populated area to the archæological record of the district.

Lastly, reference must be made to the study of the megalithic monuments of Wales by Mr. W. F. Grimes, whose map of these is in course of publication by the Ordnance Survey. The paper published here is the first systematic study of the megalithic monuments of Wales to be made, and consequently is of no little value to the student of British prehistoric archæology. It is of interest to note that the author considers that the northern area of the Principality received the megalithic culture in its greatest vigour.

The South African Institute for Medical Research. Annual report for 1935 (P. J.)

The Department of Industrial Hygiene of the Institute has continued its long investigation on the matter of air-borne dust in the mines of the Witwatersrand. It has been held that only certain dusts were associated with tuberculo-pneumonokoniosis and that this association was the significant factor in the disablement and death of those exposed, and statistical analysis seemed to prove that it was free silica that was the tuberculosis-facilitating dust. The position of this question has been shaken by the hypothesis put forward by Dr. W. R. Jones that while most air-borne dust and dust recovered from the lungs of those exposed was free silica, yet the majority of small particles, by number, were micaceous, and the number of particles is the dominant factor in the injury to the lungs. But does micaceous matter constitute the majority of the smallest particles ? It has been shown that down to the minimal size identifiable by petrological methods (just below 2 microns) the majority of the air-borne particles and those recovered from the lungs are particles of free silica, but 70 per cent. of the particles by number are of the order of 1 micron or less. Further work seems to support the view that the micaceous matter rises with the diminution in size of the particles. Free silica is thought to be tuberculosis facilitating because it is slowly soluble in the lungs and this solute lowers the resistance of the lungs to after-coming infections. Is the micaceous matter in air-borne dust also soluble, and is this solute toxic ? It is not possible to collect sufficient particles of 1 micron and under for inhalation or intratracheal injection experiments, but possibly subcutaneous and intravenous injection or abscess fixation experiments may enable a comparison

to be made of the behaviour of free silica and micaceous particles of minute size; if the particles are mostly micaceous and insoluble they may be of little importance, if they are free silica they should injure the lungs by their solubility " even if they do not contribute to scar formation." This interesting enquiry is still proceeding.

The biochemical department has considered questions arising from the native diet enquiry—investigation of plants and other foodstuffs used by natives in their homes. Especial notice is made of young shoots and leaves of common plants—pigweed, black jack, purslane, nettle, lerotho, sowthistle, etc.—which are cooked and eaten like spinach. These plants are not only rich in antiscorbutic vitamins (C and A) but also contain high amounts of calcium, iron and mineral salts, and form a valuable addition to the usual cereal diet where there is a deficiency of milk. The leaves are also dried and stored for winter use. Mention is made of a leaf belonging to the yam family which contains no less than forty times as much iron as spinach, whilst both the bulb and leaf of another specimen were found to be exceptionally rich in antiscorbutic vitamin, the bulb being twice and the leaf no less than twenty-five times as rich in this substance as orange juice. This plant (name not stated), however, has poisonous properties and is used as a food only after these have been removed by treatment. Several types of fermented beverages were examined; in eleven samples values of 2 to 5 mg. of ascorbic acid per litre were found, and one sample of home-made beer as much as 37 mg. per litre. Since these native beers are teeming with growing yeast cells they should be a good source of the vitamin B group. These beers contain 5 to 13 per cent. of solids, and are more of the nature of a food than an alcoholic drink. That prepared from the fruit of the Marula tree (*Sclerocarya caffra*) is notoriously intoxicating and the fruits are highly antiscorbutic.

Observations on the composition of lucerne have proved its suitability for human food, being rich in vitamins A and C, minerals and protein; eaten raw or cooked as spinach it is rather fibrous unless obtained young and fresh; it can be used as an emergency foodstuff of considerable value and it will improve the nutritional qualities of a ration at a very low cost. The dried leaves form a highly concentrated source of antiscorbutic vitamins readily transportable to areas where fresh fruit and vegetables are temporarily unobtainable.

Other points of interest are the composition of staple foodstuffs, investigations on the reserves of vitamin C in Europeans and in natives by the test dose method—the antiscorbutic value of human milk, the uses of synthetic vitamin C, etc. A warning is also

expressed on the seriousness of the nutritional problem and the cost to the State in terms of health and vitality.

From the pathological department it is reported that there is no evidence of an action of African snake venoms or anavenoms on the majority of malignant tumours in man; nor is there evidence of any sensitisation of cancerous tissues to X-rays by venom injections.

Several attempts were made to produce artificial cancer in tissue culture. Cells from a normal non-cancerous chick embryo heart of 7 days' incubation were cultivated for one month in normal serum embryo extract and then for another month in a similar extract to which a droplet (*sic*) of a dibenzanthracene lecithin emulsion has been added; although at this period the culture no longer contained dibenzanthracene, injection of some of the treated culture into fowls produced in one case a rapidly growing sarcoma which eventually killed its bearer with metastases in the lungs. This tumour is now being transmitted to other fowls and its filterability and biology are being studied.

The institute malarial research station has been at Eshome, Zululand, during the year. Previous work having established that *A. funestus* and *A. gambia* are house-frequenting, and also that most infected insects are found indoors and further that *A. funestus* remains indoors until its eggs are matured, control of malaria by anti-adult measures became possible. Two sets of experiments were made—(*a*) to find what interval should elapse between each spraying of habitations, and (*b*) to test efficiency and economies of anti-adult versus anti-larval campaigns. Huts were sprayed daily, bi-weekly and weekly; huts receiving no spray were taken as controls. From determination of spleen rates and spleen sizes at the beginning and at the end of the experimental period (Nov. to June) it was clearly shown that the spleen rates and size were reduced and the more often spraying takes place the lower the spleen rates will fall. Anti-larval measures were effective when breeding was restricted and cost was a minor consideration, but investigation of the spleen rates in children from December to the end of May showed that anti-adult measures were more effective and only one-third the cost of the anti-larval campaign.

Abbe's Lenses (F. W. J.)

Messrs. Carl Zeiss have recently circulated a small book (Moritz von Rohr, *Abbe's Apochromats*), commemorating the 50th anniversary of their announcement of Abbe's apochromatic objectives. M. von Rohr, one of the last of Abbe's assistants, describes the work which led to the appearance of this type of lens, while the rest of the

book is devoted to reprints of two relevant articles—the notice of the Zeiss Apochromats and Compensating Eyepieces which appeared in the *Journal of the Royal Microscopical Society*, and Abbe's paper, " On Improvements of the Microscope with the Aid of New Kinds of Optical Glass," which appeared in the same periodical. It is a tribute to these remarkable lenses that Messrs. Zeiss should have thought them worthy of commemoration in this manner.

Miscellanea

The New Year honours list published on February 1 included the following names :—*K.C.B.* : Mr. H. T. Tizard, rector of the Imperial College of Science and Technology ; *Knights Bachelor :* Dr. J. A. Arkwright, of the Agricultural Research Council ; Prof. J. C. G. Ledingham, director of the Lister Institute ; Lieut.-Col. A. G. Lee, engineer-in-chief, General Post Office ; Dr. E. K. Le Fleming, chairman of the Council of the British Medical Association ; Mr. C. G. Trevor, Inspector-General of Forests, India. *C.B.* : Mr. F. C. Cook, chief engineer, Roads Department, Ministry of Transport ; Dr. M. F. Lindley, comptroller-general of patents, designs and trade-marks. *C.I.E.* : Dr. D. Penman, chief inspector of mines in India ; Dr. G. C. Ramsay, deputy director of the Ross Institute of Tropical Hygiene. *C.B.E.* : Mr. A. G. H. White, lately librarian to the Royal Society. *O.B.E.* : Mr. C. Chaffer, principal technical officer, Compass Department, Admiralty ; Prof. S. M. Dixon, lately a member of the Safety in Mines Research Board ; Dr. G. H. Pethybridge, lately assistant director of the Plant Pathological Department, Harpenden.

Prof. E. B. Bailey, F.R.S., professor of geology in the University of Glasgow, has been appointed to be director of the Geological Survey of Great Britain and of the Museum of Practical Geology.

The gold medal of the Royal Astronomical Society has been awarded to Dr. Harold Jeffreys for his researches into the age of the earth and other planets and on the origin and age of the solar system.

The Wollaston medal of the Geological Society has been awarded to Prof. W. Lindgren of the Massachusetts Institute of Technology for his researches on the mineral structure of the earth ; the Murchison medal has been awarded to Dr. L. J. Spencer for his contributions to mineralogical science.

Sir Arthur Hill, director of the Royal Botanic Gardens, Kew, has been awarded the Veitch gold medal of the Royal Horticultural Society.

The Council of the Physical Society has awarded the Duddell medal to Prof. W. G. Cady, of the Wesleyan University, Connecticut.

Mr. D. L. Edwards has been appointed to succeed the late Dr. W. J. S. Lockyer as director of the Norman Lockyer Observatory, Salcombe Hill.

We have noted with great regret the announcements of the death of the following well-known scientific men during the past quarter : Sir John Aspinall, past-president of the Institutions of Mechanical and Electrical Engineers ; Sir John Bland-Sutton, surgeon ; Prof. J. T. Cash, F.R.S., of Aberdeen, pharmacologist ; Señor Don Juan de la Cierva, inventor of the autogyro ; Mr. T. Crook, principal of the Mineral Resources Department, Imperial Institute ; Prof. W. Ellis, of the Royal Technical College, Glasgow, bacteriologist ; Prof. D. F. Fraser-Harris, physiologist ; Dr. J. K. Fotheringham, F.B.A., reader in ancient astronomy in the University of Oxford ; Dr. C. V. Jackson, spectroscopist ; Sir Herbert Jackson, F.R.S., chemist ; Prof. E. H. Kettle, F.R.S., pathologist ; Prof. F. A. Laws, emeritus professor of electrical measurements in the Massachusetts Institute of Technology ; Prof. J. A. MacWilliam, F.R.S., emeritus professor of physiology in the University of Aberdeen ; Prof. K. K. Mathur, principal of the College of Science, Benares, geologist ; Dr. J. M. H. Munro who, with John Wrightson, introduced basic slag into agriculture ; Dr. E. E. Prince of Ottawa, fisheries expert ; Mr. P. A. Ellis Richards, president of the Society of Public Analysts ; Dr. A. A. Robb, F.R.S., mathematician ; Sir John Robertson, professor of hygiene and public health in the University of Birmingham ; Dr. G. Schack-Sommer, sugar chemist ; Dr. W. Schmidt of Vienna, meteorologist ; Sir David Semple, bacteriologist ; Sir Grafton Elliott Smith, anthropologist ; Dr. R. J. Tillyard, F.R.S., entomologist ; Prof. H. Westergaard of Copenhagen, statistician.

Viscount Falmouth has been appointed to be a member of the Advisory Council of the Privy Council for Scientific and Industrial Research. Prof. A. C. G. Egerton, F.R.S., has retired from the Council on the completion of his term of office.

The Council of the Royal Society has decided that the number of Fellows elected each year shall be increased from 17 to 20. The Society will receive the income from about £200,000 from the estate of Mr. H. B. Gordon Warren, who died in 1932, and the residuary estate of Sir Joseph Petavel will provide it with a capital sum of about £40,000.

The Fourth International Grassland Congress will be held in Great Britain under the presidency of Prof. R. G. Stapledon from July 8 to 23. The members will first meet at Oxford and then visit districts of topical interest in various parts of the country.

The British Association has arranged to meet at Nottingham this year during the week September 1–8. Sir Edward Poulton is president of the Association and the presidents of the sections are as follows : Section A (Mathematics and Physics), Dr. G. W. C. Kaye ; B (Chemistry), Dr. F. L. Pyman ; C (Geology), Prof. L. J. Wills ; D (Zoology), Prof. F. A. E. Crew ; E (Geography), Prof. C. B. Fawcett ; F (Economics), Prof. P. Sargent Florence ; G (Engineering), Sir Alexander Gibb ; I (Physiology), Dr. E. P. Poulton ; J (Psychology), Dr. Mary Collins ; K (Botany), Prof. E. J. Salisbury ; L (Education), Mr. H. G. Wells ; M (Agriculture), Mr. J. M. Caie.

The will of the late Sir Henry Wellcome vests the whole of the shares in the Wellcome Foundation (which owns the business of Burroughs, Wellcome & Co.) in five trustees who, after meeting certain prior charges, are instructed to use the balance of the profits of the Foundation for the maintenance of " The Research Undertaking Charity " and " The Museum and Library Charity." The former is a fund for the advancement of researches in medicine or branches of science bearing thereon including the financing of research expeditions, while the latter is devoted to the establishment of research museums and libraries of like character. The trustees include Sir Henry Dale, director of the National Institute for Medical Research, and Prof. T. R. Elliott of University College Hospital and Medical School.

According to a circular issued from the Imperial Institute a demand has arisen for columbite, a mineral hitherto regarded as a troublesome constituent of certain alluvial and surface deposits of tin ore in Nigeria. During the last few years several uses have been discovered for columbium, notably in the manufacture of stainless steel. The addition of ferro-columbium (containing 50 to 60 per cent. of the latter metal) to certain chromium nickel steels reduces their tendency to intergranular corrosion, especially when exposed simultaneously to heat and chemical attack, and renders them more ductile.

An account of the industrial uses of diamond is given in the *Bulletin* of the Imperial Institute (Jan. 1), from which it appears that

at least 60 per cent. by weight of the total diamond output consists of stones of inferior grade suitable only for industrial purposes. About 40 per cent. of these are used to tip tools for truing up abrasive wheels made of emery, carborundum, etc., while 20 per cent. are employed for wire-drawing. Diamond-pointed tools are used for machining many non-ferrous metals, *e.g.* for big-ends and aluminium alloy pistons. Circular saws made of steel set with diamonds round their edges are used to cut building stones and it is stated that a saw of this type 7 feet in diameter with 1000 or more diamonds embedded in its edge can be used for 2500 sawing hours before the stones need re-setting. Most of these stones, come from the Belgian Congo and Angola. African bort costing 10*s.* per carat is being used to stud the crowns of rock-boring drills and has reduced the cost of drilling in the Rand gold mines by at least 10*s.* per foot.

The same number of the *Bulletin* contains an account of the use of magnesium in the manufacture of light alloys. At present the metal used in this country is obtained from imported magnesite, but there is a prospect that it may be obtained from dolomite. The weight of the metal imported increased from 2025 cwt. in 1933 to 27,665 cwts. in 1936, mostly from Germany and the U.S.A. Germany has ample supplies in the form of magnesium chloride obtained as a by-product from the potash industry.

The *Report of the Building Research Board* for 1935 (H.M. Stationery Office, 3*s.* 6*d.* net) contains, as usual, a good deal of information of general interest. Noise, bug infestation and fire resistance receive particular attention. The noise problem is being tackled by treating buildings as a whole rather than as a number of separate sound-transmitting parts, and it is hoped to obtain a solution applicable to flats designed for " working people," since it is considered that in luxury flats " noises can be suppressed by thick carpets and other means." The bug trouble is serious, for it is estimated that in many areas practically all the houses are infested to a greater or less degree. The solution as far as new buildings are concerned is regarded as the responsibility of the building industry, which should devise a form of construction which would enable disinfection by poisonous gases to be carried out without danger to the occupants of neighbouring houses.

Fire-resisting materials are to be tested at the new fire-testing station which is being erected by the Fire Offices Committee at Elstree and, when sufficient data have been obtained, standard specifications for such materials will be prepared.

The *Report* refers to the danger of using chemical substances for

cleaning stonework or brickwork. Alkaline as well as acid cleaners may easily produce decay and other deleterious effects. The cause of the white efflorescence sometimes seen on brickwork has been investigated and shown to depend on the soluble magnesium compounds (in particular magnesium sulphate) contained in the bricks. The remedy lies with the brick-maker, for the only certain method of eliminating magnesium sulphate is to ensure that the firing temperature exceeds 1050° C.

The Science Museum, South Kensington, has issued a most interesting booklet written by Mr. W. T. O'Dea, giving a general survey of the problems and methods of illumination. In particular, it gives rules and data for making calculations of the illumination of ordinary rooms. Some of the data are very interesting. For example, the nominal lumen ratings of both pearl and clear tungsten filament lamps are the same. A 60-watt, 200–250-volt coiled coil lamp should give an initial output of 690 lumens (about 55 candle-power), a 100-watt lamp 1320 lumens (105 candle-power), and a 150-watt single-coil lamp 2030 lumens (160 candle-power). The illumination suggested as being desirable for different kinds of work by the British Electric Lamp Manufacturers' Association are, as a rule, higher than those recommended by the American Illuminating Engineering Society—given in brackets in the list which follows : school classrooms 6–10 (2–5) ; electric manufacturing 10–15 (5–10) ; type-setting, linotype engraving 25–50 (10–20) ; cotton spinning 6–10 (5–10) ; welding 10–15 (2–10) ; library reading-room (on books) 15–25 (?), the unit being one foot-candle in each case. The booklet concludes with a brief historical survey of methods of lighting. It may be obtained from H.M. Stationery Office, price 6d. or, by post, 7d.

ESSAY REVIEWS

" J. J.," A GREAT DOYEN OF PHYSICS. By G. W. C. KAYE,
O.B.E., M.A., D.Sc., Superintendent, Physics Department, The National
Physical Laboratory. Being a Review of **Recollections and Reflections**, by Sir J. J. THOMSON, O.M., D.Sc., F.R.S. [Pp. viii + 451,
with 10 plates.] (London : G. Bell & Sons, Ltd., 1936. 18*s.* net.)

IN his eightieth year, the Master of Trinity has given us a book of
quite extraordinary interest, and one to which it is a great pleasure
to pay tribute. A fascinating record of happenings in a monumental
career, the achievement is the more remarkable when we learn that
the author has never kept a diary, and that in his own view, his
memory " has always been very patchy, good for things which
are amusing, bad for those which are instructive."

Be that as it may, one cannot fail to be both amused and
instructed by the catholic range of interests referred to in this book,
which begins by taking us back to schooldays at Manchester, when
" there were no bicycles, no motor-cars, no aeroplanes, no electric
light, no telephones, no wireless, no gramophones, no electrical
engineering, no X-ray photographs, no cinemas and no germs, at
least none recognised by the doctors." Comparing present schoolboy days with his own, the Master is inclined to think that nowadays
not enough advantage is taken of the ease with which boys can
learn things by heart ; " indeed some people seem to regard memory
almost as something which ought to be apologised for."

Five years followed at Owens College, and we are reminded of
its chequered fortunes in those days, and of the parts played in
Manchester scientific affairs by Roscoe, Dalton, Joule, Osborne
Reynolds, Balfour Stewart, and the famous Manchester " Lit. &
Phil." We read of Roscoe's tireless pioneer work in a forty years'
campaign to make the public realise the importance of science in the
educational, social, and industrial life of the nation, and of the
public apathy which had to be overcome in these matters. This
was reflected in a report of a Royal Commission on the teaching of
science in schools, which was then " regarded with jealousy by the
staff, with contempt by the boys, and with indifference by the

parents." Much water has flown under the bridges since those early days of " penny lectures " and " penny readings," which came to flourish particularly in the North with its Mechanics' Institutes. In later years the then Science and Art Department did effective work in spreading the gospel of science ; and nowadays we look largely to the British Association to bring home to the community how much its welfare owes to science and its developments.

Looking back, the Master feels that few can have owed more to scholarships in their early careers than he did ; and it was his success in an entrance scholarship examination at Trinity College which took him to Cambridge in 1876, where in all the intervening years he has " kept " every term and part of each " Long," whether as undergraduate, Fellow, Professor or Master. In bringing to mind his undergraduate lodgings, he will find many an answering chord among his readers when he confesses that he has never been able to remember, while he was working, to attend to a fire, nor work to any advantage when the room got cold. As was customary then, the " Little-Go " was taken after going up to Cambridge, the compulsory Greek being mastered with the aid of " a long list of words which were irregular to the point of impropriety in their behaviour."

As an undergraduate, Thomson " coached " with Routh, to whose incredibly successful methods for the Mathematical Tripos of those days, glamour still attaches ; he does not think it is prejudice which makes him prefer the examination system when he took his degree to those which succeeded it ; " examinations are about the last things to which sentiment would cling." He attended lectures by Niven, Glaisher, Cayley, Adams and Stokes, and in contrasting their several styles thinks that for a lecturer to read from notes deprives the lecture of much of its charm ; " the best teacher is not always the clearest lecturer, but the one who is most successful in making his pupils think for themselves."

The lecturer too may not fail to find profit, particularly if he is engaged on research work. The Master indeed approves of the system of combining some teaching with research. " There is no better way of getting a good grasp of your subject or one more likely to start more ideas for research." The psychology of the incidence of ideas must be very interesting. " New ideas about a subject generally come when one is not thinking about it at the time, and it is remarkable that when ideas come in this way, they carry conviction with them."

The author's examination for the Mathematical Tripos is described as an arduous, anxious and very uncomfortable experience. Despite insomnia he came out Second Wrangler, Larmor being

Senior. At 23 Thomson was elected a Fellow of Trinity, at the first attempt, with a dissertation on the application of Lagrangian and Hamiltonian equations to various problems in physics and chemistry. Three years later he received the Adams Prize for an essay on the motion of vortex rings, and about the same time he published an important theoretical paper on the electromagnetic mass of electrified particles moving at very high speeds, such as were associated with the cathode rays in the experiments of Crookes and Goldstein.

At the age of 28, Thomson succeeded Lord Rayleigh as Cavendish Professor of Experimental Physics, much, as he says, " to my great surprise and I think to that of everyone else. I remember hearing at the time that a well-known College tutor had expressed the opinion that things had come to a pretty pass in the University when mere boys were made Professors."

Thomson at once began to follow up earlier theoretical work based on the principles of electrolytic ionisation, by experiments on the gaseous electric discharge. Many published papers resulted, to be followed presently by books on the *Applications of Dynamics to Physics and Chemistry* (1888) and *Notes on Recent Researches in Electricity and Magnetism* (1893), which latter was intended as a sequel to Clerk Maxwell's *Electricity and Magnetism*. In the *Recent Researches* the treatment of the passage of electricity through gases is remarkably penetrating. This proved to be the forerunner of the still more famous *Conduction of Electricity through Gases*, which became a " bible " for the many workers on the subject.

Immediately following Röntgen's famous discovery in 1895, Professor Thomson was among the first to demonstrate that the new rays shared with cathode rays the property of imparting ionisation to a gas. He showed that an electrically charged conductor lost its charge upon exposure to X-rays, and further, he distinguished between the true ionisation in the gas and the photoelectric action due to the X-rays impinging on the conductor itself. In the controversy regarding the nature of X-rays he supported the now accepted theory of transverse waves with wavelengths of atomic magnitude.

The story of Thomson's classic work on the cathode rays is well known : it is dealt with here in a balanced and modest account which enchains one's interest. He published an early determination of their velocity in 1894, but it was not until 1897 that he finally evaluated the velocity and the ratio e/m, and so made the far-reaching discovery that the ratio was many hundred times larger than that for the hydrogen ion in electrolysis. The first announcement

of this was made in a Friday Evening Discourse at the Royal Institution on April 29, 1897.

A year later, by means of an ingenious use of Wilson's cloud chamber, he obtained the measurement of the gaseous ionic charge, which proved to be very nearly the same as that of the hydrogen ion. Townsend's direct proof of the identity of the two charges followed soon afterwards, and thus the nature of the cathode rays was explained. The electron was discovered, its mass and charge determined, and light was thrown on the constitution of the atom and the mechanism of ionisation. In later years Thomson applied methods of like ingenuity to the positive rays of Goldstein, and so elucidated one more major factor in the mechanism of the discharge tube.

There are very many other phases of these *Recollections* on which one would like to touch, but space forbids :—psychical research ; water dowsing ; resistance thermometry (first suggested to Callendar by Thomson) ; visits to the States, Canada (as President of the British Association), and Germany ; his work as President of the Royal Society and as a member of the Board of Invention and Research in the war ; meetings with a multitude of famous men and women, and much besides. On all these matters the Master writes with a genial and shrewd pen.

The whole book, in fact, reflects Sir J. J. Thomson's deep and many-sided interests in life and all it holds. One recalls that his enthusiasm for work was only matched by his keenness for sport ; it had to be something very momentous to prevent his attendance at an important Rugby football match at Cambridge, be the weather what it would. This interest was of long standing : he tells the story of the giving of " blues " to " rugger " in 1885 after fierce controversy, to the successful issue of which he gave material support !

The Master will find himself in good company in his enjoyment of the many excellent stories he has sprinkled among these pages— stories told with a delicate humour which the reader must be left to digest and appreciate for himself. We have no doubt that many will desire to do so, for the book is bound to find a wide public.

By a happy conjunction of events, the admission to Cambridge of " Research Students " who were graduates of other universities came into force within a few weeks of the discovery of the X-rays. The " new physics " came into being, and, as is well known, the Cavendish Laboratory became a great school of research which attracted workers from all over the world, among them Lord Rutherford, Professor C. T. R. Wilson, Professor Townsend and Professor

Langevin. The silver jubilee of his tenure of the Cavendish Chair of Physics was commemorated in 1910 by the publication of a *History of the Cavendish Laboratory*. In 1926, on the occasion of his seventieth birthday, some 230 of his past and present research students combined to pay " J. J " tribute and to bear testimony to the honour and inspiration it had been to have come within his sphere of influence.

And on this note we must end our appreciation of the *Recollections and Reflections* of a scientific genius of whom this country is justly proud—and whose unique achievements and distinctions have earned for him a secure niche among the immortals. We wish him many more years of activity and health. He himself modestly sums up his career in the preface : " I realise how fortunate I have been throughout my life. I have had good parents, good teachers, good colleagues, good pupils, good friends, great opportunities, good luck and good health."

MEASUREMENT AND IDENTIFICATION. By W. N. BOND, M.A., D.Sc., F.Inst.P., The University, Reading. Being a Review of **Relativity Theory of Protons and Electrons**, by Sir ARTHUR EDDINGTON, M.A., D.Sc., LL.D., F.R.S. [Pp. vi + 336.] (Cambridge : at the University Press, 1936. 21s. net.)

DURING the past eight years Sir Arthur Eddington has published a number of papers dealing with the application of relativity theory to fundamental problems of atomic structure. He has now given a connected account of these investigations ; and in so doing he has made an important contribution to the theory of physics.

Any theory that helps to explain and correlate the behaviour of atoms is to be welcomed at the present time. Remarkable progress has been made in atomic physics during the last forty years ; but there has been a tendency for experimental data to accumulate, awaiting theoretical interpretation. Presumably we strive to explain all the known properties of more than 90 elements in terms of the properties of a few entities such as protons and electrons ; and the problem is a vast one !

Another reason for welcoming the publication of Sir Arthur's book is that it is largely concerned with the theory of measurement. Now, it is generally realised that most of experimental physics consists of attempts at accurate measurement ; but it is not always easy to discover what has really been achieved by the measuring.

In common with other books on relativity and quantum theory, the book is mathematical and abstruse. But on reading it one is impressed by the masterliness of the treatment, and the intimate

relationship between the various parts of the theory. Moreover, Sir Arthur at times leads us gently, explaining his theories with apt analogies, in his inimitable way. It is unfortunate, however, that the order in which the subject is presented is not likely to appeal to experimental physicists. The earlier chapters are mainly devoted to developing the necessary mathematics ; and it is only on reading to the end of the book that the simplicity of the postulates and the epistemological nature of the argument are clearly realised.

A general account of the principles of the method of investigation was given in Chapter XI of *New Pathways in Science*. The aim of the book is to develop a method, rather than to deduce a single comprehensive formula. Though an effort appears to be made to reduce the primary postulates to a minimum, the cataloguing of these postulates is not the main objective.

Let us consider the theories in general outline. Our senses give us information about the world. The physicist makes measurements, and is concerned with *structure* and *substance*. The latter does not appear in his numerical results and formulæ. The former ultimately resolves itself into a complex of relations. Moreover, there is a similarity between some of these relations, so that proximate relations are comparable. Absolute position in space and time do not enter into our physical laws ; and hence relativity theory and tensor calculus are applicable.

Allowance has to be made for the fact that we cannot simultaneously discover the position and velocity of a " particle," because it is interfered with in examining it. Hence we are reduced to investigating the probability of its position and velocity lying within specified ranges ; and the " particle " is replaced by what may be termed curves or waves of probability. Sir Arthur develops in the first half of the book a wave-tensor calculus which he has invented. This calculus constitutes a combination of relativity and wave-mechanics.

The wave-tensor calculus is developed by means of four E symbols or operators, from which linear functions called E-numbers, are constructed. These E-numbers are a particular case $(n = 4)$ of " Clifford's numbers." It is shown that all E-numbers can be represented by fourfold matrices, and that every fourfold matrix will represent an E-number. The matrix representation is introduced in order that the notation shall be similar to that used by Dirac and others. Space vectors and strain vectors are next discussed. The former are used for representing free particles ; the latter are for representing particles considered collectively as forming parts of one system, there being only one time co-ordinate

for the whole system. Finally, there is a treatment of the double-wave tensors formed by multiplying two simple-wave tensors which specify the probability and configuration of two independent systems.

Towards the end of the book, the fact that we can only observe relations between two entities (the terminals of a " displacement ") is shown to correspond to the use of a fourfold matrix or equivalent E-number. Hence the choice of $n = 4$ (above) is justified; and it is deduced that the continuum (space-time) must be 4-dimensional. It is also shown that three of these dimensions are similar (space) but that the fourth is of a different character (time).

Before considering any more details, it will be well to describe what has been termed " identification." When a mathematical theory of physical measurement is developed, no symbol or equation in the theory will bear a distinctive label such as " electron " or " proton." It may, however, be found that some of the equations of the theory have properties and exhibit relationships which are exactly similar to those revealed by experimenting on electrons or protons. In other words, these equations and those representing the experimental results may be identified; but there is a slight risk that the identification may be made incorrectly.

The numerical results of physical measurements usually depend on the arbitrary units used in the measurements. To obtain information about the phenomena under investigation, uninfluenced by our arbitrary choice of units, it is necessary that the measurements shall yield one or more non-dimensional quantities. Now, it is found that the physical constants which are generally recognised as being fundamental (such as the electronic charge) yield four independent non-dimensional constants. Two of these are the ratio of the masses of proton and electron, m_p/m_e, and the fine-structure constant $hc/2\pi e^2$. Sir Arthur deduces that m_p/m_e is equal to the ratio of the roots of the equation $10x^2 - 136x + 1 = 0$, namely 1847·6; and that $hc/2\pi e^2$ is equal to 137.

It may be wondered why the particular numbers 10 and 136 are involved. They are the numbers of dimensions (or degrees of freedom) of phase space, and there is equipartition of energy between the different dimensions. In the case of a simple wave-tensor there is a set of 16 matrices, of which 10 are imaginary and 6 are real; for a double wave-tensor there are $(10 \times 10) + (6 \times 6) = 136$ real matrices (and 120 imaginary ones). For simple wave-tensors the eigen values are imaginary, and the 10 imaginary matrices are space-like; but for double wave-tensors the eigen values are real, and the 136 real matrices are space-like.

In his earliest paper on the subject (1928) Sir Arthur gave the relationship $hc/2\pi e^2 = 136$. This value was not in agreement with experimental results. The change to 137 was found to be necessary to allow for the 137th degree of freedom arising from the indistinguishability of the particles. Now, in 1929 R. T. Birge estimated the experimental value as $137 \cdot 29 \pm 0 \cdot 11$; K. Shiba (1933) gave the value $136 \cdot 94 \pm 0 \cdot 5$ per cent. and A. E. Ruark (1935) gave the value $137 \cdot 04$. Hence we may regard the equation $hc/2\pi e^2 = 137$ as being in close accord with experiment.

As regards the ratio m_p/m_e, the experimental value is about $1834 \cdot 3 \pm 0 \cdot 2$, whereas the value deduced above was $1847 \cdot 6$. The discrepancy is of a similar type to that which affected the value of $hc/2\pi e^2$. The *observational* value of m_p/m_e is given by the ratio of the roots of

$$10x^2 - 136x + \frac{137}{136} = 0,$$

namely $1834 \cdot 1$.

Sir Arthur thinks that the most satisfactory way of restoring order is to admit two constants h and h', to be used in connection with internal and external wave functions respectively; the two constants are to be such that $h' = \frac{136}{137}h$. He states that, in general, the uncorrected observational determinations of the natural constants should be consistent with $hc/2\pi e^2 = 137$ and $m_p/m_e = 1834 \cdot 1$; but that the diffraction of electrons by matter might possibly involve h'.

Finally, we have to consider the two remaining non-dimensional constants to which reference has been made. These are both deduced in terms of the number of protons, $N/2$, or the equal number of electrons, $N/2$, in the universe. It is explained that in measuring any relation we have to deal with four entities, the terminals of the observed relation and those of the relation with which it is compared. The result is that a quadruple wave system is involved. Such a wave system is shown to have a total of $2 \times 136 \times 2^{256}$ independent non-degenerate eigen functions. In practice the comparison relation becomes standardised, and is vaguely contained in the Riemannian geometry; and also one terminal of the object relation is made the origin. The remaining terminal appears in the guise of a proton or electron endowed with charge and mass. Hence it is deduced that $N = 2 \times 136 \times 2^{256}$.

Using this value for N, Sir Arthur's theories predict a value of the Universal gravitational constant of about $6 \cdot 659 \times 10^{-8}$ c.g.s. units, and a limiting speed of recession of the nebulæ of 432 km. per

sec. per megaparsec. The former is in close agreement with experiment, and the latter agrees satisfactorily with the best estimate that can so far be obtained.

There are many aspects of the book besides those already mentioned. In particular, Eddington's and Dirac's theories are compared and contrasted. Also there is some discussion of the " packing ratio " in helium, of the theory of the Stern-Gerlach effect, and of the discrepancy between the two methods of measuring the electronic charge. The treatment has not yet been extended to atomic nuclei and free neutrons, and the theory of radiation has only been commenced.

In conclusion we may notice that Sir Arthur's theories have been found to give valuable results, and show promise of fruitful extension. The theories are theories of measurement; in so far as they are free from internal errors and errors of identification, they are unassailable.

REVIEWS

A First Course in Differential Equations. By NORMAN MILLER. [Pp. viii + 146.] (London : Oxford University Press, 1935. 7s. 6d. net.)

To the reader with an elementary knowledge of calculus and whose interest in differential equations is not that of the pure mathematician this book should be of much benefit. It gives an account of the most commonly used methods for solving first order, linear, and simple partial differential equations : roughly the syllabus for the subject in the London B.Sc. general degree. For such students the book should be especially useful and popular. It is clearly and engagingly written, the text is illustrated by numerous worked examples, and there is a collection of more than 400 exercises. Two chapters are devoted to the *formulation* of equations ; these will be appreciated by the physicist, chemist, or biologist who is anxious to acquire facility in the preliminary steps of expressing his scientific problem in mathematical language. Such readers may well wish for a more detailed account of Fourier series than is given here. The geometrical interpretation of the theory is given good prominence, and though existence theorems have little place in this book, the author is generally careful to distinguish between the plausible and the obviously true, and to refer the reader to deeper sources where necessary. The reader may, however, feel some uneasiness in the sections where differentiation is used to assist in the solution of a differential equation ; these sections would be improved by more careful explanation, for although the methods lead to correct results, most teachers will be familiar with the doubts they (the methods) inspire in critical students even when they are not primarily pure mathematicians. There is an agreeable continuity of ideas in the methods used for first and for higher order equations, and this should give the reader a firm basis and a confidence to use elementary arguments in cases where operational methods are out of place. Finally, the book must be commended for the excellent way in which the printer has done his work.

H. K.

Differential Equations in Applied Chemistry. By F. L. HITCHCOCK, Ph.D., and C. S. ROBINSON, S.M. Second Edition. [Pp. viii + 120, with 4 figures and 1 chart.] (New York: John Wiley & Sons, Inc. ; London: Chapman & Hall, Ltd., 1936. 7s. 6d. net.)

THE principal aim of this book is to encourage the reader in the habit of seeing scientific phenomena as so many examples which may be set up in the language of differential equations. Though the material facts may vary in different branches of science, there are certain mathematical forms

which constantly recur, and, when once these forms are recognised, differences of terminology will no longer obscure the unity which exists among the different laws. The 200 or so examples (many of which are worked in full detail) are accordingly varied in character, though the majority are drawn from chemistry and chemical engineering. Many problems have been added since the first edition : *e.g.* data and problems arising from recent experiments on radon are included, and a reference to problems of flow in gas mask canisters adds the topical touch. The chapter on Fourier series has been made much more detailed, and a valuable chapter has been added on Picard's method of numerical solution. However, the chemist who desires a systematic study, even of the most elementary sort, of first and second order equations cannot be advised to look to this book for guidance. Practically no attempt is made to develop the reader's insight into the methods of solution of equations ; the ideas of an *exact form*, of an *integrating factor*, of a *solution in series* are altogether neglected ; linear equations with constant coefficients, even of the second order, are not discussed ; in a word, almost all that goes to make differential equations a characteristic study is neglected, and what remains is a weak offshoot of the calculus. Though the reader will find plenty of material to exercise his skill in setting up equations, he will hardly be reassured by the text that the solution may be carried out just as easily and as naturally ; after the explanation given on p. 58 of how to solve a linear equation of the first order the reader may well continue to regard mathematicians with respect, but it may be the fault of the authors if that feeling is accompanied by a loss of mathematical self-confidence on the part of the reader.

H. K.

Factor Table giving the complete decomposition of all numbers less than 100,000. Prepared independently by J. PETERS, A. LODGE and E. J. TERNOUTH, and E. GIFFORD. British Association Mathematical Tables, Vol. V. [Pp. xvi + 291.] (Cambridge : at the University Press, 1935. 20s. net.)

THE fundamental theorem of arithmetic is that every whole number can be decomposed into prime factors in one and only one way. This is not difficult to prove, but the problem of finding the prime factors of a given number can only be solved by tentative procedures, which for numbers greater than 10,000,000 are laborious. Mathematicians will therefore welcome this book, which fills a gap in the previously existing factor tables. These generally exclude numbers divisible by 2, 3, or 5, thus saving much space ; the dominant object of the authors of these tables was to extend the list of prime numbers. But workers in the Theory of Numbers often need to decompose numbers which are divisible by 2, 3, or 5, and the present table gives the required decomposition at sight of every number up to 100,000. Any number greater than this, but less than 10,000,000, can best be factorised by first dividing out any powers of 2, 3, 5, or 7 that it may contain, and then looking for the quotient (if greater than 100,000) in D. N. Lehmer's *Factor Table* for the first ten millions. The present book contains at the end a table of the reciprocals of all primes from 11 to 9973, which may be used with a calculating machine to search for factors of numbers lying between ten and one hundred million.

The Introduction contains an account of the methods by which the

three independent manuscripts were calculated, and the precautions taken to secure accuracy. It is followed by a Bibliography of the more important previous Factor Tables. The *Factor Table* is a fine example of clear and good printing. Every mathematical library should possess a copy of this book.

A. E. WESTERN.

Dynamics of Rigid Bodies. By W. D. MACMILLAN, A.M., Ph.D., Sc.D. [Pp. xiii + 478, with 82 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1936. 36s. net.)

THE dynamics of rigid bodies may be considered from two main points of view. On the one hand it can be regarded as a development of kinematical geometry in three dimensions, based on intuition but developed by analytical methods leading to differential equations. On the other hand, following the methods of Lagrange and Hamilton, an entirely analytic path may be followed, minimising the appeal to intuition and in fact transforming any dynamical problem into a problem in the calculus of variations. In the book under review, which is the third and last volume of Professor MacMillan's work on Theoretical Mechanics, roughly equal weight is given to the two aspects ; in the former, vector symbolism and methods form the medium by which intuition is translated into formulæ. Throughout the book due emphasis is laid on the interpretation of the equations to which the analysis leads, and it is the thoroughness of this interpretation, coupled with the full working out of numerous illustrative examples, that are most evident among its distinguishing features.

Preliminary ground-work on vector algebra and on moments of inertia occupy the first two chapters. The third deals with the equations of motion of systems of free particles and the ten classical integrals of Clairaut, with applications to the permanent configuration problems under Keplerian motion ; the fourth to the general principles underlying the motion of a rigid body. Then follow chapters on motion parallel to a fixed plane, with one degree and with two degrees of freedom ; on motion in space about a fixed axis or about a fixed point ; a well-illustrated chapter on the three classical integrable cases of motion about a fixed point, and chapters on rolling motion and impulsive forces.

Chapter X introduces the second or purely analytical point of view based on the differential equations of Lagrange for holonomic systems ; the necessary modifications for non-holonomic systems are exemplified by the rolling sphere. The canonical equations of Hamilton occupy the next chapter, which is perhaps too brief to leave the reader convinced of the real value of the method. The concluding chapter (XII) on the method of periodic solutions is a very satisfying account of what is actually a branch of the theory of differential equations rather than of dynamics ; in particular it includes an up-to-date treatment of linear differential equations with constant coefficients by matrix methods.

Except for an occasional typographical error, one or two incorrect references, and a misleading use of the word *tensor* for the length of a vector, the book is pleasant to read and deserves every commendation. The diagrams are superior to those that often pretend to illustrate works on rigid dynamics, and the problems (nearly 200 altogether) have a useful look. The price may be above the means of the average student, but the volume should be on the library bookshelf.

E. L. I.

Magic Squares of $(2n + 1)^2$ Cells. By M.-J. VAN DRIEL. [Pp. 90.] (London: Rider & Co., 1936. 10s. 6d. net.)

A MAGIC square of order p contains the numbers 1, 2, . . . p^2 arranged in rows and columns so that the sum of the numbers in any row, in any column and in each of the two diagonals is the same in each case. The subject is an old one and is supposed to have had its origin in India. It was introduced into Europe in the fifteenth century by Moschopulos, and in medieval times magic squares were worn as charms against disaster and disease. In the seventeenth and eighteenth centuries their construction occupied the attention of many eminent mathematicians including Fermat and Euler.

The present volume gives many alternative methods of constructing magic squares of odd order (those of even order require different treatment); some of these methods date from the Middle Ages, others from the French School and many are due to the author himself. The most interesting magic squares are pandiagonal (or " diabolic " !); these have the property that the numbers in each *broken* diagonal also have the same sum, and several examples of these are given. The notation (due to Euler) is at first rather confusing and some of the author's explanations are curiously expressed. There are chapters on symmetrical magic squares, bordered magic squares and constructions by superposition; footnotes give a large number of references to original memoirs. Mr. van Driel has been successful in giving an exhaustive summary of the available processes and in classifying the various solutions. The Mayflower Press deserve credit for the clear type and the good paper.

<div align="right">P. W. W.</div>

ASTRONOMY AND METEOROLOGY.

Theoretical Astrophysics. By S. ROSSELAND. [Pp. xx + 355, with 44 figures.] The International Series of Monographs on Physics. (Oxford: at the Clarendon Press; London: Humphrey Milford, 1936. 25s. net.)

PROF. ROSSELAND, Director of the Oslo Observatory and Astrophysical Institute, is well known for his contributions to theoretical physics and astrophysics, and his volume *Astrophysik auf Atomtheoretischer Grundlage* in the series *Struktur der Materie*. In the present volume he has developed those basic theorems in dynamics and atomic theory which are required in the interpretation of astrophysical phenomena, and given a full account of their application to the analysis of stellar atmospheres and other stellar (including inter-stellar) envelopes. A second volume is planned to deal with the internal structures of stars, analysed in terms of nuclear structure and hydrodynamics.

The first eight chapters are devoted to an exposition of classical and atomic dynamics and statistical mechanics. Outstanding later chapters are concerned with the transfer of radiation in a star, the intensities and profiles of absorption lines, stellar opacity, the effect of rotation on line-intensities, molecular spectra in stars, the abundances of elements in stellar atmospheres and spectroscopic phenomena in nebulæ. Professor Rosseland shows remarkable skill in extracting the gist of isolated individual investigations and giving a general view of their relationships. He has a great gift in assimilating what was often obscure in the original memoirs and presenting a connected logical sequence.

The book assumes on the part of the reader an almost complete knowledge of astrophysical facts. For example, there is no description of the notation of the Harvard spectral sequence—the reader is supposed to be fully familiar with what is meant by an O-star or an M-star, a giant and a dwarf, or the main characteristics of the solar and chromospheric spectra. The text is largely mathematical in character, but scarcely such as to appeal to the mathematician *qua* mathematician, for the details of manipulation are often so loosely sketched that the reader must in effect sit down and struggle with the situations for himself. For example, so fundamental a theoretical result as Saha's formula for dissociative equilibrium amongst atoms, electrons and ions is not established at one blow, but is split up into an analysis following Gibbs and a rather vague later reference to the quantum theory, where such un-incisive phrases occur as " that this is the correct procedure is evident from the quantisation rules " and " in view of the requirements of the quantum theory, it is convenient to . . ." What the reader wishes to have, however, is a categorical statement of what is *assumed*.

A friend and admirer of Prof. Rosseland may be pardoned for regretting that the author appears to regard theoretical astrophysics as a mere appendage to atomic physics, rather than as a subject of study possessing its own characteristic methods and theorems. The author scarcely conveys the peculiar fascination of astrophysics as a mode of extracting from observations of bodies in the large some understanding of what is really going on. His exposition makes one feel that it is all inevitable, as against the perpetual element of surprise which actually accompanied the achieving of the present position. Romantic and moving as is Prof. Rosseland's preface, the romance of it all scarcely emerges from his treatment. If the facts are assumed, and the mathematical calculus is assumed and the explanations are represented as inevitable, what are the author and reader to consider themselves as really interested in ?

There comments must not be allowed as a criticism of the author's learning and the magnitude of the task which he has set for himself and accomplished. The book is a splendid text-book and work of reference for initiates, and its up-to-dateness ensures for it a sustained future.

<div align="right">E. A. M.</div>

Manual of Meteorology, Vol. II : Comparative Meteorology.
By SIR NAPIER SHAW, LL.D., Sc.D., F.R.S., with the assistance of ELAINE AUSTIN, M.A. Second Edition. [Pp. xlviii + 472, with 225 figures.] (Cambridge : at the University Press, 1936. 36s. net.)

THE second volume of Sir Napier Shaw's monumental work has now appeared in a second edition, eight years after its first publication. The scope of Volume II is best indicated by quoting the opening words of the author's original preface : "From the study of Meteorology in History, as set out in the introductory volume of this Manual, the conclusion was arrived at that the primary need of the science was a sufficient knowledge of the facts about the atmosphere in its length and breadth and thickness to furnish a satisfactory representation of the general circulation and its changes." The volume is thus devoted mainly to facts, and perhaps its most notable feature is the unique collection of maps of both hemispheres, giving means for every month of the various meteorological elements. Their attractiveness has been increased in the second edition by the colouration of the sea, with different tints for each element mapped.

The work is brought up to date by the inclusion of the additional material which has become available in the last eight years. This has been done without an excessive increase in the length of the book, by means of various adjustments and some exclusions. Perhaps the most important additions are the new and more complete upper air data (pp. 102–9) and a miscellaneous collection of notes, covering a very wide range, in Chapter X. Other additions include maps of monthly pressure changes over the Northern hemisphere (Fig. 185, p. 296), of monthly pressures and rainfalls over the British Isles (Figs. 184, 187) and of the daily range of temperatures for each month, in " step " diagrams, over the globe (Figs. 43, 44, pp. 84–5).

In order to make room for the new facts given in Chapter X, a collection of theoretical propositions has been cut out. The change represents an improvement in a book devoted to the factual side of the science. Meteorological theory is still in a rather fluid state, at least in respect of some of the main problems, and a book devoted only to the facts—though inspired throughout by a profound scientific knowledge—has a certain completeness of its own, which can outlast changes in theoretical ideas.

C. K. M. D.

Climatic Maps of North America. By CHARLES F. BROOKS, A. J. CONNOR and others. [26 maps, with Foreword.] (Cambridge, Mass.: Harvard University Press; London: Humphrey Milford, 1936. 12s. 6d. net.)

THIS series of 26 climatic maps of North America, published in large folio form by the Blue Hill Observatory of Harvard University, was prepared for the Köppen-Geiger *Handbuch der Klimatologie*. The actual discussion of the maps is to be found in the *Handbuch* itself but at the foot of each of the maps, the more obvious deductions are noted and a Foreword indicates the sources, adequacy and degree of reliability of the data used. The size of the base map used permits of the showing of the full detail in which the maps were prepared and location is facilitated by the inclusion of sufficient physical and political detail to render unnecessary the use of colour or shading. It is possible that the printing of traces would have proved useful for the purpose of extraction and synthesis of information from the maps.

The maps represent the climatic elements of temperature (sea-level and actual isothermals), pressure, rainfall, snowfall, humidity, cloudiness and thunderstorms. New material has been used in the compilation of the following maps which have never been attempted before for the whole of North America : mean annual maximum and minimum temperatures, snowfall, humidity and thunderstorms. The bi-monthly maps of the major elements of temperature and rainfall succeed in presenting conditions in a dynamic aspect and throughout, in spite of some inevitable crowding of lines owing to scale limitations, the maps are in sufficient detail to show not only broad effects such as those due to the rhythm of seasonal changes but also in a very striking degree the more obscure effects due to many local phenomena, *e.g.* the Chinook winds.

This series of maps presents in a highly convenient form for the first time a comprehensive view of the climate of the North America region based on adequate data, hence *a priori* should commend itself strongly to all students of climatology and geography.

G. A. G.

PHYSICS

An Elementary Survey of Modern Physics. By GORDON FERRIE HULL. [Pp. xxiv + 457, with frontispiece and 231 figures.] (New York and London: Macmillan & Co., Ltd., 1936. 20s. net.)

IT will be remembered that one of the first measurements of the pressure exerted by a beam of light was made by E. F. Nichols and G. F. Hull. When it is added that Dr. Hull at one time carried out research at the Cavendish Laboratory, it appears likely that his outlook on physics will be experimental. And we find that he commences his Introduction with the quotation " The essence of law is not logic, the essence of law is experience."

The book gives an elementary survey of the experimental advances that have been made in atomic physics and the physics of radiation since the discovery of X-rays in 1895. Very little Mathematics is given in the text itself; but a number of results are deduced by simple methods in the appendix (which extends to more than 40 pages). The book is written in a popular, crisp style, and is illustrated by line diagrams and good photographs.

There are great difficulties in giving, in one volume, even a brief outline of all the important advances that have been made in physics during the last forty years. By confining attention, in the main, to atomic and radiation problems, and by treating relativity and wave mechanics only very briefly, the author has narrowed the field. Nevertheless, a large number of subjects are discussed, including the following: electrons, isotopes; thermal radiation, X-rays, atomic and molecular spectra and the Raman effect; electron tubes; natural and artificial radioactivity; cosmic rays; waves and particles; the Uncertainty Principle.

The book is intended to furnish material for a second-year course in physics. But unless the student had made very considerable progress during his first year, he would probably be bewildered by the book. On the other hand, a fairly advanced student would wish to read further; but he is not provided with many references to books and original scientific papers (nor does the book include any questions of the examination type for solution). Unusual features, in such a book, are a chronological list of the main discoveries in atomic physics since 1895; a list of recipients of the Nobel Prize for physics; an extensive, though cursory, account of various uses of thermionic tubes; and a description of modern high potential electrostatic generators.

W. N. BOND.

Heat for Advanced Students. By the late EDWIN EDSER, A.R.C.Sc. Revised edition by N. M. BLIGH, A.R.C.Sc. [Pp. x + 487, with 207 figures.] (London: Macmillan & Co., Ltd., 1936. 6s.)

THE present volume is a revised edition of the work first published in 1899 and which has been reprinted some 19 times at regular intervals since that date. The reviewer has been interested to compare the new book with his own copy of the 1901 reprint.

It appears that the changes and additions are few and relatively unimportant. Some weight is attached to the fact that the methods of the calculus are used when possible. In the opinion of the reviewer the gain is only slight, since the scope of the work was not such as to require any

specialised mathematical knowledge. In fact, one of the strong features of Edser's books was his skill in presenting the Physics of the subject and in avoiding equations and functions whose significance would not be appreciated by the type of student for whom he was writing.

Amongst the changes noted are the more general treatment of the specific heat of a liquid by the method of cooling and the substitution of Callendar's method for measuring J for the shot in tube experiment. The Maxwell thermodynamic relations are derived and used in a short treatment of the theory of the porous plug experiment, but the reviewer feels that the students will not have sufficient acquaintance with the properties of entropy and internal energy to follow the mathematical processes. Slight mention is made of the quantum theory in the concluding chapter. The book is essentially the original work, well balanced and clear in expression, and there is every reason to believe that it will continue (like the Gilbert and Sullivan operas) to be as popular with the present generation as with their predecessors.

SYDNEY MARSH.

The Quantum Theory of Radiation. By W. HEITLER. The International Series of Monographs on Physics. [Pp. xii + 252, with 26 figures.] (Oxford : at the Clarendon Press ; London : Humphrey Milford, 1936. 17s. 6d. net.)

THE first thought which may arise on seeing a work entitled *The Quantum Theory of Radiation* is in the form of a question. Has the theory progressed so far that a book may now be written upon the subject and indeed a book of the dimensions of the volume now under review ?

This question may well be answered in the negative before more than the title has been read, but it can be answered decidedly in the affirmative after studying the author's presentation of the subject and the applications which he describes. It is true that the theory is still in its initial stages, that there are many difficulties to its present form and that a future edition of the work is likely to need radical alterations, but a great service has been rendered to physicists by its appearance now. For it is here that the deficiencies of the modern form of the quantum theory come most clearly into evidence, and it is from a study of them that further progress is likely to be made. The reader is immediately struck by the clarity of the summary of the classical theory with which the book begins, a clarity which continues throughout the fundamental work of the early chapters. These contain an introduction to the new theory of radiation which is presented as an analogy with the older quantum theory, the author making the most of physical ideas which these two theories have in common.

This part of the book is chiefly concerned with the representation of the electromagnetic field as a system of oscillators and with the theory of charged particles in this field. The difficulties of the theory and the importance of the ratio of the lengths h/mc and e^2/mc^2 are called to mind while an account of the uncertainty relations applying to field strengths is a welcome insertion in a text-book.

After the earlier chapters dealing with general principles, many special problems are discussed, amongst them the photoelectric effect, scattering, including the Klein Nishina formula, the theory of the positive electron, and in the closing chapter there is a brief mention of the theory of Born and Infeld.

The collection of so much original work and its arrangement in an interesting and readable sequence is a success on which the author is to be congratulated.

One is reminded of another work which appeared at a stage in the history of physics that has some resemblance with that through which we are now passing. The volume before us is worthy of a place on our bookshelves close to Planck's *Heat Radiation*.

<div align="right">H. T. F.</div>

The Earth's Magnetism. By S. CHAPMAN, M.A., D.Sc., F.R.S. Methuen's Monographs on Physical Subjects. [Pp. xii + 116, with 35 figures.] (London : Methuen & Co., Ltd., 1936. 3s. 6d. net.)

TERRESTRIAL magnetism is a subject which has for a long time been closely associated with other branches of scientific and technical work, and in recent years fresh interest has been aroused by its bearing on such problems as long-distance radio transmission and the origin of cosmic rays. The publication of this brief survey of the present state of knowledge of the subject is, therefore, very welcome, especially since its author is one of the leading authorities. The first chapter of the book deals with the properties of the earth's main field and its secular variation ; the large number of symbols introduced in this chapter may prove a strain on the average reader's memory, but fortunately a list of these symbols is given at the beginning of the book. In the next four chapters the author discusses what he calls the " transient " magnetic variations. These include the solar and lunar daily variations and the irregular disturbances due to magnetic storms ; present-day theories put forward to account for these variations are considered. In the last chapter the relationship between magnetic disturbance and sunspot activity is discussed.

It is remarkable that Prof. Chapman should have been able to compress such a large amount of information into a monograph of this size. The book is lucidly written and well illustrated, but the entire absence of reference to some of the more important original papers will be disappointing to many readers. In all other respects the book is very good.

<div align="right">F. J. S.</div>

Simplified Structure Factor and Electron Density Formulæ for the 230 Space Groups of Mathematical Crystallography. By KATHLEEN LONSDALE, D.Sc. [Pp. viii + 181.] (London : Published for the Royal Institution by G. Bell & Sons, Ltd., 1936. 10s. net.)

THERE are two principal ways of doing a thing : either to do it oneself or to get someone else to do it. Which of the two is chosen may be a matter of energetics, advisability, or plain necessity. In this case it is a question of all three.

It is also a question whether anyone but Mrs. Lonsdale could have carried through such a stupendous task so well. In common with other crystallographers the reviewer stands admiration-struck. Was there ever such a ten shillings' worth ?

All structure analysts at some time or other have juggled out trigonometrically some or other of the structure factors of the 230 space-groups—and promptly mislaid the result probably—but here we have all of them,

not simply in their most convenient general forms, as Mrs. Lonsdale had already prepared them for the *Internationale Tabellen zur Bestimmung von Kristallstrukturen* (Bell & Sons), but also in the contracted forms into which they fall for various special values of the indices *hkl*. With these formulæ are given the inter-relations of the structure amplitude F(*hkl*) and of the phase-angle α(*hkl*) as the different indices change sign, and also general condensed expressions for the electron density at any point in the unit cell. The origins of co-ordinates are throughout the same as those adopted for the *Internationale Tabellen*, but in 24 of the space-groups, where symmetry-centres might have been chosen but were not chosen for geometrical reasons, the structure factors, etc., have been worked out again with these alternative origins and added in a Supplement. There is of course, too, a general introduction explaining how to use the tables, with suitable advice about possible traps for the unwary.

Calculating structure factors of crystals is a tiresome business—not difficult, but tiresome ; but at least Mrs. Lonsdale has reduced it to its simplest mathematical terms for us, and others are continually endeavouring to reduce the numerical labour still further, the ultimate aim being a purely graphical system, probably.

It is a fair question now as to how it has been possible for the publishers to reproduce such a mass of trigonometrical data so neatly and yet so cheaply. The answer is that Mrs. Lonsdale has written it all out so beautifully that it was only necessary to photograph the pages. Printer's errors, at least, are therefore non-existent in these tables.

W. T. A.

Laboratory Manual in Physics. By A. A. KNOWLTON, Ph.D., and M. O'DAY, Ph.D. Second Edition. [Pp. xi + 137, with 67 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1935. 7s. 6d. net.)

THIS book is to all intents and purposes a reissue, with a few minor additions, of the first edition of 1930. It supplements on the practical side the authors' second edition of *Physics for College Students*, and to derive the full benefit it is necessary, at least for a beginner, to read the corresponding sections on the theory before attempting to carry out the practical work.

Its scope in the main corresponds with a first-year (Intermediate) course, but the limitations of size make the selection of experiments a difficult matter.

Some 44 pages are given to Mechanics, 23 to Heat, 6 to Sound, 12 to Light and 42 to Magnetism and Electricity.

Apart from a qualitative examination of spectra, there are no experiments on refraction and refractive index, and none on concave lenses ; convex lenses and concave mirrors are dealt with in rather summary fashion. The treatment of the sonometer is, in the main, of a qualitative character only.

The experiments on Mechanics by contrast are set out in considerable detail and have definite instructional value. (It should be noted that the apparatus used is often of a slightly different pattern from that made by British firms.)

Expansion of liquids and thermal conductivity are omitted from the Heat section, and in Magnetism there is no mention of plotting fields with a compass needle and location of neutral points. The exercises in Current Electricity are well distributed and deal with most of the fundamental points.

The book is very well produced, the diagrams being good and the descriptions and hints clear and to the point. As an adjunct to its theoretical partner it should serve a useful purpose, though it will be necessary to supplement the instruction in some of the sections.

<div align="right">SYDNEY MARSH.</div>

Mercury Arcs. By F. J. TEAGO, D.Sc., M.I.E.E., and J. F. GILL, M.Sc., A.M.I.Mech.E., A.M.I.E.E. Methuen's Monographs on Physical Subjects. [Pp. vii + 104, with frontispiece and 49 figures.] (London: Methuen & Co., Ltd., 1936. 3s. net.)

THIS book deals, not with mercury arcs in general, but with the particular practical application of the mercury arc as a steel-tank rectifier or inverter, and with the associated circuits.

In a preface, the authors point out the desirability of dealing with the essentials of the subject rather than with details. The first two chapters, though open to criticism in certain details, meet this requirement, and give the reader a general idea of the properties of the arc, the construction of a rectifier of this type, and the nature of its auxiliary apparatus.

The control of the rectifier by means of grids, its use for inversion and regeneration, the analysis of the output voltage wave-form and certain other calculations relating to rectifier circuits are adequately dealt with in later chapters.

But in applying calculations to a rectifier circuit the first essential is to determine the behaviour of the circuit and the assumptions on which calculations may be based. This is often difficult, involving principles that are not encountered in other branches of electrical engineering. The authors, however, in some of their calculations omit this essential step and in some instances are at fault in their own assumptions.

Such errors are not uncommon, and it is therefore unfortunate that the bibliography is short; it does not include, for example, *Mercury Arc Power Rectifiers* by Marti and Winograd, although this is regarded by many as the standard book of reference on power rectifier circuits.

In brief, many of the calculations are of interest to the experienced reader; the student who is not already familiar with the subject will find the descriptive matter valuable, but may be obliged to consult other authorities for explanation and correction of some of the circuit analysis.

<div align="right">D. H. DONALDSON.</div>

Cathode Ray Oscillography. By J. T. MacGREGOR-MORRIS, M.I.E.E., and J. A. HENLEY, M.Sc. (Eng.). Vol. II of a Series of Monographs on Electrical Engineering under the Editorship of H. P. YOUNG. [Pp. xiii + 249, with frontispiece and 151 figures, including 20 plates.] (London: Chapman & Hall, Ltd., 1936. 21s. net.)

THIS book will be welcomed by the increasing number of workers in pure and applied science using cathode ray oscillographs. The work of the senior author in this field has been known for many years, and he is well qualified to collaborate in the production of the first general book on cathode ray oscillographs to be published in England. After brief introductions to electron theory and electron optics, the space is almost equally divided between high-voltage cold-cathode oscillographs and the low-voltage sealed-off type. A valuable feature, for those seeking the best instrument for a

particular purpose, is the inclusion of descriptions and numerical data for representative commercial oscillographs, American and Continental as well as British. In view of the present rapid progress in design, it is hoped that the authors will be able to undertake periodical revision.

Chapters are devoted to auxiliary apparatus, operation and performance, time bases, and the principles underlying some applications of both types of oscillograph. There is a brief outline of the use of cathode ray tubes in television, and a bibliography of 102 references. The book is well produced and excellently illustrated, but the index is inadequate and there are a number of misprints, particularly in mathematical equations and references to figures.

In one or two respects the book is disappointing. The mathematical argument in Chapters 2 and 3 is confused in places, especially in connection with units. In the chapter on auxiliary apparatus and applications many difficulties met with in practice are passed over, while some of the diagrams as positively misleading. For example : (1) There is hardly a mention of the use of symmetrical (" push-pull ") input to deflector plates, desirable with gas-focussed oscillographs and essential with electron-optically focussed tubes. (2) The high resistances necessary to provide a conducting path from deflector plates to earth are omitted from some circuits—e.g. Figs. 93 and 123. (3) The diagram (Fig. 99) of the " Marx Impulse Generator " has the spark gaps in the wrong place. (4) Although the circuit for simple battery supply of a gas-focussed tube is included, there is not one for the more complex case of a mains operated electrostatically focussed tube. (5) The errors involved in using the circuit of Fig. 127 for hysteresis measurement are not stated.

However, this book does supply a need, and if revised is likely to become the standard work for Electrical Engineering students and for those desiring a survey of the whole field of Cathode Ray Oscillography as a preliminary to a detailed study of some particular problem.

F. A. V.

Television Reception : Construction and Operation of a Cathode Ray Tube Receiver for the Reception of Ultra-Short Wave Television Broadcasting. By MANFRED VON ARDENNE. Translated by O. S. PUCKLE, A.M.I.E.E. [Pp. xvi + 121, with 96 figures including 43 plates.] (London : Chapman & Hall, Ltd., 1936. 10s. 6d. net.)

ALTHOUGH we may perhaps claim that Great Britain leads the world in television transmission, yet we lack authoritative publications on the subject by experts who have been working in the field. For this reason the English translation of Baron von Ardenne's book is very welcome. His work in connection with the development of cathode ray tubes and their application to television problems has received world-wide attention, but, unlike many experts, he has also the gift of lucid description. The present work is no exception and has the additional advantage that the translator is also an expert in television development.

The first chapter is taken up with a discussion of the technical problems involved and the translator has taken the opportunity of introducing at this stage some important data for English readers on the nature of the signals radiated by the B.B.C. transmitters. Then follow chapters describing

the various units which make up a complete receiver, namely : television tube and its operation ; mains supply apparatus ; time bases ; amplitude filter ; detector ; ultra short wave picture receiver ; and the sound receiver. The final chapter discusses the results obtained, and a useful bibliography and index are included. The description of the requirements and of the manner of working of the various stages is clear and practically devoid of mathematics, making this book an excellent introduction for those wishing to obtain an understanding of the principles of television reception. At the same time the amount of data given on circuits and equipment whose efficiency has been proved makes the work an excellent starting point for any who intend to construct their own receivers. Many excellent illustrations are included, not the least valuable of which are those showing the various ills to which a television receiver becomes heir unless preventive measures are taken.

The opportunity has been taken in the English edition to introduce a certain amount of new material and to modify slightly some sections of the original. What little there is which one might wish to criticise is of minor importance, and altogether the book is one which can be thoroughly recommended.

D. H. B.

A Universal Stress Sag Chart for Power Line Computations. By J. T. HATTINGH, B.A., D.Sc. [Pp. xii + 74, with 16 figures.] (London and Glasgow : Blackie & Son, Ltd., 1936. 12s. 6d. net.)

THIS book should be most useful, in fact almost indispensable to anyone concerned with the design or erection of overhead transmission lines. With the lowest temperature and the highest wind and ice loading to be expected in service the tension in the conductors must not exceed a safe figure which is usually half the breaking load. The problem in the field is to determine the corresponding sag and tension for the temperature and wind at the time of stringing. These "stringing conditions" may vary from hour to hour and from span to span. The calculation is laborious, the method most commonly employed being to set up a cubic equation in which the stringing tension is the unknown. This equation can be solved on an ordinary slide rule by trial and error. Other even more elaborate calculations are necessary in the office for determining tower heights and broken conductor conditions, particularly as spans are usually of unequal length and sags vary accordingly when suspension type insulators are used.

All such calculations are greatly facilitated by the chart accompanying the book under review. The chart is truly universal being applicable to any conductor material. Before it is used several preliminary calculations are needed, and more labour would be saved if auxiliary charts or nomograms had been included for these. The scale of the chart could, with advantage to the user's eyesight, have been larger, and colouring of the spaces between lines corresponding to spans separated by a hundred feet would reduce the risk of errors. The user would be well advised to do such colouring himself. These are merely suggestions for the improvement of an already excellent piece of work.

Apart from the chart itself, the book is worth having for the sake of the thorough, clear and concise statement of the whole theory of stress-sag

calculations. The general formulæ for the elastic catenary with supports at different levels are derived. From these the non-elastic catenary as found in most textbooks is arrived at merely by putting Young's modulus equal to infinity. For the use of the chart the catenary is replaced by the usual parabola. The various errors due to the approximations adopted are fully investigated and evaluated. Thus the theory embodied in the text ensures that the chart will be used properly and with a proper understanding both of its potentialities and limitations.

REGINALD O. KAPP.

Dielectric Phenomena in High Voltage Cables. By D. M. ROBINSON, Ph.D., M.S., B.Sc., A.M.I.E.E., Assoc. A.I.E.E. Vol. III of a Series of Monographs on Electrical Engineering under the Editorship of H. P. YOUNG. [Pp. xii + 173, with 96 figures, including 20 plates.] (London : Chapman & Hall, Ltd., 1936. 15s. net.)

THIS book, in the main, is an account of the extensive research into the causes and mechanism of breakdown of high voltage cables upon which the author has been engaged for a number of years. The subject is therefore one of particular interest to electrical engineers concerned with the manufacture or maintenance of cables, but it is also deserving of study by all engaged on research into the causes of breakdown of insulating materials. It must be pointed out, however, that the studies described are strictly confined to phenomena found in the dielectric of the ordinary so-called straight type of high voltage cable, that is, paper impregnated with mineral oil with or without the addition of rosin.

Of great importance is the description of the "Magenta" wax test by which the difficulty of diagnosing breakdowns, and following the course of the mechanism from the start to finish has been overcome.

The various types of cable failures and their post-mortem investigations are described in a most simple and precise manner, so that with the information given at his disposal, a cable engineer should have no difficulty in finding the causes of failure of any particular cable in service. The author also discusses various difficulties encountered in the manufacture of oil-impregnated paper cables and the limitation, standing in the way of their improvement, imposed by the component materials.

The laboratory experiments on models designed to simulate the conditions in the cable will be found specially interesting by all insulation engineers and physicists engaged in the study of electric breakdown. There are also some data given with regard to the economic aspects of dielectric loss and reliability.

A discussion of the possible improvements in, and the merits of, different types of cables leads the author to predict that the straight type cable will be sufficiently improved and modified in some or all of the ways indicated during the next decade to enable its successful application to the transmission of the very highest voltages.

The book is fairly free from errors but for use in a second edition the following points might be rectified. It is stated on p. 67 that the appearance of Fig. 43b is due to the flow of electrons inwards, whereas it is obviously due to the flow of positive ions outwards from the point. The vector diagram, Fig. 71, given on p. 113, is hardly as self-explanatory as the author suggests and a definition of the various components might well have been given.

The printing, method of presentation and, particularly, the photomicrographs are of the highest standard. The author's scientifically detached but simple style makes pleasant reading and the book is entirely non-mathematical.

<div align="right">A. M. T.</div>

A Manual of Photo–Elasticity for Engineers. By L. N. G. Filon, M.A., D.Sc., F.R.S. [Pp. xii + 140, with 27 figures.] (Cambridge : at the University Press, 1936. 5s. net.)

ACCORDING to the preface, this little book is intended " to give, to a practical investigator who wishes to use photo-elastic methods in order to explore stress-distributions occurring in any problem in which he is interested, a brief account sufficiently complete and explicit to enable him to set up his apparatus and to use it in the best possible manner."

The value of the methods of photo-elasticity for instructional purposes is, perhaps, not sufficiently appreciated in this country, and, for this reason alone, a cheap book giving a clear exposition of the theoretical basis of the method is particularly welcome. Prof. Filon's book certainly fulfils that function ; all the necessary theory is there, whilst there is nothing superfluous for a proper understanding of the optical and elastic principles involved. But it seems questionable whether, with only this book to guide him, a research worker or teacher, using the method for the first time, and with no previous knowledge of the apparatus, would be able " to set up his apparatus and to use it in the best possible manner." The gulf between this little book and the large book by Profs. Filon and Coker seems too great ; we should have liked to see more precise laboratory instructions, the methods of drawing the isoclinics and lines of principal stress, and the calculation of the stresses worked out from the start for at least one example. If one or two illustrations of the practical value of the method had been included, the book would have had more propaganda value, it would have done more to encourage the hesitant chief designer or professor of mechanics to instal the apparatus ; such encouragement is necessary, for, at least in the opinion of the reviewer, a far more widespread use of the method of photo-elasticity is highly desirable, particularly as a medium of instruction.

<div align="right">J. C.</div>

Foundations for the Study of Engineering. By G. E. Hall, B.Sc. [Pp. vii + 259, with 153 figures.] (London : The Technical Press, Ltd., 1936. 7s. 6d. net.)

THE unorthodox title of this book inspires some doubt as to its contents. It proves, however, to be an elementary treatise on those subjects which are usually included in Engineering Science. The Author's preface does not state the intended scope of the work, but it appears to reach a standard somewhere between the second and third years of a senior course in most technical colleges. This is a high standard to reach in the space available and consequently explanation is kept at a minimum, so much so that the book will be of little value except to students who have considerable help from a teacher, or have covered much of the ground before.

Although intended for engineers, the argument in the mechanics portion of the book is developed by means of absolute units, and gravitational or engineers' units are introduced at various points. There is a danger that the young student's mind will be left confused by this intermingling of the two systems at such an early stage.

A commendable feature is the introduction of some simple ideas in theory of machines, thus getting away from the impression that theory of structures is the only natural outcome of mechanics.

The section on heat and heat engines is interesting rather than systematic, but becomes readable if some knowledge of heat is presupposed.

The diagrams are mostly reproductions of freehand or rough sketches and generally attain their object, but a few are rendered almost valueless, notably Figs. 80 and 130.

The book frequently suffers from brevity. For example, after stating Newton's Laws of Motion on page 25 the student is left to "think for himself" of the "many everyday phenomena" which "*verify* the truth of these laws." Again, the paragraph on cylinder condensation (p. 223) does not give a complete account and even fails to emphasise the salient facts.

S. H. M.

CHEMISTRY

Lehrbuch der organischen Chemie. By PAUL KARRER. Fourth edition. [Pp. xxiii + 955, with 1 plate and 3 figures.] (Leipzig: Georg Thieme, 1936. RM. 34—paper covers; RM. 36—bound.)

PROF. KARRER's text-book is arranged somewhat differently from the usual. In the aliphatic division the mono-substituted derivatives are considered together, the monohalogen derivatives monohydric alcohols, ethers and their sulphur analogues, amines and organic derivatives of phosphorus, silicon, boron, the metals, etc.; then dissubstituted derivatives, dihalogen compounds, aldehydes and ketones, then trisubstituted derivatives, hydrocyanic acid, carboxylic acids; compounds with more than one group follow, for example, glycols, dicarboxylic acids, amino acids and so on. This treatment, though systematic, does not offer any great advantage over those more usually adopted and suffers in that it separates, for example, the di and polyhydric alcohols and is not very satisfactory when applied to the aromatic series.

The material is well chosen and is ample for supplying a sound knowledge of systematic organic chemistry. In the aliphatic section the carbohydrates are naturally well treated; in the aromatic group rather more attention than usual is paid to the dyestuffs. Unusual features are the attention devoted to the sterols, hormones, vitamines and a useful series of tables. The first of the latter, listing the 108 products isolated from coal tar with dates and references, is impressive. Production figures of coal, oil, sugar, dyes, artificial silk, etc., draw the attention of the student to the economic importance of organic products and the other tables are of interest.

As a text-book of descriptive organic chemistry the work is excellent for honours students, but on the theoretical side the outlook is that of the last generation, although some of the latest ideas on the structure of organic compounds are briefly mentioned.

O. L. B.

British Chemicals and Their Manufacturers. Second Edition. [Pp. 466.] (London: The Association of British Chemical Manufacturers, 1937.)

THIS publication, the first edition of which appeared in 1935, is the official directory of the Association of British Chemical Manufacturers, and is issued

with a view to giving purchasers in all parts of the world information regarding the products manufactured by members of the Association. It is stated that all members of the Association (which is incorporated under the British Companies Law by licence of the Board of Trade) are *bona fide* British manufacturers of chemical products, and every member has declared that he is the actual maker of the products against which his name appears. In addition to a classified list of these chemicals, lists are included of proprietary and trade names and of proprietary and trade marks.

The directory is printed in English and in five other languages, *viz.* French, Spanish, Italian, Portuguese and German, and it is obtainable gratis by enquirers who are genuine purchasers of chemicals. Copies are issued only from the offices of the Association, 166 Piccadilly, London, W.1, to such enquirers, and cannot be obtained from booksellers.

H. W. C.

Carbon Dioxide. By E. L. QUINN and C. L. JONES. American Chemical Society Monograph Series, No. 72. [Pp. 294, with 89 figures.] (New York: Reinhold Publishing Corporation; London: Chapman & Hall, Ltd., 1936. 37s. 6d. net.)

IN view of the growing importance of carbon dioxide, there are many who will agree with the authors in their preface that a treatise is needed in which much of the available information concerning this substance should be assembled in usable form. Prof. Quinn and his co-author, who was formerly chief engineer to the American Dry Ice Corporation, have performed their task in a very able manner, and the book gains much from the blend of the academic and industrial points of view. They have aimed at a broad treatment of the many phenomena in nature and industry in which carbon dioxide is an important factor, and have borne in mind the needs of the industrialist, research student and general reader. It is pointed out that the rapidly changing conditions in the carbon dioxide industry, as well as the attempt to include in one volume such widely differing aspects of the subject as the carbon dioxide balance in nature, the blasting of coal with liquid carbon dioxide and the treatment of skin diseases with the solid form, has rendered it difficult to attain these objectives. Nevertheless, the authors are to be congratulated on the success of their efforts in gathering together in such a compact form as the present volume so much valuable and interesting information.

The first of the eight chapters deals briefly with the early history of carbon dioxide, both scientific and industrial, and there follows an extremely interesting account of carbon dioxide in nature. Chapters III and IV are devoted respectively to the physical and chemical properties of carbon dioxide. Chapter V, which refers to carbon dioxide and vital processes, includes such matters as the stimulation of plant growth and the indirect fertilisation of plants with carbon dioxide, the physiological action on animals, therapeutic uses, the employment of carbon dioxide as an insecticide, carbonated baths, and the use of solid carbon dioxide as an escharotic. The remaining three chapters, which comprise approximately half the book, deal with the commercial manufacture of liquid carbon dioxide, the manufacture and distribution of the solid form and finally the commercial uses of all three forms.

In addition to the 89 figures, the volume includes 70 tables in the text and a further 20 in the Appendix. The latter contains 6 pages of Patent

references, as well as physical data assembled under the headings Saturated Liquid and Vapour and Saturated Solid and Vapour.

In the opinion of the writer the authors' method of treatment is well justified, and has resulted in a work which is not only timely and valuable, but which also makes very pleasant reading.

<div align="right">H. W. CREMER.</div>

School Certificate Chemistry. By A. HOLDERNESS, M.Sc., and J. LAMBERT, M.Sc. [Pp. x + 414, with 138 figures.] (London : William Heinemann, Ltd., 1936. 4s. 6d.)

THE authors had two objects in mind when they set out on this work. They wanted to produce a book which would be suitable for revision purposes, to supplement "the pupil's own descriptive effort," and this they have done very well, for the matter is well arranged, the diagrams clear, and the exercises carefully chosen. They also wanted to avoid the dreary presentation of unconnected facts, so they used the Electrochemical Series as a basis of classification. The compounds have also been grouped by the acid radical thus showing the gradation of behaviour between the members of any one group. This seems to be a useful line of approach for pupils too immature to understand the Periodic Classification.

Two criticisms, however, can legitimately be made. As this book is intended for the two years before School Certificate, the interspersion of descriptive work among the "theory" would have been better than the concentration of the latter into the first 120 pages. Secondly, those teachers who consider that the main object of School Certificate Chemistry is not to make the pupil a "little chemist" but rather to give him some insight into the Method of Science would prefer that more attention had been paid to the experimental or historical approach to the common theories. However, for examination results the book is very suitable.

<div align="right">E. D.</div>

Distillation. By J. REILLY, M.A., D.Sc., Sc.D., D.-ès-Sc., F.Inst.P., F.R.C.Sc.I., F.I.C. [Pp. viii + 120, with 45 figures.] (London : Methuen & Co., Ltd., 1936. 3s. 6d. net.)

PROF. REILLY's little book on Distillation is one of Messrs. Methuen's useful monographs on chemical subjects. To deal at all adequately with the subject within the compass of 118 small octavo pages must have proved a difficult matter, and the author is to be congratulated on the amount of material he has managed to include and on the lucidity with which he has treated it.

The book opens with an introductory chapter on the physical principles involved in distillation processes, in the course of which a theoretical discussion of the distillation of mixtures of liquids is given. Following this general discussion of the problem, the theory and practice of fractional distillation in small or large scale, intermittent or continuous, stills at atmospheric pressure is considered in Chapter II, while the various forms of fractional distillation in a vacuum are similarly discussed in as much detail as space permits in Chapter IV. The intervening Chapter III is given over to a most useful account of the various types of pump now available for the production of varying degrees of low pressure, and of gauges designed for recording very low pressures. In the chapter on vacuum fractionation due

attention is paid to the recent developments in evaporative or "molecular" distillation.

The separation of the components of azeotropic mixtures is the subject of a separate chapter which, perhaps, might more logically be placed after Chapter II than after the chapter on vacuum fractionation.

Bearing in mind the small space available to the author, one is left with the impression of a most useful monograph, adequate in theoretical treatment, and well garnished with practical points concerning most of the many applications made of fractional distillation, both at atmospheric and reduced pressure, in the laboratory and also in manufacturing processes.

The last 25 pages of the book are devoted to distillation in steam, destructive distillation and sublimation. While one appreciates the desire to include the two last for the sake of completeness, it seems possible that the book might have gained by omitting them and utilising the space for somewhat fuller treatment of the remainder of its contents. Sublimation is of relatively small practical importance, and destructive distillation is too large a subject to receive justice in 10 pages, involving as it does the processes of coal-carbonisation, crude petroleum distillation and cracking, wood-distillation, etc.

Steam-distillation is treated conventionally, *i.e.* with respect to distillation in a current of steam at or slightly above atmospheric pressure. The important case of distillation in steam at much reduced pressures (as, for example, in the "deodorisation" of edible oils) has, however, not been included.

<div style="text-align: right">T. P. H.</div>

The Thermochemistry of the Chemical Substances. By F. R. BICHOWSKY and F. D. ROSSINI. [Pp. 460.] (New York : Reinhold Publishing Corporation ; London : Chapman & Hall, Ltd., 1936. 35*s.* net.)

THE data of thermochemistry are mostly derived from the classical researches of Thomsen and Berthelot supplemented by more recent researches carried out with more refined technique. It is often difficult to find whether a newer value is available instead of the values given in books of tables and in any case the values have usually been given in different units. The present authors, both of whom are authorities in the field, have carried out a most useful codification of all existing data, excluding organic compounds with more than two atoms of carbon, and the result is highly satisfactory. Their tables contain 5840 values of heats of formation and 350 values of heats of transition, fusion, etc., all recalculated to moles and calories of 4·1850 international joules. In the explanatory part of the book full details of the choice of value in each case are given, so that many more values are so introduced, and there are full references to the literature, the bibliography including 3730 references. The utility of such a work to the chemist, physicist and engineer is obvious. It will save a great amount of time and labour and will provide both the expert and non-expert with a reliable compendium of material. The energy values of atoms, ions and certain molecules are also given, so that the spectroscopist will find the book of considerable service. Although the arrangement is easy to follow, the book would have been improved by a table of contents.

<div style="text-align: right">J. R. PARTINGTON.</div>

Physical Chemistry for Colleges. By E. B. MILLARD. Fourth Edition. [Pp. x + 524, with 73 figures and a table of logarithms.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1936. 21*s.* net.)

THE task of writing an introduction to modern physical chemistry is no easy one. Formerly the field was well defined and the text-books, such as Walker's *Introduction*, exactly fulfilled their purpose. Nowadays molecular physics has become inextricably bound up with physical chemistry, thus rendering the choice of the inclusion of new material very difficult. Indeed, the successful author is the person who contrives to give his students just the right account of recent advances. Unfortunately opinions differ on this matter, but it is gratifying to find in this American book that Prof. Millard has courageously essayed the task shirked by some. Since the volume has run through four editions, it is evident that classical physical chemistry is dealt with adequately. The arrangement of the chapters does not follow any definite order, for example, we find Free Energy of Chemical Change and Electrochemistry relegated to the last two chapters.

The chapter on the solid state gives a more than usually extensive account of the principles of X-ray Analysis. The theory of complete ionisation of electrolytes is always a difficult problem ; in this the author makes a working compromise. It is refreshing to find that the chapter on homogeneous kinetics now deals with gas reactions—a topic shunned by many authors. The chapter on Radiation and Chemical Change occupies a rightful position in the book, but the author's injunction to students to continue with Jevon's *Report on the Band Spectra of Diatomic Molecules* is not surely to be taken seriously ; Griffith and McKeown on *Photo Processes in Gaseous and Liquid Systems* (Longmans), though a little out of date, is a good substitute.

For the benefit of future editions the following statements might be slightly altered. On p. 10, footnote 2, it is stated that deuterium is the only isotope to be isolated in a pure state. This is contradicted on p. 432, where it is stated that the isotopes of lithium and of neon have been separated. On p. 64 surely the credit for deriving an exact equation for the number of molecules colliding with a solid surface is due mainly to Knudsen. On p. 86 a few words on the significance of the parachor would be useful. For the sake of the student obtaining a balanced historical perspective would it not be advisable to mention the pioneering work of the late Lord Rayleigh and Miss Pockels on the spreading of unifilms on aqueous substrates ? (p. 105) Answers to the numerous examples would be welcome.

The book is printed and bound well, as is usual in McGraw-Hill publications, but is rather more expensive than a number of competing English texts.

H. W. MELVILLE.

A Text-book of Physical Chemistry. By SYLVANUS J. SMITH, M.A. [Pp. xii + 355, with 164 figures.] (London : Macmillan & Co., Ltd., 1936. 5*s.* 6*d.*)

THIS text-book of physical chemistry is intended for schools, and takes the subject to Higher School Certificate standard. In general, the subject matter is adequate, though the omission of any mention of the determination of the basicity of acids is to be noted. The chapter on the physical proper-

ties of liquids deals almost entirely with vapour pressure, and there is only a brief mention of surface tension. The electronic theory of valency is treated in connection with chemical reactivity. It is not easy to follow, as the author does not treat the preliminary work on the structure of the atom sufficiently fully. It is surprising to find that the name of Sidgwick is nowhere mentioned in connection with this theory, whilst the Octet theory of Lewis, now quite out-of-date, receives more than its fair share of attention. Some of the diagrams are a little misleading. That showing the path of the shared electrons in the hydrogen molecule (Fig. 99) is of little value, and leads the student to believe that the mechanism of electron-sharing is definitely known. In Fig. 34 the critical solution temperature of phenol-water mixtures is given as 68°, whilst in the text the value given is 70°. Actually both are wrong, the correct value being 66·5°. In the text the critical solution temperature of carbon disulphide-methyl alcohol mixtures is given as 80·5°, whilst in Fig. 34 it is given as 40·5°, but this is probably a printer's error. In Fig. 36, the upper critical solution temperature of nicotine-water mixtures is shown at about 230°, whilst in the text it is given as 208°.

The book should, however, serve its purpose as a school text-book quite well. It contains a number of examples, some original, others from examination papers. A great point in its favour is its low price.

A. J. M.

Solutions of Electrolytes : with particular application to Qualitative Analysis. By L. P. HAMMETT, Ph.D. International Chemical Series. [Pp. x + 211, with 11 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1936. 12s. 6d. net.)

THIS book was first published in 1929 and has reached its seventh impression. The author is concerned with inorganic analysis not primarily as a useful practical art, but as a means of teaching chemistry. He maintains that the analytical methods illustrate the general principles applying to electrolytic solutions as well as or better than any descriptive experiments designed chiefly for that purpose. From this standpoint he discusses strong electrolytes, the solubility product principle, weak electrolytes, hydrolysis, complex compounds and oxidation-reduction reactions. The properties of colloids are very briefly referred to in the chapter on hydrolysis. In the light of present knowledge the values given for the maximum co-ordination numbers of several ions will have to be increased : the book is, however, unusually free from errors.

The practical section of the work is of deliberately limited scope. Seven pages are devoted to preliminary exercises on the use of the principal analytical reagents, and stress is here laid on the desirability of discriminating from the outset between large quantities, moderate quantities and traces. A fifteen-page outline is then given for a method for the separation of the cations usually included in an elementary course but the detection of anions is not considered. A valuable feature is the emphasis placed on certain of the notable imperfections of the qualitative procedure and of the steps to be taken to avoid serious errors on this account. An example of the excellent illustration of important points is the striking warning given against the danger of attempting direct tests for specific cations before effecting a preliminary separation of an unknown mixture, by citing the fact that

strontium and nickel together interfere with the complex nitrite test for cobalt, whereas neither of them does so by itself.

The scope of the theoretical part is limited to those aspects of the subject which could be understood by a student at the time of studying the practical section, and does not therefore include for example the mathematical aspect of modern views on strong electrolytes. On the other hand the author considers it particularly important to capitalise in our instruction in chemistry the great advance in clarity and simplicity which our views on the nature of strong electrolytes have recently attained. The very limitation of scope combined with the lucidity and thoroughness of the discussion of selected topics has contributed to making this book one which is most stimulating both to student and teacher.

T. B. SMITH.

Salts and their Reactions. By LEONARD DOBBIN, Ph.D., and JOHN E. MACKENZIE, D.Sc. Sixth Edition. [Pp. ix + 246, with 1 figure.] (Edinburgh : E. & S. Livingstone, 1936. 6s. net.)

THIS little book originated in some notes of laboratory instruction prepared by Prof. A. Crum Brown in 1889. It has passed through six editions, keeping pace with the developments of practical chemistry and has now somewhat outgrown its title, a section on volumetric analysis and another on experiments with some common organic substances being included. The book is pleasantly written and produced and may be recommended with confidence as an introductory course for beginners. Only two minor criticisms suggest themselves; the multiplicity of type on pp. 158–65 is confusing, and the reviewer thinks that the way volumetric calculations are presented might be improved.

J. N. S.

A Textbook of Elementary Quantitative Analysis. By CARL J. ENGELDER, Ph.D. Second Edition. [Pp. xiv + 270, with 12 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1936. 13s. 6d. net.)

THIS book is a companion volume to Engelder's *Textbook of Elementary Qualitative Analysis* reviewed in SCIENCE PROGRESS in July 1934. The second edition has been enlarged by increasing the number of experiments described and the text has been re-arranged so that the student begins the course with volumetric instead of gravimetric analysis, as in the first edition. This change conforms with modern practice. Several differences between American and English methods are at once apparent on reading the text. The use of a second burette instead of a pipette in acid-alkali titrations enables the student to re-adjust the end point as often as he pleases, but the method becomes clumsy when, in order to standardise an acid against a weighed amount of sodium carbonate, a second burette containing sodium hydroxide (previously balanced against the acid) is required to back titrate an over-stepped end point, and the method is open to the criticism that it does not help to develop skill and confidence in the student. Thiosulphate solution is standardised against pure iodine, potassium bromate or pure copper but the use of standard dichromate or permanganate is not mentioned. The author outlines the use of normality factors in calculating

volumetric results but prefers the " titre " method, *i.e.* the immediate conversion of all volumes into weights. This is less direct and logical than the "normality" method, and when combined with the adjustable end-point technique leads to involved explanations (p. 84) which must appear very confusing to a beginner. The book is well produced, free from errors but rather expensive.

J. N. S.

Elementary Quantitative Analysis : Theory and Practice. By H. H. WILLARD, Ph.D., and N. H. FURMAN, Ph.D. Second edition. [Pp. x + 436, with 35 figures.] (New York : D. van Nostrand Co., Inc. ; London : Macmillan & Co., Ltd., 1936. 14s. net.)

THIS book aims at giving rather more than is required for a first year's college course on volumetric, gravimetric and electro-analysis. The practical details are discussed in close conjunction with the theoretical principles without which an intelligent understanding of sources of error is impossible. The suggested course amply covers what would by general consent be regarded as an essential minimum syllabus, and adds thereto an interesting selection of methods representing modern developments. Any such selection naturally reflects the interests of particular writers, and this is all to the good inasmuch as the object of giving the newer methods in an elementary text-book is to stimulate interest in analysis as a rapidly growing subject. One welcomes, for instance, the section on the use of ceric sulphate as a volumetric reagent by authors who have contributed largely to the development of the subject, even though in many places the use of this reagent would be restricted to the later years of an honours course. Much has been done in the theoretical discussions and the very numerous Review Questions and Problems to widen a student's outlook by indicating many applications of reagents old and new which are not included in the practical exercises. In view of this feature a clearer indication of the utility of titanous salts might well have been given. It is true that Knecht and Hibbert's book is included in the excellent 5-page list of works on analytical chemistry, but unfortunately the mere title does not indicate the scope of this monograph.

The instructions on the use of the pipette would hardly ensure the relatively high accuracy of which the instrument is easily capable. In particular, a drainage of only a second or two after free flow has stopped is suggested, which would be far less satisfactory than the 15 seconds recommended by the N.P.L. The weighing out of separate portions of solid for each titration is frequently rather than occasionally recommended on the ground that it avoids the error of measuring an aliquot part. Provided that measuring flask and pipette are properly handled this is hardly worth while, apart from other unusual precautions such as making allowance for temperature changes of solutions. In general, however, the experimental minutiæ as well as the underlying theoretical principles are very clearly discussed in this excellent text-book.

T. B. SMITH.

Colorimetric Methods of Analysis ; including some Turbidimetric and Nephelometric Methods. Vol. I : Inorganic. By F. D. Snell, Ph.D., and Cornelia T. Snell, Ph.D. Second edition. [Pp. xxiv + 766, with 109 figures.] (London : Chapman & Hall, Ltd., 1936. 45s. net.)

It is too often thought that the sphere of usefulness of colorimetry is limited to the determination of traces and a few macro-determinations where speed is of greater importance than accuracy. The work under review describes the estimation of over fifty of the elements as well as a number of acid radicals, and many of these methods are capable of an accuracy of 1 per cent. It includes the determination of metals and pH in many organic materials, especially those of biological importance. Besides recommended methods, useful mention is made of many proposals that merit further investigation, and where contradictory statements appear in the literature these are duly noted. In some instances the impressions given seem unduly optimistic, *e.g.* the estimation of zinc by the obscure reaction with resorcinol. When one takes into account the many sources of interference with the estimation of iron by thiocyanate a certain healthy scepticism may well be maintained towards methods which have not been critically examined by a number of workers. A valuable feature of the book is the full listing of sources of error to which the more firmly established methods are liable.

Detailed applications are preceded by 100 pages describing various types of method and apparatus, and general sources of error. The statement made on pp. 35 and 74 to the effect that the dilution method assumes Beer's Law to hold is incorrect, for the context implies a general application, whereas such considerations would only be of importance in the exceptional case of a colloidally dispersed material the colour of which varies considerably with the conditions of mixing. Among slips of less importance it may be mentioned that the discussion of dichromatism refers to Fig. 96, but neither this nor any other figure in the book illustrates the point in question.

Since only an elementary treatment of the theory underlying the determination of pH is intended, it seems of doubtful utility to include a series of equations of importance only for spectro-photometric types of measurement which are not considered in the practical part of the book. Without fuller discussion the reasons for several of the steps would hardly be apparent, *e.g.* the advantage of expressing α in terms of data obtained at two wavelengths rather than at one. There are a couple of slips in citing equations and α has been omitted from one of them. It is misleading to put the molar concentration of water as unity when defining its ionic product, and the dependence of this constant on temperature might well have been emphasised. Much valuable practical information is given concerning the determination of pH.

This work makes a very useful addition to the laboratory bookshelf, especially inasmuch as it contains a great deal of information from journals which are not easily accessible.

T. B. Smith.

Semi-Micro Qualitative Analysis. By CARL J. ENGELDER, Ph.D., TOBIAS H. DUNKELBERGER, B.S., and WILLIAM J. SCHILLER, Ph.D. [Pp. x + 265.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1936. 13s. 6d. net.)

NOT only has micro-chemistry made remarkable progress during the last ten years, but a special technique, the drop-reaction method, which occupies a position intermediate between the conventional macro-methods and the more refined micro-analysis, has been developed. The operations of qualitative analysis are performed, not in test-tubes and beakers, but on a much reduced scale using drops of solutions on black or white tiles or on filter papers, and many relatively new reagents of the dimethylglyoxime type are employed.

The Authors of this book, impressed by the possibilities of these methods, have worked out a scheme of drop-reaction analysis based on existing macro-methods and have used it during the past eight years as the sole medium of instruction in elementary qualitative analysis. To do so must have required both courage and patience and not every teacher would care to undertake the task. Students who have had a good grounding in the conventional methods can very soon master the drop-reaction technique and use it as an alternative method when only small amounts of material are available. Such students would require a relatively small text-book containing methods and tests not common to both systems. The size of this book is due to the fact that it presents an alternative and not a supplementary scheme of analysis.

Part I (approximately two-fifths of the whole) deals with fundamental theory, i.e. the law of mass action, reaction velocities, solubility product, hydrolysis, amphoterism, the nature of valence, etc. Parts II and III contain the reactions of the cations and anions respectively and Part IV (31 pages) is devoted to systematic micro-analysis. Those who wish to follow the authors' example will be interested in the book, but those seeking a supplementary text-book will find much here that they already possess elsewhere.

J. N. S.

The Scientific Principles of Plant Protection : with special reference to chemical control. By HUBERT MARTIN, D.Sc., A.R.C.S., F.I.C. Second edition. [Pp. xii + 379.] (London: Edward Arnold & Co., 1936. 21s. net.)

THE title of this book, even with its sub-heading, might lead one to suppose that it dealt with the subject of Nature Reserves, since the term plant protection has become generally adopted in that connection. Actually the control is not prevention of human activities or even the checking of Bracken invading a desirable community, but the work treats of the whole problem involved in the protection of plants against pests. After two introductory chapters treating of the more general aspects and a chapter on biological control, there follow chapters on fungicides, insecticides, weed-killers, fumigants, seed and soil treatments, toxicity and chemical nature, traps and the combating with centres of infection and vectors. The book is a very useful reference text on the subject matter with which it deals, a utility which is enhanced by the bibliographical references which follow the description of each type of fungicide, insecticide or fumigant.

As one peruses these pages one cannot help but reflect how little research has been directed towards the elucidation of the effects, either immediate or delayed, which our methods of protection have upon the plants we are protecting. This is perhaps largely because of the commercial interests involved, which are more often concerned with quantity than quality. But until we know the effects of our methods of control, alike upon the potential prey as upon the predator, we cannot feel that our methods of protection are upon a secure scientific foundation.

The book is one that can be thoroughly recommended as an account of methods at present at our disposal in the attempt to control the balance of life.

E. J. S.

Farm Soils : Their Management and Fertilisation. By E. L. WORTHEN, M.S. Second edition. The Wiley Farm Series. [Pp. xiv + 468, with frontispiece and 220 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1935. 13s. 6d. net.)

THIS book is planned in a very definite although rather unusual way. It covers the whole ground of crop husbandry as practised in the United States and the subject matter is divided into chapters ("Manuring," "Liming," "Controlling the Water Supply," etc.) in a very orderly and rational sequence. Each chapter has two main parts, "Operations" and "General Information." In the first of these parts the author deals with the practical aspect of the subject of the chapter, describing methods of procedure and management. The information given here is sound and clearly set out. Its value is enhanced, too, by a large number of photographic illustrations. In the second part of each chapter, "General Information," the scientific aspect of the subject is dealt with and a good deal of relevant categorical information is given. Many readers would probably prefer to have most of the "General Information" before "Operations" and not after. As the book is arranged one reads, for instance, a full and good account of the practical use of superphosphate and then later on what superphosphate is.

Many chapters have additional sections—"Community Studies," in which suggestions are made for visits, etc., "Problems" and "References."

The author has been too ambitious on the scientific side. He endeavours in ten pages or so to deal with all the relevant chemistry including ionisation and the meaning of pH. It is doubtful if anyone unacquainted with chemistry would gain anything worth while from this. But on the practical side and in the suggestions given for further study the book should be valuable, particularly in the States, and serve the community as successfully as the first edition appears to have done.

N. M. C.

Some Soil-Types of British Honduras, Central America. By F. HARDY, H. P. SMART and G. RODRIGUEZ. Studies in West Indian Soils, No. IX. [Pp. 56, with 3 maps.] (Port-of-Spain : Government Printing Office, 1935. 2s.)

THIS is the ninth publication in a series of studies on West Indian soils. The work was undertaken at the request of the Governor of British Honduras and its chief object was to obtain information about the soil-types of the Colony with special reference to their suitability for bananas and grape

fruit. The work has involved the study of 55 profiles and 464 samples of soil, and a very large amount of analytical data is given.

British Honduras, which is 8867 square miles in area, is largely made of upper carboniferous and tertiary limestone rocks. The authors say that although in the tropics its climate is virtually sub-tropical. The rainfall varies from 60 inches in the north to 120 in the south, with a dry period in the spring.

The soils have been grouped by the writers into four main groups with subdivisions within each. (1) Alluvial soils—beach and river. (2) Sedentary soils from granite, schists and slates. (3) Sedentary soils from non-calcareous rocks. (4) Sedentary soils from calcareous rocks. The soil data can hardly be reviewed in a small space, but they will be read with interest by those pedologists concerned with soil surveys.

The banana soils are found for the most part to be under satisfactory rainfall conditions—the banana crop makes heavy demands on water. The potash and phosphate are thought to be low but the nitrogen supply adequate. Some banana soils are considered to be too shallow.

Grape fruit requires a plentiful supply of water and mineral nutrients and the lime status of the soil is important. Many of the grape-fruit soils are acid and the authors doubt whether growers can be persuaded to apply the necessary liming material. They contend that as the potash and lime content of the leaf of the grape fruit are reciprocally related and as the lime content is directly dependent upon the soil lime, that liming will probably effect economies in potash fertilisers.

Sugar-cane soils are reported as satisfactory in nutrients except possibly in phosphate and in lime status. The root range is restricted in certain parts. We are told, however, in a quotation by the authors from Earle, that the agricultural requirements of the sugar cane are very simple and that if it were otherwise the world would be short of sugar, "for there are few large-scale crops that are grown by such crude antiquated methods."

The report is of definite interest to soil chemists and to those interested in the cultivation of bananas, grape fruit and sugar cane.

N. M. C.

Soils : Their Origin, Constitution and Classification—An Introduction to Pedology. By G. W. ROBINSON, M.A. Second edition. [Pp. xviii + 442, with 5 plates and 17 figures.] (London : Thomas Murby & Co., 1936. 20s. net.)

THE sub-title of this book is really more significant than its main title. The first edition in 1932 was the first English work on Pedology—the scientific study of the soil as a natural object of study. In that book Prof. Robinson reviewed and described the main soil types of the world and discussed the processes of soil genetics in a way that had not been done before in English.

Much scope for improvement was not very apparent when the first edition was published, but the author has found ways of enhancing the value of the book now that a second edition is demanded.

This second edition is made both more attractive and more useful by the additional plates and the better frontispiece. The plate showing different types of soil structure is particularly welcome. Many additional references are given and recent work has been incorporated in the text, involving a recasting of the chapters on clay, soil water and soil classification.

The book is far removed from a categorical text-book. The author desired to give the book "something of the character of a work of art" interpreting the ideas of the author—"a picture rather than a photograph"; and he has succeeded. It is true, as the author says, that another pedologist might arrange the facts in a very different perspective, but that is far from a criticism of the book. Much of the value of the book lies in the fact that it is a personal presentation of the subject.

<div align="right">N. M. C.</div>

Humus: Origin, Chemical Composition and Importance in Nature. By S. A. WAKSMAN. [Pp. xii + 494, with 45 figures.] (London: Baillière, Tindall & Cox, 1936. 30s. net.)

THE very large amount of work that has been carried out in the field of which this volume is the subject can be estimated from the fact that the bibliography contains over thirteen hundred references.

It is quite unnecessary to emphasise either the scientific interest or the economic importance of humus, and probably there is no one better fitted to deal with the subject than the present author, who for more than twenty years has been engaged in research on the changes which the organic matter undergoes in the soil or on the organisms responsible for bringing these changes about.

The first section of the book is concerned mainly with the history of the subject and the development of our conceptions of the nature of humus.

The second and largest section deals with our present knowledge of the origin and nature of humus, whilst the third section treats of its decomposition, its functions and applications.

The extremely complex character of humus fully justifies such extensive treatment as it here receives, for though we may admit that the ligno-proteins constitute the major complex involved, there is still much to be learnt concerning their compounds and associates.

The text is clearly written and constitutes a standard work of reference that everyone interested in pedology should possess. For this very reason one would have welcomed a fuller subject index, which scarcely constitutes an adequate guide to the large amount of information which these pages contain.

<div align="right">E. J. SALISBURY.</div>

A Shellac Patent Index. By R. W. ALDIS, Ph.D., D.I.C. [Pp. iv + 115.] (Nankum, Ranchi, Bihar and Orissa: Indian Lac Research Institute, 1935. Rs. 2/8.)

THIS is an extremely interesting and useful publication, for although the title correctly indicates the nature of the contents, the book is something more than a mere index.

The authors have divided the patents into thirteen chapters, the first relating to lac manufacture, the following eleven to various industrial applications. The thirteenth and last chapter is devoted to patents for the manufacture of shellac substitutes.

The present status of shellac in modern industries and the possibilities of development are briefly discussed in an introductory note and in each chapter the list of patents is prefaced by a short statement of the position which shellac occupies in the industries in question.

The patents are arranged chronologically and contents of each indicated. These notes though short are adequate, and there is a certain terse vigour about the style of the whole work which is all too frequently absent from technical publications of greater bulk and more ambitious literary pretensions.

The work has been carefully done and we have noted only a few errors, mainly of obvious character, as for example the use of " sulphonation " to describe the process of heating with sulphur (p. 64) and the reference to aniline as an " oxylamine " (p. 90). The name and subject indexes are very thorough, and altogether this is an invaluable and stimulating compilation to all interested in the development of the lac industry upon systematic and scientific lines.

T. H. B.

Flour Milling Processes. By J. H. Scott, B.Sc. [Pp. xvi + 416, with 51 figures, including 8 plates.] (London : Chapman & Hall, Ltd., 1936. 21s. net.)

Except for Simon's book on *The Physical Science of Flour Milling* (Liverpool, Northern Publishing Co., 1930), and certain specialised pamphlets in the Technical Education Series of the National Joint Industrial Council for the Flour Milling Industry, nothing relating to English milling has been published for very many years. Thus there was a distinct need which the present work largely fills. The form of the book is unusual. It is hardly a treatise, because although some of its chapters make the work a very valuable reference book, others are far from exhaustive. Again, in some ways the book has the character of a monograph. Thus, like his illustrious predecessors, Kick and Kozmin, the author devotes much space to original theoretical investigations. These are often most refreshing, being based on first principles and shrewdly conducted (for example, the derivation of the ultimate proportion of the endosperm by means of density data on p. 127, and the general discussion of the allotment of roll surface between breaks on pp. 150 *et seq.*), but in some cases they could well have been omitted, since they lead nowhere and may impress but needlessly dismay the non-scientific reader (for example, the attempt on p. 67 to apply the general law of diffusion to water movement within grain).

Much of the valuable information contained in the book results from the author's own experiments. A few of these, especially some concerned with wheat conditioning and movement of water inside the grain, are open to severe criticism, but for the most part they are satisfying and often of an original nature (for example, the relation of frictional coefficient and moisture content on p. 131 and the determination of trajectory of stock leaving the rolls on pp. 389–91).

The chapters cover successively the various types of process used in milling. Not all are of the same high merit. In particular the amount of valuable published work on the behaviour of moisture in wheat and on conditioning is small and the present book can hardly be said to add to it. Chapters III and IV, which deal with these problems, are its least satisfactory features. On the other hand, the section devoted to milling (particularly the chapters on break rolls, purification and the reduction system) is as a whole a very valuable contribution to the literature.

The book is particularly valuable as representing up-to-date and comprehensive knowledge of trends throughout the English milling industry

(often revealed in epitomised form, *e.g.* the fourth paragraph on p. 139); because of its underlying practical experience, resulting in a balanced presentation of the pros and cons of various types of machine; because of the author's intellectual honesty and fairness which are manifest, not only in his recording of his own experiments, but in his discussions of machines by various makers; and because of his shrewd statements of matters needing further investigation. The book is attractively produced.

E. A. FISHER.

Flavours and Essences : A Handbook of Formulæ. By M. H. GAZAN. [Pp. viii + 115, with 5 figures.] (London : Chapman & Hall, Ltd., 1936. 25s. net.)

FLAVOURING is one of the most important factors in the manufacture and production of all foods and drinks, because an agreeable taste or flavour makes them more palatable and thus acts as an appetiser and stimulant to the human system. All people who are actually connected with the manufacture of foods and drinks should possess a knowledge of the composition and properties of flavours and essences whether they be foods-chemist, craftsman or lecturer.

Hitherto, the literature on this subject has been of rather stinted character in the English libraries. Mr. Gazan in his book entitled *Flavours and Essences* has given this information in a very comprehensive manner, clearly indicating that the manufacturing of flavours is a special trade of its own, and that a vast amount of labour, time and experience must have been necessary to gain the knowledge of the composition of the innumerable bases which are required to be prepared and kept in stock before proceeding to the actual preparation of the essences as given in the book.

The appendix supplies much useful information, including diagrams of the simple and inexpensive apparatus recommended by the author to be used, a brief description of the properties and uses of each synthetic ester or flavour which is used in the various formulæ. There is also a solubility table of the terpeneless oils and their concentration, and last but not least, a useful description of the preparation and mixing of colourings in order to give the essence a natural appearance.

To manufacturers and practical foods-chemists such a book will be invaluable; it is also interleaved for notes and additional formulæ. As a reference book it should be in every library.

MARION CHADWICK.

A Rudimentary Treatise on the Manufacture of Bricks and Tiles. Based on the work of EDWARD DOBSON. Fourteenth edition. Revised by ALFRED B. SEARLE. [Pp. xiii + 234, with 136 figures.] (London : The Technical Press, Ltd., 1936. 10s. 6d. net.)

IN this fourteenth edition of Edward Dobson's book there has been considerable revision and extension of the subject matter by Alfred B. Searle, so that the work has been brought entirely up to date.

After a few historical notes and a useful summary of the general principles of brickmaking, there follows a description of the materials used, which is sufficient without being too highly technical. This is succeeded by a chapter on preliminary operations, which lays proper emphasis on the importance of sufficient preparation of the clay before moulding.

Descriptions of the following types of bricks are given in detail : London stocks, hand-moulded facing bricks, slop-moulded bricks, dressed bricks, cutters and rubbers, ornamental, white, and fire bricks ; the beauty of hand-moulded facing bricks is rightly stressed, although machines are supplanting the brick moulder. Both red and blue engineering bricks are described, with some interesting notes on the origin of the colour of the latter. In connection with the foregoing varieties, the intermittent Scotch and down-draught kilns are mentioned.

Mechanical methods of manufacture are described in the sections on the wire-cut, stiff-plastic, and semi-dry processes (the latter including the manufacture of " Flettons "), with notes on the various types of continuous kilns in use.

There is a useful section on the manufacture of roofing tiles, encaustic tiles, hollow blocks and drain-pipes.

The subject matter is treated in an interesting manner, and the book provides an excellent introduction to a detailed study, or the general know-ledge suitable for those interested in clay products or building work.

<div align="right">F. E. O.</div>

GEOLOGY

Principles of Structural Geology. By C. M. NEVIN. Second edition. [Pp. xii + 348, with 163 figures.] (New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1936. 17s. 6d. net.)

THIS is the second edition of a work which seems to have escaped review in SCIENCE PROGRESS on its first publication. The book begins appro-priately with chapters on the physical properties of rocks, and stress and strain relations. These are followed by four chapters each dealing with a single type of rock deformation, namely, flexures, faults, jointing and cleavage. Then come three chapters on structures associated with igneous intrusion, structures in unconsolidated sediments, and reflection of rock structures in the topography respectively. The three final chapters deal with more general and theoretical topics. Chapter X is entitled " Some Facts, Inferences and Hypotheses regarding the Earth," and treats of density, isostasy, earthquakes, temperature gradient, shrinkage, compression, etc. " Continents and Ocean Basins " form the subjects of Chapter XI (" conti-nents " misspelled " contents " in Contents, p. vii); while " Mountain Systems " are expounded in Chapter XII. An admirable Introduction stresses the fact that the earth is in a state of mobile equilibrium, and that the major structural features of the crust result from deep-seated adjustments to over-acting stresses produced by various causes. In the later inter-pretative chapters the pros and cons of disputed theories, such as continental drift, are set out impartially for the student to think about for himself. Facts, inferences and hypotheses are clearly marked off in separate paragraphs and sections, with the result that the text has the utmost lucidity and force. The preface states that the book is primarily for " beginning " students, but we feel that many of the subjects dealt with are more suitable for students of higher grade than elementary ; and for these we think that Prof. Nevin's book provides a most valuable text.

<div align="right">G. W. T.</div>

The Structure of the Alps. By Léon W. Collet, D.Sc., LL.D.
Second edition. [Pp. xvi + 304, with 12 plates and 84 figures.]
(London : Edward Arnold & Co., 1936. 20s. net.)

Prof. Collet's book, published in 1927, has been out of print for some
little time and all British and American geologists will welcome this work
in its revised form.   In the present edition certain notable changes and
additions are introduced and the author has brought the work up to date
by the inclusion of the results of the more important recent advances in our
knowledge of the structure of the Alps.

Some twenty new text figures have been added and the map is now
printed in two colours which certainly makes for greater clearness.

In the Introduction, Part I, the author, when dealing with Argand's
Synthesis, criticises the term " ground folds " which has appeared in certain
English writings (a translation of Argand's term " plis-de-fond ") as not
conveying Argand's true meaning and prefers to use the term "foundation
folding."   While still upholding Argand's general conclusion, he gives also
the more recent views of Kober.   This author has postulated a southern
thrust-mass as well as a northern one, separated by " Internides " or
" Betwixt mountains."   These latter are absent from the Alps where, owing
to intense compression, the Dinarids lie in juxtaposition to the Alps.   Stille
and Seidlitz support Kober's double-thrusting theory in place of a general
movement from the south as explained by Argand.   Our author while
considering both these possibilities, remarks : " From what I have seen in
different regions of the Western Mediterranean, I regard Argand's theory
as fitting the facts much better than Kober's Synthesis."   The author's
translation of Kober's term " Zwischengebirge," though literally correct, is
somewhat unfortunate and as Prof. Boswell has pointed out the term " Median
mass," already in use in this country, is certainly preferable.

In Part III, the chapter on Mont Blanc and the Aiguilles Rouges Massifs,
a district to which the author has himself made important contributions,
has been rewritten.

These Massifs which, as he points out, originally formed one crystalline
massif are treated together.   They represent portions of the Hercynian
Chain which formed the southern border of the Foreland of the Alpine geo-
syncline and are now separated owing to the erosion which has taken place
along the softer sedimentaries of the Chamonix Zone.   Several important
maps and memoirs on this district have appeared since 1927.   These are
largely the work of Corbin and Oulianoff who have, among other things,
discovered zones of mylonite in the granite of the central part of the Mont
Blanc Massif: these zones separate granite wedges formed by the Alpine
movements.   The connection of these softer zones with the formation of cols
and depressions, now filled with glaciers, is of considerable interest.   The
chapter on the Jura Mountains has been shortened to allow more space
for the description of the Alps and the bibliography has also been reduced
and confined to a record of the more important papers.

The section on the geology of the Jungfrau has also been rewritten and
here the author is dealing with his own ground.   The account is based on
the fine map of the chain of the Jungfrau, published by Collet and Paréjes
in 1927 and a new section across the Monch has been included.

But the most important additional material occurs in Chapter VI, in
which the author gives an interesting description of the geology of the Alpine

Range in the Western Mediterranean. This is of special importance in view of the fact that the geology of the Alpine chain cannot be separated from the geology of the Mediterranean Region.

The account is based upon the recent work of Staub, Kober, von Seidlitz and others and the author has himself studied many of the main facts on which the conflicting theories are founded.

The account deals specially with the relation of the Northern Apennines to the Alps as shown by a study of the window of the Alpi Apuane in Tuscany. This consists of two nappes—the Carrara nappe forming the window and the Spezzia nappe forming the overlying frame. The Carrara nappe is considered to belong to the upper part of the Pennine nappes of the Alps while the Spezzia nappe which has been thrust over the Carrara nappe, may, according to Staub, represent the lower part of the Grisonides and also the top of the Dent-Blanche nappe. The general conclusion, therefore, arrived at would be that the Northern Apennines from Genoa to Urbetello belong, geologically speaking, to the Alps : further an upper nappe, the Liguride Nappe, has been described by Steinmann who has shown that the isolated exposures, formerly known to Italian geologists under the name of " Terciario ofiolitico "—include in reality deep-sea radiolites and pillow lavas and are overlain by limestone of upper Jurassic or lower Cretaceous age. These form outliers or klippes and according to Steinmann constitute fragments of a third (higher) nappe, the Liguride Nappe, thrust over the Tertiary beds of the Apennines.

Recent work in Corsica and Sardinia, again points to the presence of equivalents of the Hercynian Massif of the foreland of the Alps, while in Corsica nappes, representing part of the Austrides and Schistes lustrées, also occur corresponding to the Pennine Nappes of the Alps.

Part VI concludes with a description of the more speculative results of recent work on Elba, Southern Spain and the Balearic Islands.

We congratulate Prof. Collet on introducing to English readers the interesting results so far obtained regarding the structure of the Western Mediterranean and their bearing on the extension of the Alpine Range into this region.

E. J. G.

Down to Earth : An Introduction to Geology. By C. CRONEIS and W. C. KRUMBEIN. [Pp. xviii + 501, with 64 plates and numerous illustrations in the text.] (U.S.A.: University of Chicago Press ; Great Britain and Ireland: Cambridge University Press, 1936. 17s. 6d. net.)

THIS is a serious and well-conceived introduction to geological science, with several novelties of manner and illustration. The illustrations are of two kinds : first, there are 64 pages of rotogravure pictures done like the pictures in an illustrated magazine. It is claimed that this method makes it possible to reproduce more illustrations than can be inserted by the usual procedure. It must be said that these rotogravures are of uniform excellence, both in reproduction and in selection, although the cut edges have an odd effect in a scientific book. The other kinds of illustration, inserted profusely in the text, include modern diagrams and sketches, woodcuts taken from old geological works, and a series of humorous drawings illustrating some point or some phrase in the text which has taken the artist's fancy. Some of these

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vignettes are inane and pointless, but others are genuinely witty and amusing, and may help to sustain the reader's interest.

The text is written in a bright and colloquial style ; the authors "have attempted to enliven the subject without in any sense writing it down." An analytical rather than a descriptive method, unusual in an introductory treatment, has been adopted. Furthermore, a good deal of astronomical, chemical, physical and biological material is introduced (too much in the reviewer's opinion), on the ground that this makes for a more unified treatment of the earth sciences, and brings out the relationships of geology to other scientific fields.

After a few general introductory chapters, physical and structural geology occupy the first half of the book, historical geology the second. There can be no denying that such picturesque chapter headings as " The Everlasting Hills, which are not everlasting," or "Megalomania, the curse of the Dinosaurs, who placed reliance on brawn, not brains " (which may have a modern application), to quote only two instances, must enlist the eager interest of the discerning student.

This book makes a new departure in the presentation of geological science, and we think that it is a successful venture. It may be strongly recommended to all amateurs of geology, especially to adult continuation classes, as well as to university students.

<div align="right">G. W. T.</div>

Interpretative Petrology of the Igneous Rocks. By H. L. ALLING, Ph.D. [Pp. xv + 353, with 11 plates and 48 figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1936. 24s. net.)

THIS excellent text-book utilises to the full the physico-chemical approach to the study of igneous rocks. It aims, among other things, at presenting phase-rule chemistry applicable to rocks in a form simple enough for students of geology. Such topics are treated which are necessary for interpretation of the origin of igneous rocks ; and optical mineralogy, descriptive petrography and classification are therefore not dealt with. This mode of treatment necessitates an unusual amount of attention to physico-chemical study of the constituent minerals of igneous rocks. Rock-forming minerals are described, not as definite compounds of fixed composition and of constant optical properties, but as mixtures in varying proportions of end-members which are referred to as "minals." As "Nature does not use pyrex glass and platinum crucibles," during their formation minerals have suffered different degrees of contamination with small amounts of various substances, and are thus what the chemist would call "dirty compounds." Igneous rock mineralogy is therefore somewhat complex and difficult.

As silicon is the predominating element in the mineral world, and carbon of organic nature, the book begins with a short account of these two elements and their compounds, and then passes on to a section on the classification of matter. Chapter II deals with principles of equilibria, Chapter III with solid solutions and isomorphism, and Chapter IV with polymorphism. Readjustment of composition during crystallisation, and petrographic mineralogy (types of compounds) are the subjects of succeeding chapters, after which the various rock-forming mineral groups are dealt with in the order : feldspars, pyroxenes, olivines, amphiboles and micas, alkali minerals and accessory minerals. Further chapters deal with zoned crystals, twinned crystals, order of crystallisation and intergrowths, hyperfusible constituents, and homo-

geneous equilibria. Finally, the purely petrological part of the book discusses the crystallisation of magmas, the diversification of igneous rocks, magmatic assimilation, final consolidation of igneous rocks (pegmatites), petrographic provinces and co-magmatic regions, and the origin of igneous rocks, in successive chapters. This part of the book is extremely good. Prof. Alling has assimilated the most recent literature and has presented contrasting views with the utmost fairness. The book ends with a chapter entitled "Interpretative Petrology," in which the philosophy of the subject is well treated.

The omission of decimal points in the figures for Na_2O and K_2O makes nonsense of the analyses of basaltic magmas on p. 289, but this is the only error the reviewer has been able to detect. This book should be in the hands of every student of advanced petrology, and of every instructor too.

G. W. T.

The Book of Minerals. By A. C. HAWKINS. [Pp. xii + 161, with frontispiece, 5 plates and 62 figures.] (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1935. 7s. 6d. net.)

THIS little book has been written "to tell to the people the story of the minerals" that provide us with materials and metals for the arts and industries, and gemstones for the adornment of both rich and poor. It is the author's view that most books on mineralogy cater more for the expert and the student than for the man in the street. The book begins with "Some Facts about Minerals," including how and where to collect them, their identification and origin. Then follow chapters providing a little simple crystal lore, and chemical and microscopical methods of identification. Chapter IV gives much useful information on the kinds of minerals to be found in limestone and granite quarries, in clay pits and in mines. In the final chapter some of the commoner mineral species are described.

In general the book fulfils its declared purpose excellently. We do not know what to make of the statement (p. 131) that quantities of apatite are mined in Eastern Canada to make baking powder. It may be true! The illustrations consist mainly of good photographic reproductions of actual mineral specimens.

G. W. T.

BOTANY

Botany: A Textbook for Colleges. By J. BEN HILL, LEE O. OVERHOLTS and HENRY W. POPP. McGraw-Hill Publications in the Agricultural and Botanical Sciences. [Pp. xiv + 672, with frontispiece and 335 figures.] (New York and London: McGraw-Hill Publishing Co., Ltd., 1936. 24s. net.)

THIS text-book has been used in mimeographed form for many years so that this first printed edition has much experience behind it. It is obvious that great care has been taken over its production; the writing is very clear, often, indeed, descriptions could not be bettered; and the arrangement of the subject matter is admirable, especially in the first part in which general morphology, structure and physiology are skilfully interwoven.

The illustrations, of which there are 335, are a conspicuous feature; for the most part they are new and generally of high quality, some are excellent, but a few have been spoilt by over-reduction. The inclusion of half-tone reproductions to show the habit of various plants is admirable.

The authors' endeavour to give a balanced survey of the plant kingdom has a considerable measure of success in the first part which is concerned with the structure and physiology of seed plants. The second part—the Plant Groups—is not, I think, so successful; this is due to the inclusion of too many plants in a survey occupying 345 pages from which must be deducted the space taken by 205 figures. The result is that descriptions sometimes are too short to be of any value, thus I doubt whether the ten and a half lines account of *Phylloglossum* (p. 533) will give anything but the foggiest idea to the student unfamiliar with the plant. In a general course of instruction it is, I think, better to omit certain plants and groups, such as the Myxomycetes, Anthocerotales, Rhodophyceæ and the Hydropterideæ, else the mass of detail obscures the definition, as it were, of the mental picture. But this opinion will not be shared by all teachers.

In a short review detailed criticism is hardly possible ; the authors may, however, like to know of a few statements which require revision ; for instance, the composition of anthocyanins (p. 12) and the absence of chlorophyll from the epidermis of leaves except in a few submersed plants (p. 48). Incidentally the presence of chlorophyll in the epidermis of a shade leaf of *Pteris* is indicated in Fig. 236A.

On the whole the book is attractive ; it has character and is therefore refreshing ; the authors show enthusiasm and are convinced of the importance of botany in general education, they are alive to the significance of the plant in its relation to man and to his inspiration. " Much could also be said of the æsthetic value of plants and the part it has played in the production of such masterpieces as Bryant's ' Thanatopsis ' or his ' Forest Hymn,' Emerson's ' Rhodora,' Moore's ' Last Rose of Summer,' or Joyce Kilmer's ' Trees.' Indeed, plants have been the incentive for the production of some of the most beautiful works of art, literature and music. Furthermore, the study of plants has proved to be a most pleasing and gratifying avocation for lawyers, bankers, physicians, and other professional men."

<div align="right">T. G. H.</div>

Intermediate Botany. By L. J. F. BRIMBLE, B.Sc. (London and Reading). [Pp. viii + 562, with 337 figures.] (London : Macmillan & Co., Ltd., 1936. 8s. 6d.)

THIS book is intended as a text-book for the Intermediate B.Sc. and the Higher School Certificate stage and has many of the admirable features of the author's earlier Everyday Botany. This will make it attractive to the ordinary student. While well written and arranged, there are not a few statements or figures which are either incorrect or likely to cause a false impression. Several of the figures are too darkly drawn or sometimes on too small a scale, with the result that the details are not clearly rendered ; to others there is insufficient description, as in Fig. 55, where different plant cells are figured, some in one sectional view, others in another, others again in the " solid " or in surface view. There is little by way of essential difference between the scalariform and reticulate types on p. 144. The yeast nucleus in Fig. 131 is incorrectly drawn, and the account of conjugation in Spirogyra does not take into consideration the results of recent work. The description also of the development of the columella in Mucor is incorrect. There is some difficulty in seeing how the author can accept hormone activity in growth and refuse to accept it in growth curvatures.

<div align="right">E. M. C.</div>

Morphology of Vascular Plants—Lower Groups. By ARTHUR J. EAMES. [Pp. xviii + 433, with frontispiece and 215 figures.] (London : McGraw-Hill Publishing Co., Ltd., 1936. 24s. net.)

THIS book is rather disappointing. It may be that the title is misleading, for it deals with much more than the morphology of the lower groups ; describing also the life histories and the anatomy. Prof. Eames in the preface states that not much stress has been laid on the anatomy, but what consideration is given is apparently not specially related either to the morphological or phylogenetic problems and would seem to be inadequate as a general statement or summary. The anatomy of the stem of Selaginella is described as similar to that of the stem of Lycopodium, and the roots of Lycopodium as being simple in structure. Doubt is thrown on secondary thickening in Psilotum, but no reasons are given for this view, and there is no reference to the paper in which this process was described, so that the student has no means of judging the matter for himself. Secondary thickening in the ferns is very inadequately treated, and, while vessels are described in the stem of some species of Selaginella and some ferns, no references are here given to original papers. References are, however, given to the more important recent papers dealing with other non-anatomical matters. The living and fossil representatives of the groups receive separate treatment, a method of presentation for which the author gives his reasons in the preface. This method seems an artificial one and is almost sure to give a student an incorrect idea, as if fossil plants were things apart. It would seem too that in this portion of the book too much has been attempted in the allotted space.

The best portion of the work is that dealing with living groups and plants rather scantily treated elsewhere, e.g. Isoetes, Marsilia, Salvinea, Psilotum, etc., and these sections should prove of great use to the student. The illustrations here and elsewhere in the book are well chosen and include unusual but apt figures, both from the accustomed and from new sources.

E. M. C.

The Identification of Trees and Shrubs. By F. K. MAKINS, F.L.S. [Pp. 326, with 128 figures.] (London : J. M. Dent & Sons, Ltd., 1936. 15s. net.)

THE purpose of this book is frankly to enable gardeners and the non-botanical to attach a name to the shrubs and trees which they may encounter. It is a widespread and natural desire to wish to know the names of the plants we grow, though too often the search for knowledge stops here, whereas nomenclature should be the starting-point for further enquiry.

The method employed is to provide a brief key by means of which the reader is referred to a number of diagrams ranging from only a few in one section to over two hundred in another. The diagrams in the appropriate section are then searched over and those closest to the specimen selected. The descriptions are then referred to and the identification checked by this means.

It will be evident that the main emphasis is upon the diagrams, which is as it should be in a book intended for those with no botanical knowledge. It is a book which should be very helpful to many, though it must be admitted that the diagrams are of rather unequal merit. A few of these are extremely poor and indeed we should not be surprised if anyone failed

to recognise *Kerria japonica*, or some of the Loniceras. Taken as a whole, however, the book is a very laudable attempt to provide names without tears.

E. J. S.

The Principal Rots of English Oak. By K. St. G. CARTWRIGHT, M.A., F.L.S., and W. P. K. FINDLAY, M.Sc., D.I.C. [Pp. vi + 38, with 13 plates and 2 figures.] (London : H.M. Stationery Office, 1936. 2s. net.)

THIS low-priced volume is intended to be the first of a series dealing with fungi causing decay in the chief timber species. Combining a résumé of the rather scattered information on the subject with results of recent work at the Forest Products Research Laboratory, the book is addressed to the user of wood rather than the sylviculturalist. At the same time it contains much information of more purely academic interest.

The rots are classified as those which attack (1) the standing tree, (2) the felled timber, (3) timber in buildings and structures. Keys are provided for the identification of the fungi, both in wood and in culture. The individual treatment of the more important rots includes an account of the sporophores and of the peculiarities of the rots, details of the behaviour of the fungi in culture, and of their economic importance.

Polyporus sulphureus is regarded as probably the most serious agent of decay in parkland oaks and structural timbers, while *Stereum spadiceum* ruins much standing timber, although, unless previously present, confining its attacks to the sapwood of felled trees. While *Fistulina hepatica* may cause a heart rot in over mature trees, the incipient stages produce Brown Oak : it would appear that the fungus is unable to continue development after the wood has been converted. Of the fungi attacking felled Oak *Dædalea quercina* causes much damage in this country, while the most important rot in oak in buildings seems to be *Phellinus cryptarum*.

F. W. J.

Citrus Diseases and their Control. By HOWARD S. FAWCETT. Second edition. [Pp. xv + 656, with 187 figures, including 15 plates.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1936. 36s. net.)

THIS book, originally written by H. S. Fawcett and H. Atherton Lee, but now appearing under the sole authorship of H. S. Fawcett, of the famous Citrus Experiment Station, California, has deservedly achieved a second edition, for it is the only comprehensive treatise on the diseases that attack Citrus trees and their fruits in transit. The first edition was not only a most valuable guide to plant pathologists especially concerned with citrus diseases, but it was also looked upon as a model of what such books should be and as a most illuminating commentary on the interpretation of disease in plants in general. The second edition, which has been largely re-written, is even more valuable in these respects.

The diseases of Citrus are innumerable, and with the recent development of intensive citrus cultivation in other parts of the world, *e.g.* South Africa and Palestine, besides the former centres of production, new diseases have appeared which are fully described in the second edition. It is of interest to note that the most serious diseases of citrus differ considerably

in the various countries where cultivation of these fruits occurs on a large scale.

Most of the important Citrus diseases are caused by fungi, but there is one, "canker," which is due to the bacterium *Phytomonas citri*. Citrus canker was first recognised as a really serious disease about 1914, but fortunately several of the producing countries are still free from it. Canker appeared some years ago in Florida and South Africa, but it has been entirely eradicated there ; as Fawcett points out, this is one of the most remarkable achievements in the history of plant-disease control.

Everyone is familiar with the green and blue moulds, caused by *Penicillium digitatum* and *P. italicum* respectively, which attack citrus fruits in transit to the consumer. Much work has been done recently on the means of preventing these and other rots, which is fully described in this book.

A new chapter on diseases in the growing tree due to deficiency and excess of inorganic constituents in the soil has been added in collaboration with A. R. C. Haas. Citrus trees are sensitive to deficiency of zinc, manganese and boron, elements that have only recently been shown to play an important rôle in plant nutrition.

The book is profusely and beautifully illustrated, some of the fruit rots being reproduced in natural colours. As an indication of the intensive study of citrus diseases in recent years it may be mentioned that the bibliography extends to forty pages of citations.

<div style="text-align: right">F. T. B.</div>

Biological Effects of Radiation. Vols. I and II. Edited by Professor B. M. Duggar. Prepared under the auspices of the Committee on Radiation, Division of Biology and Agriculture, National Research Council, Washington. [Pp. x + 1343, with numerous figures.] (New York and London : McGraw-Hill Publishing Co., Ltd., 1936. 70s. net.)

The effects of radiation on organisms, especially plants, are so many, both direct and indirect, that it is scarcely surprising to find that the editor of this admirable survey has utilised the services of no less than 46 contributors, including leading authorities on their subjects, to write the 43 papers that constitute this symposium.

The first volume treats mainly of what may be termed the physical background : the mechanism and measurement of radiant energy and photochemical reactions. Consideration of the direct influence of radiation on specific groups of substances such as vitamins, proteins, toxins, antibodies, etc., is followed by consideration of the effect on protoplast, organs and organisms.

Prof. Heilbrunn expresses the view that, despite some uncertainty in experimentation, the preponderance of opinion is that all types of radiation from visible light to gamma rays cause a substantial increase of permeability. Since the shorter wavelength radiations cause calcium to be set free from plant and animal tissues, and moreover the semipermeable character of the plasma membrane is due largely to the calcium ion, this may well be the cause of the change in permeability consequent upon radiation.

Apart from the fundamental necessity of radiation for the photosynthesis of green plants, the time-distribution, the quantity, and the quality of radiation have important influences, as manifested in the phenomena of

photo-periodism, in the pre-treatment of seeds, in the development of vitamins or the destruction of bacteria.

Dr. Garner gives a summary of photo-periodic phenomena, and other authors treat of the effects of radiation of different types upon green plants and the debateable problem of mitogenetic rays. There are also chapters on the effects of radiation on other organisms such as fungi and bacteria.

The final chapters deal with the subject of induced chromosomal alterations in animals and plants and the effects of radiation in inducing mutation.

It would be quite impossible to summarise the contents of this many-sided survey in the space at our disposal, and it must suffice to say that these volumes constitute a reference work on radiation which, alike on account of the papers themselves and the excellent bibliographies which accompany them, should not be omitted from any well-equipped biological library.

E. J. SALISBURY.

Narrative of an Investigation concerning an Ancient Medicinal Remedy and its Modern Utilities. The *Symphytum officinale* and its contained allantoin. By CHARLES J. MACALISTER, M.D., F.R.C.P. Together with an account of the Chemical Constitution of Allantoin. By A. W. TITHERLEY, D.Sc., Ph.D. [Pp. vi + 60, with 4 plates.] (London: John Bale, Sons & Danielsson, Ltd., 1936. 2s. 6d. net.)

IN this little book Dr. Macalister records his experience and that of others on the clinical uses of allantoin, the physiologically active constituent of the root of the Comfrey ; it is claimed that this substance induces cell proliferation which " renders it useful not only as a vulnerary but also because, as a promoter of leucocytosis it helps to establish immunities in some infective conditions." Experiments are also quoted which tend to show that allantoin solutions injected into bulbs of hyacinth or tulip induce more rapid growth of the bulbs. To quote the author's own words " this record has been written by way of opening the door for further investigations by those who may feel interested enough to avail themselves of clinical opportunities which, in my retirement, I do not possess."

P. H.

ZOOLOGY

Parade of the Animal Kingdom. By ROBERT HEGNER, Ph.D. Assisted by JANE Z. HEGNER. [Pp. viii + 675, with 743 figures.] (New York and London : Macmillan & Co., Ltd., 1935. 21s. net.)

IN this volume the authors have attempted to give a survey of the main divisions of the Animal Kingdom in a form suited to the needs of the reader who has no knowledge of scientific zoology. At the present day there is a tendency for the narrow specialisation that is necessary for research to be reflected in the popular exposition of biological subjects. It is well, therefore, that the general reader should sometimes be reminded of the infinite variety of animal life and of the long way to be travelled before we can assume the general application of biological laws formulated from the study of some single animal type. This "parade of the animal kingdom " with its abundant and, for the most part, excellent illustrations, is well fitted

to supply a necessary corrective in the mind of the large public which it will no doubt reach. It is perhaps not quite an ideal book of its kind, but we should probably not be far wrong in attributing some of its faults to the demands of its publishers rather than to the preferences of its authors. For example, the endeavour after lightness of touch sometimes degenerates into sprightliness of writing which not every reader will find attractive. To the same cause we must attribute the almost total absence of scientific names and the attempt to find or to manufacture an "English" name for every animal mentioned. It is a mistake to suppose that such names are always simpler or more generally understood than the technical names would be. How many zoologists, one wonders, could say at once what a "hellgrammite" is.

The unusually high level of excellence in the illustrations makes it unfortunate that one or two have been wrongly labelled. The "Edible Crab" figured on p. 107 is obviously not the species that is described under that name in the text, and the authors can surely not be responsible for the photograph of a cobra coiled round a mongoose on p. 617, or that of a gangetic dolphin which is said to represent the common porpoise on p. 639.

<div align="right">W. T. C.</div>

Pheretima (The Indian Earthworm). By K. N. BAHL, D.Sc., D.Phil. Second edition. The Indian Zoological Memoirs, No. I. [Pp. x + 85, with 35 figures.] (Lucknow : Lucknow Publishing House, 1936. Rs.1/8.)

TEN years ago, largely at the instigation of Prof. Bahl, the publication of the above series of Memoirs was commenced with the issue of the first edition of the present work. In the interval four other memoirs have appeared, under the general editorship of Prof. Bahl, and the supply of the first has become exhausted. Good though the first edition was, the second is an improvement on it in various respects. It has increased from 72 pp. to 85 pp.; the figures have been increased from 31 to 35 and the type and paper are distinctly better. While the number of figures has only increased by four yet actually five of the original ones have been omitted, nine new ones added and several others redrawn. The chapter on development has been cut out and it was the chapter least likely to interest the ordinary student in the laboratory. A new chapter on the receptor organs has been inserted to considerable advantage. While each chapter has been carefully revised and added to, there are perhaps two that call for notice. In the Introduction further remarks on Oligochætes in general are introduced, followed by Keys to the families of Indian Oligochætes, to the sub-families of the Megascolecidæ, to the genera of the Megascolecinæ and to the species of *Pheretima*. This should prove of considerable value to workers in Indian laboratories for whom the Memoirs are primarily intended. The chapter on Habits and Habitat has been noticeably expanded and rearranged and transferred from the beginning to near the end as Bionomics, Distribution and Relationships.

The author is to be congratulated on the improvement in the present edition and, as editor, on the series of which it forms so worthy a member.

<div align="right">C. H. O'D.</div>

The Red Centre : Man and Beast in the Heart of Australia. By H. H. FINLAYSON. [Pp. 146, with 52 plates and 1 map.] (Sydney : Angus & Robertson, Ltd., 1935. 7s. 6d. net.)

THIS book is a most careful and interesting account of the impressions and observations of one who, as Prof. Wood Jones says in his introduction, knows the fauna of the interior of Australia extremely thoroughly. Although much of the book is in narrative form, it is not a record of a single journey taken into a region hitherto unknown to the author, as is the case so often among travel books, but an account of the country tempered with such records of journeys as seemed to the author to be worthy of special narration. Much of the book will appeal especially to those interested in animals of Central Australia. There is also an account of the aborigines, but rather from the point of view of one who has frequently come into contact with them, and has employed them as servants, than from the anthropological viewpoint. The author devotes considerable space to the camel and some to the car, chapters which will be read by all who practise desert travel with considerable appreciation. Like most of those who have experience of the camel, Mr. Finlayson speaks with what almost might be described as affection of this strange ungainly beast, which in the pre-motorcar age was the only means of crossing arid country, and which still under many conditions is an inevitable and patient companion on many a slow and difficult journey.

In reading this book the reviewer was especially struck by the extremely human and personal outlook. Many such books are apt to become either an account of " high spots " in otherwise monotonous travelling, or technical accounts simplified as far as possible, and often losing whatever scientific value they may originally have had by such simplification. In *The Red Centre* we have a straightforward account, technical where necessary, but never over technical, of the ecological conditions in this particularly interesting country, and a general consideration of how man and beast get on there, either separately or in association. The effect of the stranger, white man, horse and camel, in relation to this environment forms the not least interesting part.

<div align="right">L. H. D. B.</div>

Plant and Animal Geography. By MARION I. NEWBIGIN, D.Sc. [Pp. xv + 298, with 39 figures.] (London : Methuen & Co., Ltd., 1936. 12s. 6d. net.)

ALL writers on the geographical distribution of living organisms emphasise the fact that it is impossible to consider the botanical and the zoological aspects in strict separation. Here, for the first time, the two are dealt with by an English author within the compass of one book. The student will be grateful to Dr. Newbigin for attempting this logical treatment, and the teacher of that ill-defined subject, biogeography, will appreciate something of the difficulties with which such a task must be beset.

Not least among these difficulties is the inadequate biological background of the majority of students of geography. A sound knowledge of botany and zoology is obviously a fundamental preliminary to any real understanding of the problems involved in a study of biogeography. The fact that Dr. Newbigin devotes whole chapters to the classification of plants and animals and to the plant's response to its environment, shows that she

knew she could not take for granted even the most elementary knowledge: and one wonders what the geographer pure and simple will make of the necessarily "potted" botany and zoology she conscientiously endeavours to supply.

Space has had to be sacrificed to such topics that else might have been used to greater profit. Some of us may think, for instance, that the ecological aspect of zoogeography is treated far too summarily in this book. Admittedly, animal ecology is a comparatively new science; but already it is evident that ecological study will eventually offer clues to much that is otherwise insoluble in the problem of animal distribution.

On the other hand, the question of soils gets admirable treatment at the hands of a collaborator, Miss Margaret Dunlop; and here is a topic generally neglected in text-books of the kind.

One of the book's best qualities is its determined rejection of the static view. Things are described, not only as they now appear, but also as they may have been and as they may yet be. The narrative moves, and the reader sees the world change before the eyes of his imagination. When one realises that its distinguished author died before this, her last work, was ready for the press, one must admire what has been achieved. To the able editing by Miss Florence Newbigin and Prof. Fleure the book, as it now appears, doubtless owes much of its coherence.

D. L. M.

Mechanistic Biology and Animal Behaviour. By THEODORE H. SAVORY, M.A. [Pp. xv + 182, with 7 figures.] (London: Watts & Co., Ltd., 1936. 7s. 6d. net.)

THIS is a vigorous and well-written defence of the mechanistic standpoint in the study of animal behaviour. Many points are well taken, and much of the criticism of old-fashioned views is acute and well founded. But the book is one-sided, and modern non-mechanistic views are not considered. Thus, to take one example, Doflein's interpretation of the ant-lion's behaviour in terms of reflexes is discussed without reference to the fact that W. M. Wheeler, Bierens de Haan and others have shown Doflein's interpretation to be inadequate. There is no mention of two most important developments in the study of behaviour, Gestalt theory and the theory of "Umwelt." Much space is given to Loeb's theory of tropisms, which has now only historical interest. The professional student will find little of value in the theoretical discussions; all this ground has been gone over twenty years ago.

Mr. Savory's point of view seems to have been determined largely by the specialised character of his favourite objects of study, the Arachnida. They show instinctive behaviour in its most stereotyped and rigid form, and lend themselves less badly than most other animals to a mechanistic interpretation. The most valuable parts of the book are precisely those relating the author's own highly interesting observations and experiments on the behaviour of spiders; his account, for example, of the varying behaviour of the wolf spider with respect to her cocoon, according to the state of her ovaries, brings out many points of great significance.

A priori theories, whether mechanistic or vitalistic, have been the bane of the study of animal behaviour. We want far more direct observation and experiment, *unbiassed by theory*, before we can hope to build up an adequate

science of behaviour.   Here Mr. Savory can help us more effectively than by stirring up old controversies.

<div align="right">E. S. Russell.</div>

Laws and Regulations in Summary concerning Salmon and Trout Fisheries.   Compiled by T. E. Pryce-Tannatt.   Rapp. et Proc.-Verb., Conseil Internat. pour l'Explor. de la Mer, Vol. XCVI.   [Pp. 37.]   (Copenhagen : Andr. Fred. Høst & Fils, 1936.   Kr.2.00.)

This paper gives a very useful account of the laws relating to salmon and trout fisheries in Denmark, England and Wales, Finland, France, Germany, the Irish Free State, Latvia, the Netherlands, Norway, Poland, Portugal, Scotland and Sweden.   All the summaries are in English save those of France and Portugal which are in French.   The laws of practically all countries recognise the importance of protecting spawning beds and immature fish, allowing a sufficiency of adults to reproduce to maintain the stock, the control of obstructions such as dams and weirs, so that fish can reach the headwaters of rivers, and the prevention of pollution.   As, however, many other interests are affected by the control of rivers, the laws of most countries represent a compromise and the variations from country to country are worthy of notice.   Other features of interest are the differences between English and Scottish laws and the complexity of the French code.

This summary should be of interest, not only to those concerned with the administration of freshwater fisheries, but also to any angler who contemplates a fishing holiday abroad.

<div align="right">F. T. K. P.</div>

The Interpretation of the Zones on the Scales of Salmon, Sea Trout and Brown Trout.   By T. H. Järvi and W. J. M. Menzies.   Rapp. et Proc.-Verb., Conseil Internat. pour l'Explor. de la Mer, Vol. XCVII.   [Pp. 63, with 82 figures.]   (Copenhagen : Andr. Fred. Høst & Fils, 1936.   Kr.4.00.)

That fishes of the genus *Salmo*, like many others, bear on their scales a summary of their life histories has long been known but a good deal of experience is still necessary before the maximum amount of reliable information can be obtained from scale reading.   The authors of this paper trace the development of the salmon scale through each stage of the growth of the fish ; its formation in the fry, the laying down of ridges, widely spaced in summer, close together in winter, the change which occurs when the fish migrates to the sea, the erosion which precedes spawning and the repair and consequent formation of a spawning mark if the fish survives.   The text is accompanied by an excellent selection of photographs which clearly illustrate the features described and show the differences between Baltic and North Sea salmon.   Abnormalities such as summer checks, false spawning marks, regenerated and perforated scales are also described.   Sea trout and brown trout are discussed in the same way.

The paper also includes a most useful list of common expressions for the various stages of salmon and of the popular names of the members of the Salmonidæ, in all the languages of Northern Europe.

<div align="right">F. T. K. P.</div>

A Review of Fishery Statistics in relation to Wholesale Index. By W. NELLEMOSE. Rapp. et Proc.-Verb., Conseil Internat. pour l'Explor. de la Mer, Vol. XCVIII. [Pp. 72, with 38 figures.] (Copenhagen : Andr. Fred Høst & Fils, 1936. Kr.4.00.)

IN the *Bulletins Statistiques* of the International Council the value of the fish landed in the different countries are still converted into shillings ("current shillings") at the average rate of exchange, but nowadays this conversion has little meaning in comparing, between the countries, the value to the producers of the produce of the fisheries.

Captain Nellemose has taken advantage of the fact that most countries publish a "wholesale index" to weight the value of the fisheries in each country by this index. He has carried through the laborious task of recalculating, in terms of a "pre-war shilling," the values published in the *Bulletins Statistiques*. He fully bears out his claim that there is greater consistency between quantities and values in these "pre-war shillings" than between quantities and values in "current shillings," and the former also gives some indications of the changes in demand from year to year.

He promises a further review of the value of the fisheries in the even more difficult years since 1933. It is to be hoped, however, that he will simplify his diagrams by omitting the less important data from which he calculates his weighted values.

C. F. H.

Reports of the Proceedings of the Special Plankton Meeting held May 27th, 1935, at Copenhagen, and of the Special Meeting held at Videnskabernes Selskab's Premises on May 31st, 1935. Rapp. et Proc.-Verb., Conseil Internat. pour l'Explor. de la Mer, Vol. XCV. [Pp. 61, with 28 figures.] (Copenhagen : Andr. Fred. Høst & Fils, 1936. Kr.3.00.)

THE most important papers in this volume are by Mr. F. S. Russell and by Prof. A. C. Hardy. Mr. Russell's paper deals first with the distribution of plankton animals, and he has drawn up two very important tables, based on a survey of the literature and on the collaboration of specialists, showing (Table I) the subdivision into broad geographical areas of the known species of the more important plankton groups, and (Table II) a list of the plankton animals, characteristic of certain types of seawater, which may be used as indicators of the presence of such water. He points out the limitations in using these plankton-indicators, and emphasises the importance of a fuller knowledge of the life histories of plankton animals, and even of racial differences between the same species in different regions. Finally he deals briefly with the environmental relationships of the plankton animals, their metabolism and physiology, and gives, as usual, a very full list of references.

Prof. Hardy gives an account of some of the results of three years' work with his continuous plankton recorder. He describes the difficulties he has encountered in using this method of surveying the plankton along certain steamship routes in the North Sea. The chief of these are the vertical migrations of the plankton, which must seriously affect the catch of an instrument towed at a constant depth, and the clogging of the meshes of the silk collector when the plankton is dense. Such clogging must cause a temporary slowing down of the rate of filtration, which will affect the capture of the larger and more active organisms to a greater degree than

the small and less active, and to some extent spoil the sampling. A series
of charts are given which show vividly enough the possibilities of this method
of research; they could hardly be clearer or more instructive.

C. F. H.

Evolution and Heredity. By Charles Edward Walker, D.Sc.,
   M.R.C.S., L.R.C.P. [Pp. ix + 222, with 20 figures.] (London:
   A. & C. Black, Ltd., 1936. 6s. net.)

Those who desire information on heredity and evolutionary theory should
read this book with caution. A short introductory chapter is followed by
an account of cytology which, unfortunately, is not up to date. The cyto-
logical advances of the last ten years have profoundly modified our conception
even of the elements of mitosis and meiosis, but this description has been
written without regard to them. We are actually told that the chromosomes
break up from a continuous "spireme," while there is no suggestion that
they split longitudinally prior to the mitotic metaphase. Meiosis is treated
as a single cell-division followed by a mitosis, instead of a process involving
two divisions of the nucleus with but one division of the chromosomes. As
the author makes no attempt to describe chiasma-formation, he does not
encounter the difficulty that the separation at some loci will be reductional
at the first meiotic division and equational at the second, and at other loci
the reverse. His mention of segmental interchange as a basis for crossing-
over seems, in fact, in the nature of a passing allusion (p. 88), so vague as
to be quite unintelligible to those unacquainted with the facts. However,
he states that the opportunities for such changes are represented as "most
easy and numerous" in the latest descriptions of it (here follows a list of
references, the most recent of which is 1926).

The genetic account of crossing-over (p. 87) is not more helpful than
the cytological. Students should of course be introduced to this process
by a study of the back-cross, not by the results of interbreeding the F_1
generation to produce F_3 (presumably F_2 is intended). It is, perhaps,
unfortunate that the examples chosen are sex-linked characters in *Drosophila*,
though sex-linkage is not described till later in the book. It is then remarked
that in birds "the breeding experiments and microscopical observations at
present do not agree" (p. 92); but the most recent work referred to was
published in 1912!

As no account is given of phenotypic variation, the discussion of pure-
lines and of the genotype is greatly hampered. Furthermore, the whole
concept of factor-interaction is omitted.

The book contains much loose phraseology and many mis-statements.
It is not characters, but the factors which control the characters, which are
inherited; so that characters are not distributed as units to the offspring,
as here stated (p. 181). It is actually maintained that "Mendelian char-
acters" are all small and unimportant: on the most superficial consideration,
a concept difficult to reconcile with the fact that they often kill the individual
which manifests them. Had it been stressed that if the F_2 generation is
more variable than the F_1, the inheritance involved must be particulate
(that is Mendelian), and that this forms a general test of the hereditary
mechanism involved, much discussion on "blending" could have been
avoided. Furthermore, there is confusion between the blending of characters
and of genetic factors, although these are ideas which it is imperative to

keep strictly apart, while the term "mutation" is not clearly defined and is variously used. The argument that the number of genes is insufficient if all inheritance is Mendelian, apparently assumes that each gene controls a single character only, and is therefore invalid.

It is extraordinary that any account of the chromosome basis of heredity could now be written in which the modern evidence derived from combined cytological and genetic study is entirely ignored. Some reference to the work of Painter and Muller, of Stern, or of Dobzhansky, is essential if the matter is to be discussed at all.

In general, the author quotes too much from his references instead of explaining them. However, there are a few valuable passages, as that on p. 164 in which it is pointed out that evolution shows no sign of intelligent direction ; but the book is not one to be read uncritically.

E. B. F.

MEDICINE

Muscular Exercise. By M. GRACE EGGLETON, M.R.C.S., L.R.C.P., M.Sc. [Pp. x + 298, with frontispiece and 68 figures.] (London : Kegan Paul, Trench, Trubner & Co., Ltd., 1936. 10s. 6d. net.)

THIS book, as the author says in the Preface, " endeavours to give a coherent and fairly detailed account, in non-technical language, of all the changes which occur in the body during muscular exercise. It presupposes no knowledge of general physiology in the reader other than that acquired in ordinary life——" Her main object, she says, has been " to help to dispel the atmosphere of magic and mystery which still surrounds the human body."

Consideration of the bodily changes accompanying muscular exercise leaves very little of human physiology untouched. It can begin, as the author does, with things familiar to all ; if we run fast we cannot run for long ; when we stop we are out of breath and hot, and our hearts are thumping. The physiological account of these events leads naturally to more recondite subjects : the carriage of oxygen by the blood, the maintenance of normal acidity in the body, the regulation of temperature, the supply of fuel to the muscles, and, most important and difficult of all, the control of movement. Other chapters deal with the intimate nature of the muscle machine and the factors limiting the force and speed of movement. Nothing has been scamped, and difficult and controversial points have been very skilfully dealt with. The last chapter contains an interesting attempt to explain what constitutes an athlete.

Mrs. Eggleton's book can be warmly recommended as an introduction to physiology for those who are expected to learn something about it and even more to anybody, whatever his profession, who professes to be educated. One of the great obstacles to the health and happiness of mankind is " the atmosphere of magic and mystery that still surrounds the human body."

A. D. R.

An Enquiry into Prognosis in the Neuroses. By T. A. Ross, M.D., F.R.C.P. [Pp. viii + 194, with 1 plate.] (Cambridge : at the University Press, 1936. 10s. 6d. net.)

THE author of this interesting book is a psychiatrist, at one time medical director of the Cassel Hospital for functional disorders, "Swaylands." The enquiry is concerned with the follow up (after history) of patients who have

been treated in this institution. The subject is introduced by an account of the methods of treatment employed there. These comprise chiefly "persuasion" and analysis, but not psycho-analysis in the Freudian sense; hypnotism is only rarely used; of chief value is the general influence of institutional life, the association and co-operation with other patients, a form of occupational therapy teaching the patient to "stand on his own feet."

After leaving the hospital the patients were written to at intervals up to five years. Replies were either not received or received from the patient or from friends. Obviously it was often a matter of guess-work to evaluate these replies; statistical treatment of the results were necessarily subject to a high degree of error. The terms "well", "cured" and "improved" are vague and of no mathematical meaning or value. In neurosis there is no yard-stick for measurement. Often the receipt of an enquiry occasioned a relapse of the patient's condition, or the replies from patient and from friends were contradictory.

In spite of these many difficulties, tables of results are given. Out of 1186 patients 547 (45 per cent.) reported "well" after one year; out of 1043 patients 421 (40 per cent.) were "well" after three years, but 510 were lost sight of; out of 850 patients 290 (34 per cent.) were "well" after five years and 502 were lost sight of. These results should be regarded as satisfactory. The author adds a note that there is no claim that the patients who reported "well" remain "well" all the time. There were also patients who reported "well" or "improved" but who did not regard their cure or improvement as resulting from any treatment they had received at the institution. Details of individual cases are set out over some eighty pages and will be of great interest and value to psychiatrists and other physicians who are working at this group of functional nervous disorders.

P. J.

HISTORY OF SCIENCE AND PHILOSOPHY

The Study of the History of Science. By GEORGE SARTON, S.D., A.C.I., L.H.D., LL.D. [Pp. vi + 75, with 6 figures.] (Cambridge, Mass.: Harvard University Press; London: Humphrey Milford, 1936. $1.50.)

DR. SARTON's inaugural lecture on the History of Science in Harvard University in 1935 is here expanded into a stimulating little book with a classified and critical bibliography. It is emphasised that "accuracy is as fundamental in the historical field as in the scientific one, and has the same meaning in both," and the author elaborates this in his characteristic way. The book contains many striking sayings. "Many scientists, like Hannibal, know how to conquer but not how to use their victories." Dr. Sarton, at least, does not belong to this class, since he has shared in this book the wisdom he has gathered in his own years of effort in this field. His latest book is one which will be helpful to all who are interested in the teaching or study of the history of science.

J. R. P.

The Study of the History of Mathematics. By GEORGE SARTON, S.D., A.C.I., L.H.D., LL.D. [Pp. vi + 113, with 2 figures.] (Cambridge, Mass.: Harvard University Press; London: Humphrey Milford, 1936. $1.50.)

THIS is a short book in three separate divisions. The first, expanded from an inaugural lecture delivered by the author at Harvard University, gives

his general outlook on the history and development of Mathematics, but does not really go into the actual methods of study of the subject.

The second part, entitled "A note on the study of the history of Modern Mathematics," does give very useful guidance to those who have to study the history of a branch of Mathematics. Very wisely the author accepts from the outset the position that such a study is for the professional and he goes to the root of the matter when he says that "in short, the history of modern Mathematics should be taught by mathematical teachers in the course of their ordinary teaching, while the history of older Mathematics can be properly taught only by a specialist, who must be as much of a historian as of a mathematician, if not more."

The most valuable part of the book is undoubtedly the bibliographical section at the end. Here, however, the absence of Applied Mathematics and particularly of Astronomy, the early history of which is hardly distinguishable from that of Mathematics, seems regrettable, the more so that in the Appendix (p. 101) the author pays a tribute to the importance of Applied Mathematics and has included Applied Mathematicians and Astronomers in his list of biographies.

There are curious omissions. "Academies" are dismissed in five lines and the (British) Mathematical Association is left out, while the corresponding American body is put in. Although the author says (p. 59) that "it is out of the question to deal separately with all the journals which are wholly or partly mathematical," it should not have been impossible to have given a list of the more important ones. As it is, there is no hint of the seven or eight publications in which almost the whole of the work of British mathematicians is to be looked for.

The selection of nineteenth-century mathematicians may give the reader a few surprises, in spite of the disarming candour of the preamble. We may agree not to quarrel over the inclusion of some doubtful names, but why omit, for instance, Delambre, Mathieu and Boussinesq?

Apart from these lesser deficiencies, the book breaks new ground and will be helpful to many by indicating the more immediate sources to be consulted.

L. N. G. F.

The Identity Theory. By BLAMEY STEVENS. Second edition. [Pp. xvi + 247, with 29 figures.] (Manchester: Sherratt & Hughes, 1936. 7s. 6d. net.)

THE difficulty of assessing this book begins with the task of discovering its purpose. By implication one gathers that it aims at presenting a theory of the whole of existence, and so includes what we ordinarily call psychology and natural science. It is mainly occupied with the problems of physics, however, but for some reason it does not use the ordinary language of physics, the positive electron being called the "antron," the neutrino, the "ditron," etc. The theory takes its name from the postulate that the three "primal entities," space, time and inertia, are "all complete measurements of the same thing"—namely, substance, or ether. There are therefore relations between them which are called "identities," and the various expressions of these identities constitute the physical laws of nature. The author evidently has considerable facility in the manipulation of mathematical formulæ, and a great deal of the book is occupied with an expression of familiar mathe-

3 E

matical physics in terms of the identity postulate. When one looks for what has been achieved, however, it appears that there is nothing definite to be found beyond a change of language : to call the ordinary relativity relation between space and time an "identity" instead of a "relation" does not take one much further. The author is unfortunate in lacking the first essential of successful exposition—namely, the ability to put himself mentally in the reader's position and to describe his theory in a readily apprehended form. The presentation is cryptic and disordered, and while it is possible that the author has clear ideas, he does not succeed in transferring them to the ordinary physicist. Even a statement of what the theory adds to previous knowledge is lacking. We can only hope that others who may read the book will derive more benefit from it than we have done.

H. D.

MISCELLANEOUS

Practical Photomicrography. By J. E. BARNARD, F.R.S., F.Inst.P., F.R.M.S., and FRANK V. WELCH, F.R.M.S. Third edition. [Pp. xii + 352, with 23 plates and 121 figures.] (London : Edward Arnold & Co., 1936. 21s. net.)

THE third edition of this well-known book is thoroughly practical in its out-look, but theoretical aspects of the subject are adequately treated where necessary. An admirable balance has been kept in the space devoted to the different parts of the photomicrographic equipment, the microscope, illuminant and camera receiving equal attention. Such special processes as the photography of metals, microcinematography and the use of infra-red rays, are dealt with, and there is an up-to-date outline of the technique of photography with ultra-violet light.

In a final chapter photomicrographs of a variety of objects are reproduced, accompanied by a detailed account of the methods by which they were made. While they are, it is claimed, "fair examples of what may be expected from an object of average quality," they are of excellent quality, and appear to have lost little in their reproduction as plates. There are some particularly fine examples of photographs taken with ultra-violet light.

To the expert the volume will prove a valuable work of reference, while the beginner, either in microscopy or photography, or both, should find little difficulty in mastering the details of photomicrography with this book as his guide.

Although a treatise on photomicrography, the majority of users of the microscope might study it with profit. Reference, for example, to the plate showing the effects of tube length on the image, to that in which the effects of condenser aperture are illustrated, or again, to the examples illustrating accurate and faulty centring of the condenser, should act as a corrective to slovenly handling of the microscope. The chapter on colour screens should also prove of value to the microscopist.

F. W. J.

Practical Stereoscopic Photography. By J. MOIR DALZELL. [Pp. xvi + 224.] (London : The Technical Press, Ltd., 1936. 10s. 6d. net.)

IN adducing reasons for the lack of interest taken in the stereoscopic camera in Great Britain, Mr. Dalzell suggests that many of us suffer from physical

and psychical defects which prevent us from appreciating the fused images of the stereoscope. This suggestion is at least open to argument : not only is the process popular abroad, but also if any reliance can be placed on the psychological mechanism of over-compensation such persons should be the first to take up this branch of photography. A more reasonable answer would probably be that pictorial photography is predominant in this country, and that its exponents have been too concerned with the principles of the graphic arts to concern themselves with the possibilities of the stereoscope.

Mr. Dalzell's book manages to combine effectively a great number of delicate classical allusions to the Greek and Roman Pantheon with an exceedingly comprehensive course of instruction in the art and craft of stereoscopic photography, and his work deserves to become the standard reference book on this subject in English. No part of the subject has been neglected, and those " ordinary " photographers who read the book will be surprised at the number of formulæ, etc., which they have never come across previously.

If one may find a fault with the whole book, it is that the author is so enthusiastic about his subject that he finds it hard to say a kind word for the " one-eyed " camera. In the reviewer's opinion, too, he is over-optimistic regarding stereoscopic cinema films. There is no doubt that some of the processes mentioned are extremely satisfactory in the experimental laboratory, but it is very doubtful whether cinema proprietors would seriously consider rebuilding to deal with some of the technical requirements of the processes involved.

<div align="right">P. C. S.</div>

Kenya : Contrasts and Problems. By L. S. B. LEAKEY, M.A., Ph.D., F.S.A., F.R.A.I. [Pp. xvi + 189, with 8 plates.] (London : Methuen & Co., Ltd., 1936. 7s. 6d. net.)

ON KENYA, as one born and bred there, Dr. Leakey can speak with authority, and as an anthropologist with understanding. In this book his aim is first to present the reader with a picture of conditions in as full an account as his space will allow of the country, its peoples, white and black, and their social and economic circumstances, and then to state in outline the problems which arise from these conditions, as well as from the influx of a large number of natives of India. Finally he sums up and weighs tendencies as he sees them. It will be obvious to anyone who has followed recent history in Kenya that Dr. Leakey is not lacking in courage.

Although critical, and at times severely critical, the author is neither bitter nor partisan. Anyone who desires a clear view of the shortcomings of European relations with the native, will find it here. But on the other hand, the author is not blind to the merits of the Administration and the missionary ; nor is his vision obscured by an indiscriminate admiration for native ways. His attitude is one of well-balanced common sense. His discussion of the vexed question of initiation may be commended as a discerning appreciation of what might be conserved in native custom to the advantage of the native. His attitude to native religion, however, which some would preserve, is an example of his recognition that European contacts break down native custom irrevocably. Native religion, he points out, is meaningless when family life goes, while the native wants to know what the white man's religion is like.

In regard to the future, Dr. Leakey, whose book was written before the

publication of the recent report on the finances of Kenya, sees the weaknesses of the white settlers' desire for a greater share of the government ; but then he does not think Kenya is a white man's country. This influences his final judgment that the future lies in co-operation between white and black and not in domination by the former. Unless this co-operation is attained a great struggle lies ahead.

E. N. F.

Sacrifice to Attis : A Study of Sex and Civilisation. By W. A. BREND, M.A., M.D., B.Sc. [Pp. vi + 350, with frontispiece and 2 figures.] (London : William Heinemann, Ltd., 1936. 10s. 6d. net.)

THE title of this interesting book refers to the horrible self-mutilations of priests in the ritual of the Phrygian god Attis, described by Sir J. Fraser in the *Golden Bough*, and seems perhaps rather strained to fit into the real subject of the book, which is the diminishing birth rate, a serious and urgent factor in the survival of our race.

It is estimated that if the present trend of the birth rate continues the British Empire, other than native people, will disappear within a century.

In this study of sex and civilisation Dr. Brend searches for the causes that have operated to bring about this deplorable condition.

Artificial restriction of the family is a new feature in the history of mankind, and has had more far-reaching effects than any of the great past movements which have influenced human development. It is not generally realised that the total effect of a fall in birth rate is greater than its immediate result in reducing numbers ; it has yet a further influence, obvious only some twenty years later, in diminishing the number of potential mothers. The effect is likely to be accelerated by further social changes, for there are large classes to whom the use of contraceptives has not yet spread and who are being actively encouraged to use them. The present use is largely in inverse ratio to the degree of wealth.

The assumption that restriction of family is due to economic pressure (inability to rear and feed a family) is true only to some extent among the working classes. Had economic pressure been a serious factor, the principle would have started with the poorest and need not have affected the wealthy at all. It is a fact that countries with low standards of living (Japan, Italy) have higher birth rates than wealthier countries (France, Britain).

Dr. Brend believes that the declining birth rate is part of a larger struggle " rooted in man's primitive terror of the civilisation he has created." He thinks that this terror has been assisted by, if not originated in, the conception of the Christian church that sex is inherently sinful, and, after analysis of the lives of St. Paul, St. Augustine and the arguments of early Christian fathers, he makes the revolutionary, and in our opinion unwarranted, generalisation that the fundamental purpose of Christianity is to give those who suffer from morbid emotions towards their parents an opportunity of relieving their sense of guilt.

The remedy, according to the author, should derive from the societies of ancient Greece and modern Soviet Russia ; but the prescription is vague —" revision of our attitude to sex," " recognition of rights and responsibilities of the young," " freer intercourse between the sexes," " the conception of sin in relation to sex should cease to be taught," " marriage should be facilitated by extension of the grounds for divorce."

Surely it might be difficult to administer this advice ?

The frontispiece, representing a castration clamp, has antiquarian interest but only slight contact with the main thesis of the book.

P. J.

Nodule Implements in the Bournemouth District. By E. HUGH
KITCHIN, M.A., M.B. [Pp. v + 41, with 32 figures.] (Cambridge :
W. Heffer & Sons, Ltd., 1936. 4s. 6d. net.)

ONE of the most distinguished of modern French archæologists, the late Prof. E. Capitan, who was by no means an indiscriminate advocate of the eolith, always used to maintain that in the last resort the best test of an artificially fashioned flint was the subjective judgment of experience. Although Dr. Kitchin in this account of the primitive stone implements he collected in the gravel pits of the Bournemouth district devoted attention to the question of the criteria of natural and artificial fracture in flint, his classification of a previously undescribed form which he termed " nodule implement " was based on his experience in handling a large number of specimens agreeing in certain characters, rather than upon evidence which would immediately and intrinsically carry conviction when pointed out. The technique is simple, and, in fact, as *ex hypothesi* the nodule was selected on account of its suitability for its purpose, the shape is of more importance than the working—usually a sharpening blow, assisted, it may be, by a truncating, or a shaping blow. The axis of the implement, however, is always oblique.

No evidence is available for dating the Bournemouth specimens ; but two similar nodules come from East Anglia, one from Bawdsey, Suffolk, from the subcrag, and the other from the Forest Bed flints at Cromer.

E. N. F.

Man makes Himself. By V. GORDON CHILDE. [Pp. xii + 275, with
12 figures.] (London : Watts & Co., 1936. 7s. 6d. net.)

IN *Man makes Himself* Prof. Childe surveys the whole range of archæology in the light of his recent dictum that the divisions of prehistory, the stone, bronze and iron ages, can no longer be regarded as periods in a chronological sense, but must be looked upon as betokening economic revolutions, comparable with the industrial revolution of the early nineteenth century, by which the whole pattern of civilisation was rearranged. Thus the domestication of animals and plants made possible the settled life of the village community, the introduction of the use of metals demanded specialisation in production in a number of industries and opened the way to organised trade and exchange in an urban civilisation, and so forth. The argument is given a topical interest by being turned against the current pessimism which regards the development of modern civilisation as a regress rather than a progress—a view which, Prof. Childe points out, is usually based upon the insufficient evidence of a limited period in a restricted area, and evaluated in a personal equation. Prof. Childe offers as an objective standard, which will serve to eliminate the personal equation in the estimation of progress, the biological principle of the survival of the fittest, advance being gauged by capacity to enable a greater number of the species to survive. Prof. Childe's book is a demonstration of how the economic revolutions of pre-

history did, as a matter of fact, bring this about. He argues his case with a brilliance and lucidity which makes every point tell.

<div align="right">E. N. F.</div>

Out of the Night : A Biologist's View of the Future. By H. J. Muller. [Pp. 160.] (London : Victor Gollancz, Ltd., 1936. 4s. 6d. net.)

WE will not throw this book into the fire as Prof. Haldane suggests, nor shall we dismiss it as mere phantasy ; it is rather a torrential procession of ideas and suggestions, often Wellsian in daring but compelling by their reasonableness and convincingly practicable nature.

In his preface the author states the central theme of the book is " an attempt to show that for a continuance of material, cultural and biological progress in the human race a thoroughgoing economic and social change to a more truly co-operative basis of society, together with the regeneration in human motivation attendant upon this, is a prior necessity." The first chapter deals with a general scheme of organic evolution ingeniously symbolised by a cord each yard of which stands for 10,000 years and which stretches from Boston to the centre of the private desk of J. P. Morgan in his office in New York ; in this scale a human generation would occupy less than $\frac{1}{8}$th of an inch ; a cross-section of the cord about the size of an ordinary aspirin tablet would represent the total heredity substance of both eggs and sperm for the next generation of mankind, about two thousand million in number. Discussing industrial evolution, the contest of man with the outside world, the author refers to the elimination of old and introduction of new methods and technics, the production of undreamed-of artificial substances and the employment of as yet quite unknown sources of energy from sunlight, wind, waves, tides, rain, subterranean heat, sources extra mundane, and interplanatory, and probably enormous forces from the interior of the atom nucleus ; he foresees a liquidation of our present resources and the release of human energy for higher limits of accomplishment. The important part of the book is concerned with a new science of eugenics and methods for improving and maintaining a better race in a better social environment. The encouragement of motherhood and methods of relief for child-bearing, including the uses of contraception and abortion, are discussed ; the questions of ectogenesis and artificial insemination are seriously considered, the selection of parents depending on " comradeliness and intelligence." So far research on this subject has scarcely begun. It is conceivable that seminal cultures can be made and maintained for long after the death of the individuals from which they had been taken, and the author reflects how fortunate we should be had such methods been in existence in time to have enabled us to secure living cultures from some of our departed great. This is a most entertaining book.

<div align="right">P. J.</div>

Glances at Industrial Research. By E. R. Weidlein and W. A. Hamor. [Pp. x + 238, with frontispiece and 25 figures.] (New York : Reinhold Publishing Corporation ; London : Chapman & Hall, Ltd., 1936. 13s. 6d. net.)

THE principal object of this book is to draw attention to the place of the Mellon Institute of Industrial Research, Pittsburg, in the field of technology,

and an account is given of the pioneer work of Robert Kennedy Duncan in connection with the founding of the Institute's Industrial Fellowships, particular stress being laid upon the advantages to manufacturers which are to be gained from associate research. The later sections of the book deal with some examples of research achievements, the professional aspects of industrial research, the scientific use of literature, the control of research, and publicity. An appendix is included which summarises the progress of American chemistry since the outbreak of the Great War.

There must be many in this country ready to welcome a book designed to familiarise the general public, and in particular the business men of to-day, with the objects and very far-reaching effects of industrial research. It is doubtful, however, whether their desire will be satisfied by the volume under review. To some dispassionate, scientifically minded readers the literary style will appear decidedly flamboyant, at times almost to the point of physical discomfort, whereas to the lay mind it must indeed seem strange that, with so much resource and efficiency abroad in the world, Utopia is still so far distant. Seldom can so many superlatives have been crowded together in the space of one small volume, and it is a pity that a work which contains so much of interest could not have been presented in a more temperate manner. As it is, the subject matter loses much of its appeal through overstatement, and verboseness. Doubtless the authors have some good reason for choosing to use one or more long words in place of one short word, but it is difficult to see what they hope to gain in cogency by this means. The "captions" which precede each chapter would appear better suited to a film treatment of the subject, than to a literary presentation. Some of these are amusingly naïve, perhaps the high-water mark in platitudes being reached in the introduction to the index, which states that "An index should be like a compass; directive to the point one would reach"!

H. W. CREMER.

Statistical Research Memoirs. Vol. I. Edited by J. NEYMAN and E. S. PEARSON. [Pp. viii + 161, with numerous figures.] (Issued by the Department of Statistics, University College, London, 1936. 15s. net.)

THIS new series of Memoirs continues the tradition of the publication of Memoirs, established by Karl Pearson, from the Applied Statistics and Eugenics Department at University College. But, whereas the Drapers' Company Research Memoirs included theory of statistics and the results of statistical investigations in a variety of allied fields, the present series is to be confined to statistical theory. The papers published are to be those containing the researches of the members of the Department of Statistics at University College, in this special branch, which may be described as "the establishment of a theory of statistics on a level of accuracy which is usual in other branches of mathematics" (Foreword by the Editors).

The present volume includes two papers by J. Neyman and E. S. Pearson on, "Contributions to the theory of testing statistical hypotheses (I). Unbiassed critical regions of Type A and Type A_1," and "Sufficient statistics and uniformly most powerful tests of statistical hypotheses." The first is concerned with methods which may be used when choosing a critical region for testing a certain hypothesis when no uniformly most powerful test exists. The second is concerned with problems relating to the co-existence or not

of uniformly most powerful tests of statistical hypotheses and sufficient statistics.

In addition there is a paper by P. P. N. Nayer, who is concerned with the problem of how k groups of samples of a quantity x can be used to test the hypothesis that these samples have been drawn from populations with a common standard deviation.

B. L. Welch has a note on an extension of the L_1 test. Palmer O. Johnson and J. Neyman deal with tests of linear hypotheses and their application to some educational problems. As an instance, they consider the problem of comparing the academic achievement of students of opposite sex, when the data (fictitious) consist of " Gains in Biology " of male and female students, classified according to social level and preparatory school.

P. V. Sukhatme has a paper " On the analysis of k samples from exponential populations with especial reference to the problem of random intervals." Robert W. B. Jackson has a paper on " Tests of statistical hypotheses in the case when the set of alternatives is discontinuous, illustrated on some genetical problems."

A very specialised, but very interesting volume. Good luck !

E. C. RHODES.

Choosing a Career. By E. D. LABORDE, Ph.D. [Pp. x + 312.] (London : William Heinemann, Ltd., 1936. 3s. 6d. net.)

THE author is Careers Master at Harrow School, a recent and valuable addition to public school organisation, and it should be of interest and service to the public to be informed of the considerations which he thinks should be had in mind in advising a boy on the choice of a career. He writes with good sense and good feeling, and with much, perhaps with most of what he writes the reader will find himself in agreement. If, to some extent, we feel inclined to qualify our acceptance of his views, it is because he seems at times to deal too much in generalisations, and himself to accept certain generalisations the truth of which is very questionable. For instance, when he writes that a boy should ask himself, " Am I better at handling men or things ? " we would express a doubt whether this is the sort of question that a boy would or should commonly ask himself or that should be asked for him. Again, when he goes on to say that the public schools have evolved, through their system of games, a method of selecting boys with the quality of leadership, we may feel the inadequacy of this test ; and when he says of the leader that " his extrovertial nature precludes him as a rule from being a thinker " our doubts will be intensified. A good deal of this earlier part of the book is reminiscent of the psychological tests which are so popular nowadays, tests which are indeed valuable to a limited extent, but cannot weigh the imponderable. Mr. Laborde is on surer ground when he attaches most weight to personality. Those who, like him, have to advise boys, or, like the present reviewer, young men and women on the choice of a career, know that it is not this or that type that they have to advise, but this or that individual.

The latter and larger part of the book consists of a useful summary of methods of business organisation and of classified careers and the training required for them.

L. S.

BOOKS RECEIVED

Mathematical Snack Bar. A Collection of Notes and Results. By Norman Alliston. Cambridge : W. Heffer & Sons, Ltd., 1936. (Pp. viii + 155.) 7s. 6d. net.

Interpolation and Allied Tables. Reprinted from the Nautical Almanac for 1937. London : H.M. Stationery Office, 1936. (Pp. 48.) 1s. net.

Am Fernrohr. Ein Führer bei der Betrachtung des Sternhimmels mit blossem Auge und kleineren Instrumenten. By Dr. Friedrich Becker. Second edition. Berlin and Bonn : Ferd. Dümmlers Verlag, 1937. (Pp. 48, with numerous figures and tables.) RM.2.—(25 per cent. reduction abroad).

Cosmic Rays. Being the Halley Lecture, delivered on June 5, 1936, by P. M. S. Blackett, F.R.S., Professor of Physics, Birkbeck College, University of London. Oxford : at the Clarendon Press ; London : Humphrey Milford, 1936. (Pp. 26, with 2 plates and 6 figures.) 2s. net.

Elements of Nuclear Physics. By Franco Rasetti, Professor of Spectroscopy, Royal University of Rome, Research Associate, Columbia University. London and Glasgow : Blackie & Son, Ltd., 1937. (Pp. xvi + 327, with 73 figures, including 8 plates.) 18s. 6d. net.

An Introduction to Nuclear Physics. By N. Feather, Ph.D., University Lecturer in Physics and Fellow of Trinity College, Cambridge. Cambridge : at the University Press, 1936. (Pp. x + 213, with 3 plates, 21 figures and 21 tables.) 10s. 6d. net.

Die Atomkerne. Grundlagen und Anwendungen ihrer Theorie. By Dr. C. F. von Weizsäcker, Berlin. Physik und Chemie und ihre Anwendungen in Einzeldarstellungen, Band II. Leipzig : Akademische Verlagsgesellschaft m.b.H., 1937. (Pp. viii + 214, with 40 figures, including 1 folding plate, and 12 tables.) RM.16.—, bound ; RM.14.40, paper covers.

Statistical Mechanics. The Theory of the Properties of Matter in Equilibrium. By R. H. Fowler, M.A., Fellow of Trinity College and Plummer Professor of Mathematical Physics in the University of Cambridge. Second edition. Cambridge : at the University Press, 1936. (Pp. x + 864, with 101 figures and 74 tables.) 50s. net.

The Philosophy of Relativity. By A. P. Ushenko, Associate Professor of Philosophy, the University of Michigan. London : George Allen & Unwin, Ltd., 1937. (Pp. 208, with 11 figures.) 8s. 6d. net.

Vibration and Sound. By Philip M. Morse, Associate Professor of Physics, Massachusetts Institute of Technology. International Series in Physics. New York and London: McGraw-Hill Publishing Co., Ltd., 1936. (Pp. xvi + 351, with 91 figures and 14 tables.) 24s. net.

Ions in Solution. By R. W. Gurney, M.A., Ph.D., Research Associate in the University of Bristol. Cambridge: at the University Press, 1936. (Pp. viii + 206, with 45 figures and 15 tables.) 10s. 6d. net.

A School Physics. Revision Notes and Questions. Hydrostatics, Heat, Light and Sound. By S. R. Humby, M.C., M.A., and F. W. Goddard, M.A., F.C.S., The Science School, The College, Winchester. London, New York, Toronto: Longmans, Green & Co., 1937. (Pp. viii + 235, with 130 figures.) 3s.

Practical Physics for Inter. B.Sc. Students. By H. M. Browning, M.Sc, Ph.D., F.Inst.P., and L. Starbuck, B.Sc., A.Inst.P. London and Glasgow: Blackie & Son, Ltd., 1936. (Pp. x + 146, with 46 figures.) 3s. 6d. net.

Thermodynamic Properties of Steam, including Data for the Liquid and Solid Phases. By Joseph H. Keenan, Associate Professor of Mechanical Engineering, and Frederick G. Keyes, Chairman of Department of Chemistry, Director of Research Laboratory of Physical Chemistry, Massachusetts Institute of Technology. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1936. (Pp. 89, with 10 figures and 11 tables.) 13s. 6d. net.

Hydraulics. By Charles W. Harris, C.E., M.Am.Soc.C.E., Professor of Hydraulic Engineering, University of Washington. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1936. (Pp. xiv + 220, with 132 figures.) 13s. 6d. net.

Press Tool Making, including Hardening and Tempering. A Practical Manual. By Ernest Perry. London: The Technical Press, Ltd., 1936. (Pp. viii + 92, with 34 figures.) 3s. 6d. net.

Electron Tubes in Industry. By Keith Henney. Second edition. New York and London: McGraw-Hill Publishing Co., Ltd., 1937. (Pp. viii + 539, with 397 figures.) 30s. net.

Commercial A.C. Measurements. By G. W. Stubbings, B.Sc., F.Inst.P., A.M.I.E.E. Second edition. London: Chapman & Hall, Ltd., 1937. (Pp. xvi + 348, with 169 figures.) 15s. net.

Averages of Temperature for the British Isles for periods ending 1935. Meteorological Office, Air Ministry, M.O. 407. London: H. M. Stationery Office, 1936. (Pp. 52.) 1s. 3d. net.

Averages of Bright Sunshine for the British Isles for periods ending 1935. Meteorological Office, Air Ministry, M.O. 408. London: H.M. Stationery Office, 1936. (Pp. 43.) 1s. net.

Introduction to Theoretical Seismology. Part I: Geodynamics. By J. B. Macelwane, S.J., Department of Geophysics, Saint Louis University. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1936. (Pp. x + 366, with 80 figures, 40 tables and chart in pocket.) 30s. net.

Die Chromatographische Adsorptionsmethode. Grundlagen, Methodik, Anwendungen. Von Dr. L. Zechmeister, Professor, und Dr. L. v. Cholnoky, Privatdozent, am Chemischen Institut der Universität Pécs (Ungarn). Vienna : Verlag von Julius Springer, 1937. (Pp. xii + 231, with 45 figures and 18 tables.) RM.14.40.

Gmelins Handbuch der anorganischen Chemie. 8. Auflage. Herausgegeben von der Deutschen Chemischen Gesellschaft. System-Nummer 23 : Ammonium, Lieferung 2. Berlin : Verlag Chemie, G.m.b.H., 1936. (Pp. xviii + 376, with 42 figures.) RM.44.25.

Gmelins Handbuch der anorganischen Chemie. 8. Auflage. Herausgegeben von der Deutschen Chemischen Gesellschaft. System-Nummer 36 : Gallium. Berlin : Verlag Chemie, G.m.b.H., 1936. (Pp. xviii + 104, with 8 figures.) RM.13.87.

Gmelins Handbuch der anorganischen Chemie. 8. Auflage. Herausgegeben von der Deutschen Chemischen Gesellschaft. System-Nummer 37 : Indium. Berlin : Verlag Chemie, G.m.b.H., 1936. (Pp. xviii + 120, with 8 figures.) RM.15.75.

The Theory of Metals. Based on an Essay awarded the Adams Prize in the University of Cambridge, 1931–32. By A. H. Wilson, M.A., Fellow of Trinity College, Cambridge. Cambridge : at the University Press, 1936. (Pp. viii + 272, with 31 figures.) 18s. net.

Applied Chemistry for Engineers. By A. F. H. Ward, M.A., Ph.D., B.Sc., A.I.C., Assistant Lecturer in Physical Chemistry in the College of Technology and the University, Manchester. London, New York, Toronto : Longmans, Green & Co., 1936. (Pp. xii + 127, with 6 figures and 4 tables.) 5s.

Elements of Chemical Engineering. By Walter L. Badger, Professor of Chemical Engineering, and Warren L. McCabe, Associate Professor of Chemical Engineering, University of Michigan. With an Introduction by Arthur D. Little. Second edition. Chemical Engineering Series. New York and London : McGraw-Hill Publishing Co., Ltd., 1936. (Pp. xviii + 660, with 311 figures.) 30s. net.

Cyanidation and Concentration of Gold and Silver Ores. By John V. N. Dorr, E.M., D.Sc. New York and London : McGraw-Hill Publishing Co., Ltd., 1936. (Pp. x + 485, with frontispiece and 135 figures.) 30s. net.

British Chemicals and their Manufacturers. The Official Directory of the Association of British Chemical Manufacturers. Second edition. London : The Association of British Chemical Manufacturers, 1937. (Pp. 466.) Free to buyers of chemicals on application to the publishers.

Intermediate Chemistry. By T. M. Lowry, C.B.E., M.A., D.Sc., F.R.S., Professor of Physical Chemistry in the University of Cambridge, and A. C. Cavell, B.A., B.Sc., Senior Science Master at Uppingham School. London : Macmillan & Co., Ltd., 1936. (Pp. xvi + 876, with 188 figures.) 12s. 6d.

Lecture Experiments in Chemistry. By G. Fowles, M.Sc., A.I.C., F.C.S. London : G. Bell & Sons, Ltd., 1937. (Pp. xvi + 564, with 150 figures.) 16s. net.

The New Chemistry. By E. N. da C. Andrade, D.Sc., Ph.D., F.R.S., Quain Professor of Physics, University of London. London : G. Bell & Sons, Ltd., 1936. (Pp. 58, with 8 plates and 5 figures.) 3s. 6d. net.

Modern Elementary Chemistry. By F. Sherwood Taylor, Ph.D., M.A., B.Sc., Assistant Lecturer, Queen Mary College, University of London. London : William Heinemann, Ltd., 1936. (Pp. xii + 461, with 8 plates and 118 figures.) 5s.

Handbook to Modern Elementary Chemistry. By F. Sherwood Taylor, Ph.D., M.A., B.Sc., Assistant Lecturer, Queen Mary College, University of London. London : William Heinemann, Ltd., 1936. (Pp. vi + 137, with 3 figures.) 6s. net.

Inorganic Preparations. A Systematic Course of Experiments. By Alexander King, M.Sc., A.R.C.S., D.I.C., of the Chemistry Department, Imperial College of Science and Technology. London : Thomas Murby & Co. ; New York : D. van Nostrand Co., 1936. (Pp. xii + 164, with 22 figures.) 5s. 6d. net.

Nota Bene. Anatomy of Chemistry. By S. B. Davis, M.Sc., A.I.C., Senior Chemistry Master, Highbury County School. London, New York, Toronto : Longmans, Green & Co., 1937. (Pp. 16.) 6d.

Chemical Calculations. By Clifford M. Jones, M.Sc., Senior Chemistry Master, Wheelwright Grammar School, Dewsbury. London : John Murray, 1937. (Pp. 95.) 1s. 9d.

Chemical Arithmetic (School Certificate). By F. W. Goddard, M.A., F.C.S., Senior Science Master, The College, Winchester. London, New York, Toronto : Longmans, Green & Co., 1937. (Pp. viii + 99.) 1s. 6d.

Disperse Systems in Gases ; Dust, Smoke and Fog. A General Discussion. Reprinted from the " Transactions of the Faraday Society." London and Edinburgh : Gurney & Jackson, for the Society, 1936. (Pp. 1041–1300, with 4 plates and numerous figures.) 12s. 6d. net.

Organic Chemistry. The Chemistry of the Compounds of Carbon. By Lucius Junius Desha, Professor of Chemistry, Washington & Lee University. International Chemical Series. New York and London : McGraw-Hill Publishing Co., Ltd., 1936. (Pp. xvi + 750, with 53 figures and 43 tables.) 21s. net.

The Aromatic Diazo-Compounds and their Technical Applications. By K. H. Saunders, M.C., M.A., B.Sc., F.I.C., Chemist, Imperial Chemical Industries, Ltd. With a Foreword by Professor A. G. Green, F.R.S., M.Sc., F.I.C. London : Edward Arnold & Co., 1936. (Pp. xii + 224, with 22 tables.) 12s. 6d. net.

An Introduction to the Preparation and Identification of Organic Compounds. By Robert D. Coghill, Assistant Professor of Chemistry, and Julian M. Sturtevant, Instructor in Chemistry, Yale University. International Chemical Series. New York and London : McGraw-Hill Publishing Co., Ltd., 1936. (Pp. xiv + 226, with 24 figures.) 10s. 6d. net.

Laboratory Methods of Organic Chemistry. By L. Gattermann. Completely revised by Heinrich Wieland. Translated from the twenty-fourth German edition by W. McCartney, Ph.D., A.I.C., late Assistant

in the Department of Medical Chemistry, University of Edinburgh. London: Macmillan & Co., Ltd., 1937. (Pp. xvi + 435, with 59 figures.) 18s. net.

Practical Organic Chemistry. By Frederick George Mann, Sc.D., D.Sc., F.I.C., Fellow and Lecturer, Trinity College, Cambridge, University Lecturer in Chemistry, and Bernard Charles Saunders, M.A., Ph.D., B.Sc., Charles Kingsley Lecturer, Magdalene College, Cambridge, University Demonstrator in Chemistry. With a Foreword by Sir William J. Pope, K.B.E., F.R.S. London, New York, Toronto: Longmans, Green & Co., 1936. (Pp. xiv + 403, with 66 figures.) 8s. 6d. net.

An Introduction to the Scientific Study of the Soil. By Norman M. Comber, D.Sc., A.R.C.S., F.I.C., Professor of Agricultural Chemistry and Head of the Department of Agriculture in the University of Leeds. Third edition. London: Edward Arnold & Co., 1936. (Pp. viii + 206, with 25 figures and 14 tables.) 7s. 6d. net.

The Study of the Soil in the Field. By G. R. Clarke, B.Sc., M.A., Lecturer in Soil Science in the Department of Rural Economy, University of Oxford. Published under the auspices of the Imperial Forestry Institute, University of Oxford. Oxford: at the Clarendon Press; London: Humphrey Milford, 1936. (Pp. 142, with 7 figures and 3 tables.) 5s. net.

The Cacao and Forest Soils of Trinidad: (B) South-Central District. By F. Hardy, D. W. Duthie and G. Rodriguez (Analyst), Imperial College of Tropical Agriculture, Trinidad. With the Assistance of Members of the Staff of the Forest Department of Trinidad and Tobago. Studies in West Indian Soils, No. X. Port-of-Spain: Government Printing Office, 1936. (Pp. 56, with 2 maps and 8 tables.) 2s.

A Practical Course in Agricultural Chemistry. For Senior Students of Agriculture, Dairying, Horticulture and Poultry Husbandry. By Frank Knowles, F.I.C., Head of the Chemistry Department and Lecturer and Adviser in Agricultural Chemistry, and J. Elphin Watkin, B.Sc., Ph.D., A.I.C., Lecturer in Agricultural Chemistry, East Anglian Institute of Agriculture, Chelmsford. With a Foreword by Sir John Russell, D.Sc., F.R.S. London: Macmillan & Co., Ltd., 1937. (Pp. x + 188, with 21 figures.) 10s. net.

Soil Erosion and its Control. By Quincy Claude Ayres, C.E., Associate Professor, in charge of Drainage and Conservation Engineering, Agricultural Engineering Department, Iowa State College. McGraw-Hill Publications in Agricultural Engineering. New York and London: McGraw-Hill Publishing Co., Ltd., 1936. (Pp. xii + 365, with 244 figures and 35 tables.) 21s. net.

Our Natural Resources and Their Conservation. Edited by A. E. Parkins, George Peabody College for Teachers, and J. R. Whitaker, University of Wisconsin. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1936. (Pp. xii + 650, with 130 figures and 42 tables.) 25s. net.

Engineering Geology. By H. Ries, Ph.D., Professor of Geology in Cornell University, and Thomas L. Watson, Ph.D., late Professor of Geology in the University of Virginia and State Geologist of Virginia. Fifth

edition. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1936. (Pp. viii + 750, with 88 plates and 271 figures.) 25*s.* net.

The Quaternary Ice Age. By W. B. Wright of the Geological Survey of Great Britain. Second edition. London : Macmillan & Co., Ltd., 1937. (Pp. xxvi + 478, with 23 plates and 155 figures.) 25*s.* net.

British Trees and Shrubs, including those Commonly Planted. A Systematic Introduction to our Conifers and Woody Dicotyledons. By H. Gilbert-Carter, M.A., M.B., Ch.B., Director of the Cambridge University Botanic Garden and University Lecturer in Botany. Oxford : at the Clarendon Press ; London : Humphrey Milford, 1936. (Pp. xvi + 291.) 12*s.* 6*d.* net.

Methods in Plant Physiology. A Laboratory Manual and Research Handbook. By Walter E. Loomis, Ph.D., Associate Professor of Plant Physiology and Research Associate, Iowa State College, and Charles A. Shull, Ph.D., Professor of Plant Physiology, University of Chicago. With a chapter on Statistical Methods by George W. Snedecor, M.S., Professor of Mathematics and Director of the Statistical Laboratory, Iowa State College. New York and London : McGraw-Hill Publishing Co., Ltd., 1937. (Pp. xviii + 472, with 94 figures and 45 tables.) 25*s.* net.

Biology in the School. By H. Alan Peacock, M.Sc., Head of the Biology Department, Cheltenham Grammar School. London : William Heinemann, Ltd., 1937. (Pp. xvi + 354.) 10*s.* 6*d.* net.

Biological Time. By Lecomte du Noüy. London : Methuen & Co., Ltd., 1936. (Pp. x + 180, with 31 figures.) 7*s.* 6*d.* net.

Recent Advances in Cytology. By C. D. Darlington, D.Sc., Ph.D., Head of the Cytological Department, John Innes Horticultural Institution, London. With a Foreword by Professor J. B. S. Haldane, M.A., F.R.S. Second edition. London : J. & A. Churchill, Ltd., 1937. (Pp. xvi + 671, with 16 plates, 160 figures and 81 tables.) 21*s.* net.

An Account of the Fishing Gear of England and Wales. By F. M. Davis, B.A., Fisheries Laboratory, Lowestoft. Third edition. Ministry of Agriculture and Fisheries, Fishery Investigations, Series II, Vol. XV, No. 2, 1936. London : H.M. Stationery Office, 1937. (Pp. 139, with 98 figures.) 6*s.* net.

The Haddock Stocks of the North-East Atlantic, 1916–1935. By D. S. Raitt, B.Sc., Ph.D., F.R.S.E., F.L.S. Fishery Board for Scotland, Scientific Investigations, 1936, No. I. Edinburgh : H.M. Stationery Office, 1936. (Pp. 32, with 7 figures and 17 tables.) 1*s.* 6*d.* net.

Calanus finmarchicus and other Copepods in Scottish Waters in 1933. By S. G. Gibbons, Ph.D., B.Sc., F.R.S.E. Fishery Board for Scotland, Scientific Investigations, 1936, No. II. Edinburgh : H.M. Stationery Office, 1936. (Pp. 37, with 3 figures and 6 charts.) 2*s.* net.

Statistical Methods in Biology, Medicine and Psychology. By C. B. Davenport, Carnegie Institution of Washington, Cold Spring Harbor, N.Y., and Merle P. Ekas, Northeast High School, Philadelphia, Pa. Fourth edition. New York : John Wiley & Sons, Inc. ; London : Chapman & Hall, Ltd., 1936. (Pp. xii + 216, with 26 figures and 37 tables.) 13*s.* 6*d.* net.

An Introduction to Comparative Biochemistry. By Ernest Baldwin, B.A., Ph.D., Fellow of St. John's College, Cambridge, University Demonstrator in Biochemistry, Cambridge. With a Foreword by Professor Sir Frederick Gowland Hopkins, F.R.S. Cambridge : at the University Press, 1937. (Pp. xviii + 112, with 11 figures and 9 tables.) 5s. net.

A Textbook of Applied Biochemistry for Pharmacists and Pharmaceutical Students. By Frank Wokes, B.Sc., Ph.C., F.I.C., Member of the Staff of the Pharmacological Laboratories, College of the Pharmaceutical Society of Great Britain. London : Baillière, Tindall & Cox, 1937. (Pp. x + 522, with 79 figures, including 3 plates, and 30 tables.) 15s. net.

Modern Discoveries in Medical Psychology. By Clifford Allen, M.D., M.R.C.P., D.P.M., Psychotherapist to the Institute of Medical Psychology, Chief Clinical Assistant to the Psychiatric Department of Charing Cross Hospital. London : Macmillan & Co., Ltd., 1937. (Pp. x + 280.) 8s. 6d. net.

The Development of Modern Medicine. An Interpretation of the Social and Scientific Factors Involved. By Richard Harrison Shryock, Professor of History, Duke University. Philadelphia : University of Pennsylvania Press ; London : Humphrey Milford, 1936. (Pp. xvi + 442, with 8 plates and 10 charts.) 18s. net.

Accidents and Their Prevention. By H. M. Vernon, M.A., M.D., Member of the Technical Advisory Board, National Institute of Industrial Psychology. Cambridge : at the University Press, 1936. (Pp. x + 336, with 61 figures.) 15s. net.

Recollections and Reflections. By Sir J. J. Thomson, O.M., D.Sc., F.R.S., Master of Trinity College, Cambridge. London : G. Bell & Sons, Ltd., 1936. (Pp. viii + 451, with 10 plates.) 18s. net.

Albert Einstein. A Bibliography of his Scientific Papers, 1901–1930. Compiled by E. Weil, Ph.D. London : E. P. Goldschmidt & Co., Ltd., 45 Old Bond Street, W.1, 1937. (Pp. 24.) 6s.

Matthew Boulton. By H. W. Dickinson. Cambridge : at the University Press, 1937. (Pp. xiv + 218, with 15 plates and 6 figures.) 10s. 6d. net.

The Preservation of our Scenery. Essays and Addresses. By Vaughan Cornish, D.Sc. With illustrations by the Author. Cambridge : at the University Press, 1937. (Pp. xiv + 91, with 12 illustrations.) 7s. 6d. net.

Environment and Nation. Geographical Factors in the Cultural and Political History of Europe. By Griffith Taylor, D.Sc., B.E. (Mining), B.A. (Research, Cambridge), Professor of Geography, The University of Toronto. Toronto : The University of Toronto Press ; London : Humphrey Milford, 1936. (Pp. 571, with frontispiece, 147 figures and 32 tables.) 17s. net.

Naven. A Survey of the Problems suggested by a Composite Picture of the Culture of a New Guinea Tribe drawn from Three Points of View. By Gregory Bateson, M.A., Fellow of St. John's College, Cambridge. Cambridge : at the University Press, 1936. (Pp. x + 286, with 28 plates and 6 figures.) 18s. net.

Back in the Stone Age. The Natives of Central Australia. By Charles Chewings, Ph.D., F.G.S., F.R.G.S., A.M.I.M.E. Sydney : Angus & Robertson, Ltd., 1936. (Pp. xxii + 161, with 23 plates.) 7s. 6d.

The Wandering Spirit. A Study of Human Migration. By Ragnar Numelin, Ph.D. With a Foreword by Dr. Edward Westermarck. London : Macmillan & Co., Ltd., 1937. (Pp. xvi + 375.) 20s. net.

The Silver World. An Essay on the Ultimate Problems of Philosophy. By Aegidius Jahn, Ph.D. London : George Allen & Unwin, Ltd., 1937. (Pp. 392.) 12s. 6d. net.

The Consequences of Philosophy. A Reply to Planck and Einstein. By Max Mundlak. London : John Bale, Sons & Danielsson, Ltd., 1936. (Pp. viii + 86.) 3s. 6d. net.

A Creed for Sceptics. By C. A. Strong, LL.D., formerly Professor of Psychology in Columbia University. London : Macmillan & Co., Ltd., 1936. (Pp. x + 98.) 6s. net.

The Phenomena of Life. A Radio-Electric Interpretation. By George Crile. Edited by Amy Rowland. London : William Heinemann, Ltd., 1936. (Pp. 379, with 113 figures, including 23 plates, and 22 tables.) 15s. net.

The World of Science. By F. Sherwood Taylor, Ph.D., M.A., B.Sc. London : William Heinemann, Ltd., 1936. (Pp. xvi + 1064, with 48 plates and 533 figures.) 8s. 6d. net.

The March of Science. A First Quinquennial Review, 1931–1935. By various Authors. Issued under the Authority of the Council of the British Association for the Advancement of Science. London : Sir Isaac Pitman & Sons, Ltd., 1937. (Pp. viii + 215.) 3s. 6d. net.

Science Fights Death. By D. Stark Murray, B.Sc., M.B., Ch.B. Changing World Library, No. 4. London : C. A. Watts & Co., Ltd., 1936. (Pp. x + 149, with 18 figures.) 2s. 6d. net.

An Experimental Study of the Frustration of Will-Acts and Conation. By Howard L. Philp, B.A., Ph.D. The British Journal of Psychology Monograph Supplements. Cambridge : at the University Press, 1936. (Pp. xii + 103.) 8s. 6d. net.

Creative Education and the Future. By Olive A. Wheeler, D.Sc., Professor of Education, University College, Cardiff. London : University of London Press, Ltd., 1936. (Pp. xii + 365, with 8 figures and 3 tables.) 8s. 6d. net.

The Teaching of General Science. Science Masters' Association. Interim Report of the Sub-Committee appointed in 1935, adopted by the General Committee in 1936. London : John Murray, 1936. (Pp. vi + 49.) 2s. 6d. net.

Books & Documents. Dating, Permanence and Preservation. By Julius Grant, Ph.D., M.Sc., F.I.C. London : Grafton & Co., 1937. (Pp. xii + 218, with 36 figures, including 12 plates.) 10s. 6d. net.

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GENERAL INDEX

INDEX OF REVIEWS